Supplemental Information for *Misdemeanor Disenfranchisement*

October 2018

Contents

1 Estimates from Main Paper

1 Regression Table from Figure 2

Table A1: IV estimates: Jail sentences on voting, by race

	Dependent variable:	
	Voted 2012	
	Black Defendants	White Defendants
	(1)	(2)
jail	-0.136^{**}	-0.006
	(0.060)	(0.049)
Constant	0.263^{***}	0.091^{***}
	(0.036)	(0.029)
Year dummies	Yes	Yes
Clustered SE's	Courtroom	Courtroom
First Stage F-Statistic	52.81	64.63
Observations	31,507	77,750
Adjusted R^2	0.034	0.003
Note:		*p<0.1; **p<0.05; ***p<0.01

2 Placebo test

To see whether my IV setup tends to yield spurious results, I run a placebo test. I re-run my main analysis for defendants with cases filed from November 2012-October 2014. The outcome variable is still voter turnout in the 2012 election, so I should find no effect of post-election cases on election turnout. If I found an "effect", that would throw the main paper results into question.

The naive OLS regression of 2012 voting on post-election jail sentences yields a large negative estimate, underscoring the bias of OLS in this setting (table available on request). People who voted in the 2012 election are apparently more successful at interacting with the court system, and this unobserved difference in defendants yields a spurious estimated "effect" of post-election sentencing on pre-arrest voting.

In contrast, I do not find any statistically or substantively significant effects of post-election cases on voter turnout in my IV analyses of all defendants. These estimates appear in Table A2. The first-stage F-statistics suggest that the instrument is strong enough to be used, despite there being fewer available post-election observations than I used in my main analysis. The point estimates are small and vary in direction between the overall sample and the racial subsets. These null results are reassuring: they provide one piece of evidence that my main analytical approach is not producing spurious results. In addition, Table A37 below (in Section 7.2) extends this placebo test to include cases filed through 2016, and also finds null effects as expected. Section 7.1 below presents an alternative placebo test focused on 2008 voting, and discusses some reasons it may perform differently than this one.

Table A2: Placebo IV estimates: Jail on pre-arrest voting

	Dependent variable:		
	vote2012		
	All Defendants	Black Defendants	White Defendants
	(1)	(2)	(3)
Jail	−0.031	0.034	−0.007
	(0.052)	(0.092)	(0.048)
Constant	0.139*	0.171*	0.100*
	(0.029)	(0.050)	(0.028)
Year dummies	Yes	Yes	Yes
First Stage F-Statistic	512.4	124.93	398.6
Observations	48,575	14,041	32,444
Adjusted R^2	0.008	−0.014	0.002
Note:			*p<0.05

3 Homeownership and other economic indicators

In this section, I merge the main dataset to a dataset from the Harris County Appraisal District to identify defendants who owned homes in Harris County as of 2008.[1] I identify matches as follows: first, I check that the defendant's first and last names appear in the full homeowner name field of the appraisal data, that the zip code of the property address matches the zip code of the address on record for the defendant, and that the street addresses share the same house number. Then, I narrow down these possible matches using string distance between the street names of the assessed property and the defendant's recorded address. I use the jaro-winkler metric, retaining matches with string distances below .45. This fuzzy match allows for some minor differences in the transcription of street names ("Street" versus "St.", minor misspellings, omissions of modifiers like "North," etc.). However, this overall approach is fairly conservative, as it requires an exact match on the defendant's first and last names and their house number. It is possible that some defendants own houses but were not detected by this approach.

Using this method, I identify nearly 6000 defendants who own homes. In the first table below, I present separate IV estimates of jail's effect on voting for this subset of homeowners, as well as for the remainder of the sample with Harris County addresses recorded but no match to the appraisal database. These results should be viewed with some caution as they are run on a much smaller sample than other analyses.[2] Still, they suggest that homeowners may show a much larger effect of jail on voting than the main sample.[3] I interpret this as evidence that something more than economic disruption could be at play: homeowners are probably less likely than the rest of the sample to suffer immediate and catastrophic economic consequences such as homelessness from a short jail sentence. The fact that they still show such a large effect suggests that the political socialization mechanism described by Weaver & Lerman (2012, 2014) may be operating here as well. However, in addition to the imprecision of these estimates, they merit one more note of caution in that apparent non-homeowners have low enough prior voter turnout (10% in 2008) that they may be showing some sort of "floor effect" on demobilization. That is, it is possible that turnout among this group is already so low that there is not much room for further demobilization.

[1] I downloaded the full set of homeowner names and addresses from http://pdata.hcad.org/ in June 2016.

[2] The first-stage F-statistic of 9.8 is just within the range of concern raised by Stock, Wright & Yogo (2002), so we should worry about weak instruments.

[3] Running the IV analysis on the full dataset and including an interaction between jail sentencing and homeownership yields similar results; the difference between homeowners and non-homeowners in the effect of jail is statistically significant at $p < .05$.

Table A3: IV estimates: Jail sentences on voting, by homeownership

	Dependent variable:			
	Voted 2012			
	Homeowners	Homeowners	Others	Others
	(1)	(2)	(3)	(4)
Jail	−0.254	−0.329**	−0.044	−0.040
	(0.167)	(0.162)	(0.037)	(0.036)
Male		0.005		−0.058***
		(0.027)		(0.007)
Charge Severity		0.067***		0.014***
		(0.020)		(0.003)
Age at Filing		0.00002***		0.00001***
		(0.00000)		(0.00000)
Black		0.187***		0.122***
		(0.018)		(0.002)
Constant	0.342***	−0.355***	0.134***	−0.039**
	(0.077)	(0.089)	(0.022)	(0.017)
Year dummies	Yes	Yes	Yes	Yes
Observations	5,860	5,850	88,787	88,688
Adjusted R^2	−0.026	0.024	0.017	0.065
Note:		*p<0.1; **p<0.05; ***p<0.01		

An alternative approach to exploring economic heterogeneity in these effects would involve merging local Census data (such as poverty rates) into the dataset and seeing whether the effects look different among people living in different types of neighborhoods. This approach has the benefit of not assuming that homeownership is the only relevant economic characteristic, but the drawback of relying on aggregate data as a proxy for individual characteristics. Nonetheless, Table A4 reports the effects of jail on voting (main IV specification) among Black voters that live in neighborhoods (census tracts) with poverty rates above and below the median rate in the dataset. Beyond the ecological concerns of using aggregate data, these results come with an additional caution: the process of merging in census data involved geocoding defendant addresses in order to map them into census tracts, and about one-third of these addresses could not be reliably geocoded to locations in Harris County. Thus, the results shown here are based on a smaller and less complete sample than most other analyses in this SI.

These results suggest a somewhat different conclusion from the homeownership analysis above. Whereas the homeownership analyses showed larger demobilization effects among homeowners (suggesting that economic/resource mechanisms were unlikely to explain the whole effect, and that political socialization may be at work), these estimates show a much larger demobilization effect among people living in high-poverty areas compared to those in lower-poverty neighborhoods. These results, though noisy and based on a limited sample, suggest that the effects of jail on voting may be concentrated among lower-income people (or at least people living in high-poverty areas). That said, it is important not to over-interpret these differences; though the estimates in columns 1 and 2 of the table are substantively quite different, I cannot statistically distinguish them from each other.

Table A4: IV estimates: Jail sentences on voting (Black defendants)

	Dependent variable:	
	vote2012	
	Below-median Poverty	Above-median Poverty
	(1)	(2)
Jail	-0.126	-0.242^{**}
	(0.089)	(0.097)
Constant	0.267^{***}	0.341^{***}
	(0.048)	(0.056)
Year dummies	Yes	Yes
Observations	9,335	9,315
Adjusted R^2	0.030	0.017
Note:		*p<0.1; **p<0.05; ***p<0.01

4 Other Outcomes: Additional Jail, Felony Convictions

One possible mechanism discussed in the main paper is rearrest: maybe people who get jail in their first case also become more likely to get sent back to jail and to be incarcerated during the election, or to end up with a felony conviction that renders them ineligible to vote in the next election. Here I look into this possibility by using the same IV setup as presented in the main paper, but with two different outcome measures: future jail sentences, and future felony convictions (occurring before the 2012 election). Table A5 below presents IV estimates of the effect of a jail sentence (in the first case) on defendants' future outcomes: do they become more likely to be rearrested and sentenced to more jail, or to end up with a felony conviction if they get sent to jail in their first case? It appears not, likely because these are quite rare outcomes in this sample. For example, fewer than one in ten of the defendants end up with a felony conviction by the next election. This does not appear to be the mechanism by which jail sentencing reduces future voting.

A reviewer also asked whether the main results presented in the paper persisted when dropping anyone who subsequently was convicted of a felony or sentenced to another jail sentence. Table A6 reproduces the main paper's analysis of Black defendants, this time omitting everyone who was convicted of a felony or sentenced to jail again by Election Day 2012, and finds very similar results to the main analysis, though it is worth approaching this analysis with caution (since these other outcomes are post-treatment variables).

Table A5: IV estimates: Jail sentence on new jail sentence/felony conviction, Black defendants

	Dependent variable:	
	More Jail	Felony Conviction
	(1)	(2)
Jail	0.015	0.001
	(0.130)	(0.078)
Constant	0.574***	0.261***
	(0.072)	(0.043)
Year dummies	Yes	Yes
Observations	31,507	31,507
Adjusted R^2	0.036	0.020
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table A6: Main IV estimates, dropping people with new jail sentence/felony conviction

	Dependent variable:	
	Voted2012	
	All Defendants	Black Defendants
	(1)	(2)
Jail	-0.055	-0.143^{**}
	(0.036)	(0.062)
Constant	0.157^{***}	0.289^{***}
	(0.019)	(0.030)
Year dummies	Yes	Yes
Observations	96,986	24,806
Adjusted R^2	0.019	0.029
Note:		*p<0.1; **p<0.05; ***p<0.01

5 Another OLS table; More on selection bias

Table 2 in the main paper reports results from a simple regression of 2012 voting onto an indicator for whether a person was sentenced to jail, as well as a few available covariates (indicators for whether the person is male/Black, and a continuous measure of their birth year). Here, I supplement that table with geographic (zip code) information about defendants, as well as past turnout (in the 2008 election).

Columns 4 and 5 present specifications from the original table in the paper. Column 3 adds in an indicator for whether the person voted in 2008 (pre-arrest), while Column 2 adds in zip code fixed effects, and Column 1 includes both.

Much as in Gerber et al. (2017)'s analysis of selection bias in the estimates of prison's effect on voting, including past voting and (to a lesser extent) geographic information dramatically attenuates the "effect" estimates. But unlike in that paper, the estimates here remain negative, statistically significant, and actually fairly close in size to the LATE generated by the main IV estimation approach of the paper. Here, I speculate about several possible reasons for these differences.

One key difference is the population being examined; Gerber et al. focus on people who have been convicted of felony crimes and face state prison (or in a supplemental analysis, county jail), not people being sent to jail over more minor misdemeanor crimes. This is an important distinction; given the frequency and arbitrariness of misdemeanor arrests, there are many reasons to expect the population of people facing misdemeanor charges to be more likely to vote at baseline, and so more able to be demobilized by jail time. This is borne out by a comparison of voting rates: Gerber et al. (Table 4) report 2012 turnout rates on the order of 16 percent among 2008 Pennsylvania registrants who were convicted of a felony, which constitutes little change from their pre-arrest rates in 2008. This is a strikingly low rate of turnout among registrants, and the overall rate of turnout would be much lower if examining the full population (unconditional on registration). Conversely, the 2008 turnout rate *among registrants* who would ultimately face misdemeanor charges in Harris County was approximately 30 percent, substantially higher.[4] This is despite the state of Texas having general-population turnout rates that are usually substantially lower than Pennsylvania's.

The difference in these populations stretches beyond their baseline turnout rates; they also face different treatments, and different levels of novelty in those treatments. The people in Gerber et al.'s analysis have been convicted of felony crimes, more serious than misdemeanors. This means that, for one thing, a comparison of people who get sent to county jail (after a felony conviction) to those with non-custodial sentences may be essentially comparing people who "got lucky" (they received a sentence short enough that they did not get sent to state prison). By comparison, jail is the harshest custodial sentence available in the misdemeanor cases examined here; people who get sent to jail have been treated as harshly by the state as they could have been, and for a fairly minor

[4] As noted in the paper, 2008 turnout records in this dataset are post-treatment and incomplete. This makes me reluctant to include them in the main analysis, but for the purpose of comparing pre-arrest turnout rates to other published work, they provide a conservative estimate of past turnout.

criminal offense. This seems like a qualitatively different experience of government, and could well shape people's reactions. This is especially true given the novelty of these experiences. Gerber et al. (pg. 26) note that most first-time prison inmates—over three-quarters—had been arrested in the past, and many of them had been previously convicted of other offenses. By comparison, the analysis presented here focuses on people facing criminal charges in Harris County for the first time; some of them may have been arrested elsewhere, but most of them are experiencing their first serious interaction with the criminal legal system. This is a population that seems more likely to be deterred from voting by jail than people that have already had a number of these experiences.

Table A7: OLS estimates of jail's effect on voting

	Dependent variable:				
	Voted 2012				
	(1)	(2)	(3)	(4)	(5)
Jail	−0.049*	−0.083*	−0.055*	−0.097*	−0.105*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Voter Birth Year	−0.002*	−0.005*	−0.002*	−0.005*	
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	
Black	0.073*	0.124*	0.067*	0.115*	
	(0.002)	(0.003)	(0.002)	(0.002)	
Male	−0.028*	−0.043*	−0.028*	−0.043*	
	(0.002)	(0.002)	(0.002)	(0.002)	
Voted 2008	0.510*		0.519*		
	(0.003)		(0.003)		
Constant	3.312*	9.877*	3.081*	9.466*	0.183*
	(0.174)	(0.192)	(0.157)	(0.175)	(0.001)
Zip Code Fixed Effects	Yes	Yes	No	No	No
Observations	104,298	104,298	113,237	113,237	113,367
R^2	0.302	0.101	0.289	0.072	0.025
Adjusted R^2	0.281	0.074	0.289	0.072	0.025
Note:					*p<0.05

6 Another Approach: Case Timing

This section presents an entirely different identification strategy, a simple cut at case timing that I ran to convince myself that the IV estimates were not a fluke. The basic intuition here is similar to that of a test run by Weaver and Lerman (2010), which compares people who have been convicted of crimes before and after a given election. This holds constant (with some assumptions about time-varying confounders and selection) unobservable defendant characteristics: theoretically, people convicted shortly before an election shouldn't be more or less "criminal" or "socially connected" than people convicted after. I think this kind of design is especially credible in the case of local misdemeanor courts, where each case is extremely minor and judges are evaluated based on their ability to clear cases, not their "toughness on crime", so they are unlikely to act particularly strategically around election periods. This isn't a perfect design by any means, but it provides a nice check on the LATE provided by the IV setup in the main paper. For further tests demonstrating that people sentenced before and after the election don't differ on observable characteristics, see my paper using this design to explore the effects of criminal cases on defendants' household members (White, Forthcoming).

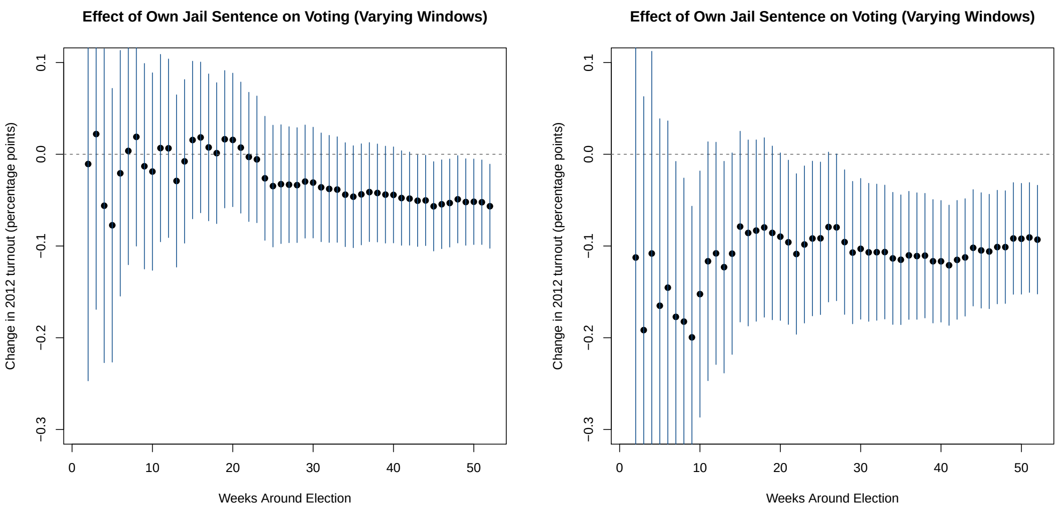
In this section, I use cases from the months before and after the 2012 election, faced by registered voters (as of mid-2012). I then simply compare the voter turnout of people who were convicted of misdemeanor charges and sent to jail before the 2012 election to the turnout of people who hadn't been arrested as of the election, but would later be convicted and jailed. I present results separately for White and Black defendants, to facilitate comparison to the main estimates of the paper.

Figure A1 presents results from a series of analyses. I look at various windows around the election, to ensure that my choice of bandwidth doesn't drive the results I find. For example, the point estimate for "10 weeks around the election" includes data—all misdemeanor cases that resulted in a jail sentence—from the ten weeks preceding and the ten weeks following the 2012 election. For each point estimate, I regress 2012 voter turnout onto an indicator variable for whether the defendant was jailed before the election or not. This yields an estimate of the effect of being arrested, convicted, and jailed before the election (compared to being jailed afterwards).

Regardless of the time window used, the estimates are broadly similar to the IV estimates presented in the paper. White defendants do not show large demobilization effects from jail sentences before the election (even when their sentence falls shortly before the election). Black defendants, in contrast, show substantial demobilization effects (on the order of 10 percentage points), whether looking only at the few weeks around the election or the full year around. These are substantively similar to the results presented in the main paper, despite coming from an entirely different design; this bolsters my confidence in the main results.

However, I should note that these are not entirely comparable estimates. Not only does this approach use all registered voters (as opposed to focusing on compliers in the IV setup and ignoring registration) and use data from a different time frame (because it relies in before/after cases being comparable, I'm reluctant to look much more than a year out from the election), it also estimates the effect of a slightly different treatment. Rather than estimating the marginal effect of a jail sentence on people that have already been arrested, it estimates the effect of a bundled treatment: being arrested, convicted, and jailed, all either before or after an election. This makes the results not perfectly comparable, but I think they are still a useful check on the main paper's results.

Figure A1: Effects of jail on voting, using a case-timing approach (White defendants on left, Black defendants on right)



2 Record Linkage Details

1 Benchmarking the Nationbuilder Voter File

In this section, I check turnout numbers from the Texas voter file used in the paper (acquired by Nationbuilder in mid-2014) against the Texas Secretary of State's reported registration and turnout totals.[5]

Nationbuilder acquired this copy of the voter file mid-2014, between the primary and general elections. It contains 13767912 registered voters, midway between the SOS' reported May registration total of 13601324 and the November registration total of 14025441.

Table A8 compares the SOS reported vote totals in presidential years to those from the voter file. As discussed in the main paper, turnout looks quite complete for 2012, but drops off in prior elections because of voter list maintenance.

Table A8

	SOS	Voter File	Difference	Pct. Diff
2012	7,993,851	7,782,542	211,309	0.026
2008	8,077,795	7,301,750	776,045	0.096
2004	7,410,765	5,665,648	1,745,117	0.235

[5] All SOS numbers are from here: http://www.sos.state.tx.us/elections/historical/70-92.shtml.

2 Sensitivity to Match Quality

I discuss my approach to merging the defendant data with the Texas voter file in the main paper. The permutation tests I run demonstrate that I do not have a high rate of false positives (finding matches where there is not a true match). But what if I have a high rate of false negatives, deciding there is not a match when in fact there is one? In this section, I perform an exercise to see how sensitive these findings are to the addition of false negatives. I take the matches I have and randomly discard some of them, such that some people who do appear in the voter file are listed as not having been registered or voted in 2012 (regardless of their actual 2012 turnout). This should give me a sense of how missed matches would attenuate the results I find.

Figure A2 presents the results of this procedure. I discard between 1% and 50% of the matches in the dataset, choosing matches at random to delete. I do this ten times for each percentage (1-50), and then perform the main IV analyses presented in the paper on the resulting dataset (jail's effect on voting, both for all defendants and for black and white defendants separately). I then plot the resulting point estimates and their 95% confidence intervals. As expected, dropping more matches shrinks the effect estimates and makes them more uncertain. This suggests that if I am missing some true matches in my main dataset, the effects I find should be conservative estimates of the true value of the effect.

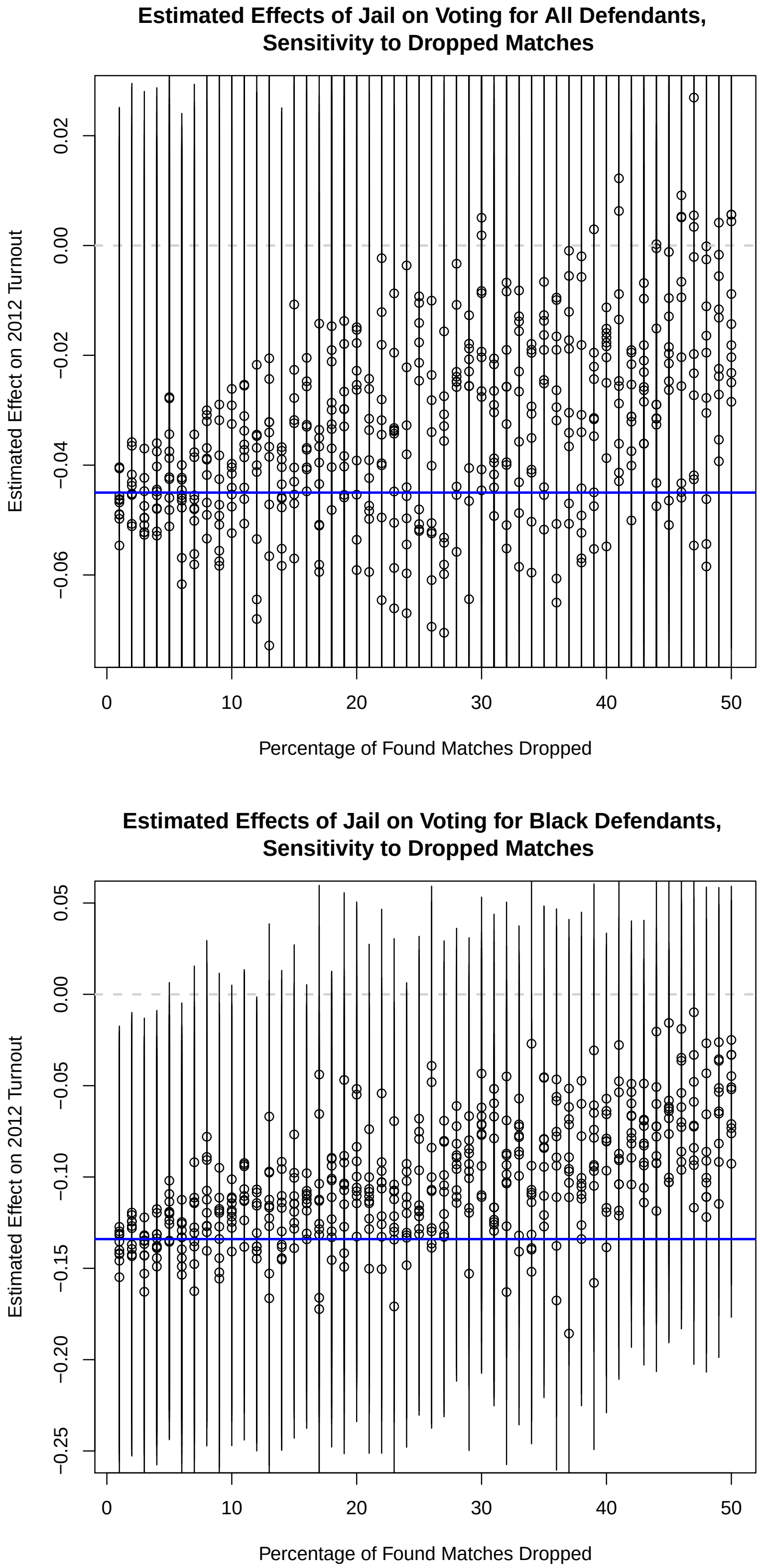
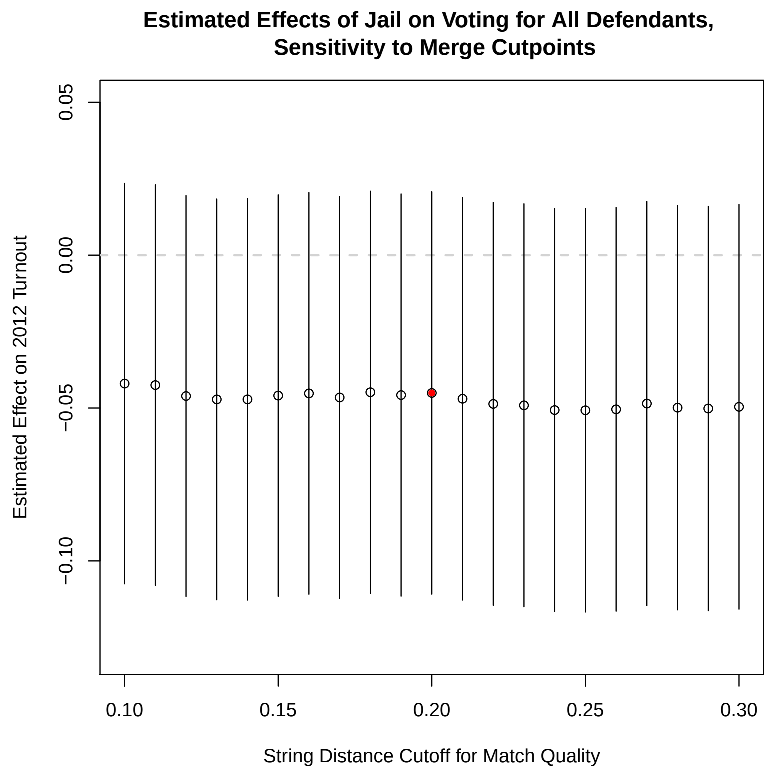


Figure A2: Sensitivity to dropping some (actual) matches.

3 Sensitivity to String-Distance Cutpoint

Next, I explore the decision I make to discard matches with first-name string distances of higher than .2 (using the Jaro-Winkler metric). I repeat the merge process using cutpoints between .1 and .3, and then rerun the main IV analyses with those new merged datasets.

Figure A3 plots the estimated effect of jail on voter turnout for all defendants, and for black defendants, under these different merge protocols. The red point estimate in the middle of the plot indicates the estimates presented in the main paper, while the similarity of the estimates across these different cutpoints suggests that this merging decision is not making a big difference in the results.



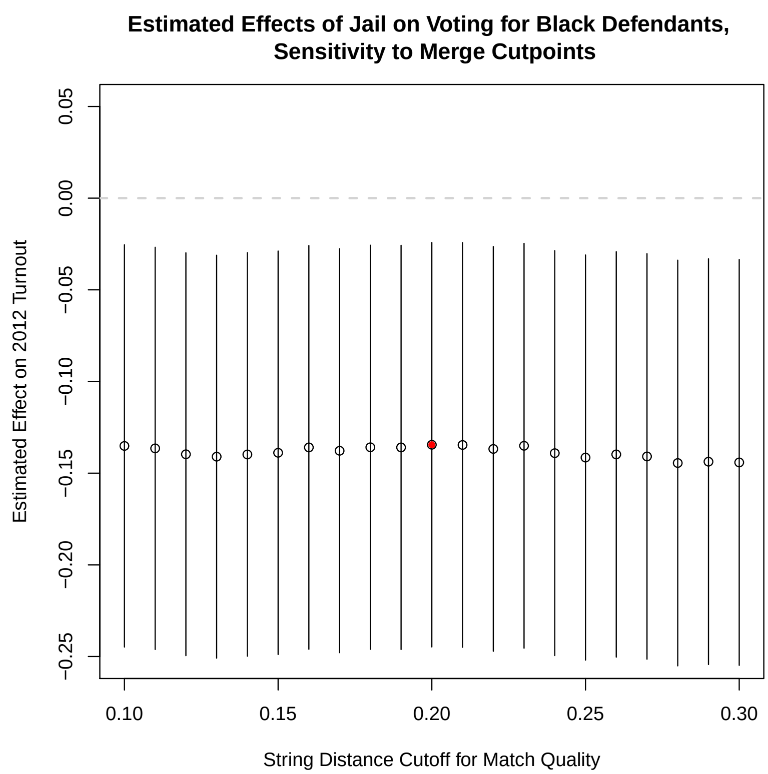


Figure A3: Sensitivity to changing the string distance cutpoint used to determine valid first-name matches.

4 Other Concerns: Racial Differences?

Another possible concern around the merge process is that defendants of different races might have names that are harder or easier to match to the voter file. If one group of voters was systematically less likely to be matched to the voter file (due to having more common or more commonly-misspelled names, for example), these missed matches could understate the effect of jail on voter turnout for that group. So if, for example, white defendants were more likely to experience missed matches, that difference could explain the reported difference (in the main paper) between Black and White defendants' demobilization effects.

There is no complete database of "correct" matches to validate my matches against, so I cannot measure the actual rate of missed matches across racial groups. However, one robustness check I can do is to focus in on the matched defendants, those that have been successfully found in the voter file. Focusing on registered voters has other drawbacks, but it provides a valuable check here. If the null results reported in the main paper for white defendants were being driven by poor matching, an analysis limited to registered voters should uncover the (true) larger effects.

Section 4.2.5 below presents this analysis for black and white defendants separately. In both cases, the estimates are less precise than the main estimates presented in the paper, as would be expected with a smaller sample. However, nothing in these results suggests that there is a substantial demobilization effect among white defendants that had been obscured by missed matches. The estimates for black defendants remain large and (marginally) significant, while the estimates for white defendants remain small (under 2 percentage points) and extremely noisy: still null results.

3 Courtroom Details

1 Random Assignment to Courtrooms

As discussed in the main paper, the court has a stated policy of random assignment of cases to courtrooms, done by a computer in the clerk's office. However, here I perform some additional checks to make sure the data looks as if cases were indeed assigned to courtrooms without regard to defendant or case characteristics.

I begin by regressing several key pre-treatment characteristics onto courtroom assignment dummies.[6] I try to predict defendants' characteristics using courtroom assignment: if I could predict gender or race from people's assigned courtroom, that would suggest some systematic variation in courtrooms' caseloads. Table A9 then presents F-statistics from these models. For pre-assignment characteristics like age or sex, the F-statistics are relatively small. This is as we would expect from random assignment. However, at the bottom of the table I regress sentencing outcomes onto courtroom assignment and find much larger F-statistics. This demonstrates that, as shown in Figure A7, courtrooms do not differ much on their cases' pre-assignment covariates (random assignment), but they differ a great deal in the sentences they give out to defendants (sentencing variation). This makes courtroom assignment a useful instrument for sentencing harshness.

Table A9: Testing Court Caseload Differences

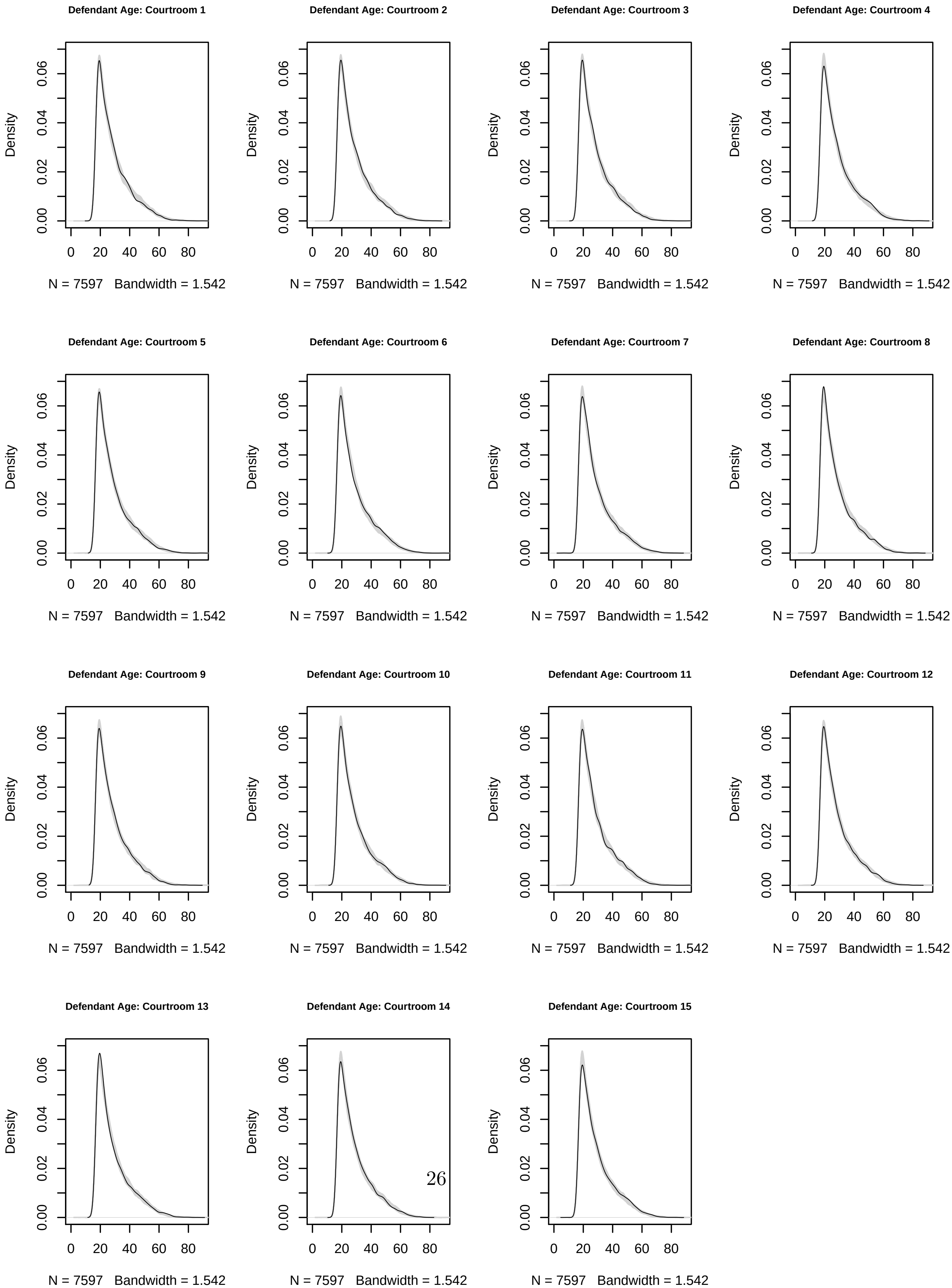
Variable	F-Statistic
Male	1.22
Black	1.38
Age	1.37
Conviction	8.99
Fine	21.79
Probation	9.33
Jail	6.61
Jail Time	11.89

[6] So "Courtroom1" is one if a person was assigned to courtroom 1 and zero otherwise, etc.

Next, I do some permutation tests for the main continuous pre-treatment variable that is available in these court records: age.[7] We might worry that courtrooms' caseloads would have the same mean defendant age, but perhaps have different distributions. In Figure A4, I plot both the courtrooms' actual age distributions as well as a set of many possible age distributions that could have arisen from random assignment. I begin with the actual (observed) distribution of cases to courtrooms. Then, I permute this dataset 100 times, each time "shuffling" the courtroom assignment of all defendants without consideration for defendant or case characteristics. For each of these "random-assignment" datasets, I plot the age distribution for each courtroom in gray. This gives us a sense for the possible range of age distributions that could have been observed under true random assignment. Then, atop this set of possibilities, I plot the observed age distribution for each courtroom. These actual distributions fall squarely within the range of possible distributions that could arise under random assignment.

The next two subsections continue to explore case assignment: Section 3.2 presents a by-race version of the main scatterplot from the paper, while 3.3 presents court caseloads in a variety of ways.

[7] Court records contain relatively few covariates about defendants, and most are binary or categorical: gender, race, hair and eye color.


Defendant Age: Courtroom 1
Defendant Age: Courtroom 2
Defendant Age: Courtroom 3
Defendant Age: Courtroom 4
Defendant Age: Courtroom 5
Defendant Age: Courtroom 6
Defendant Age: Courtroom 7
Defendant Age: Courtroom 8
Defendant Age: Courtroom 9
Defendant Age: Courtroom 10
Defendant Age: Courtroom 11
Defendant Age: Courtroom 12
Defendant Age: Courtroom 13
Defendant Age: Courtroom 14
Defendant Age: Courtroom 15
Density
0.00 0.02 0.04 0.06
0 20 40 60 80
N = 7597 Bandwidth = 1.542

2 Scatterplots by Race

Here, I present a scatterplot of baseline characteristics against courtroom harshness, just like Figure 1 in the main paper. But here, I present them by race, to make sure there isn't some subgroup imbalance that could be driving the effect heterogeneity found.

As in the main figure, these covariates look relatively balanced across courtrooms. To the extent there are small imbalances (such as an apparent positive relationship between the proportion of black defendants facing marijuana possession charges and the proportion jailed), this section presents evidence that these imbalances are not driving the main results. First, I note that Section 4.1 presents results when sequentially dropping each courtroom from the analysis and re-estimating the main models, suggesting that any given outlier courtroom cannot drive the main results. Second, table A10 presents versions of the main estimates of jail's effect on voting, using a dataset that omits marijuana possession cases (to address concerns that imbalance in courts' receiving these cases could be driving the main results). The results are quite similar to the main estimates.

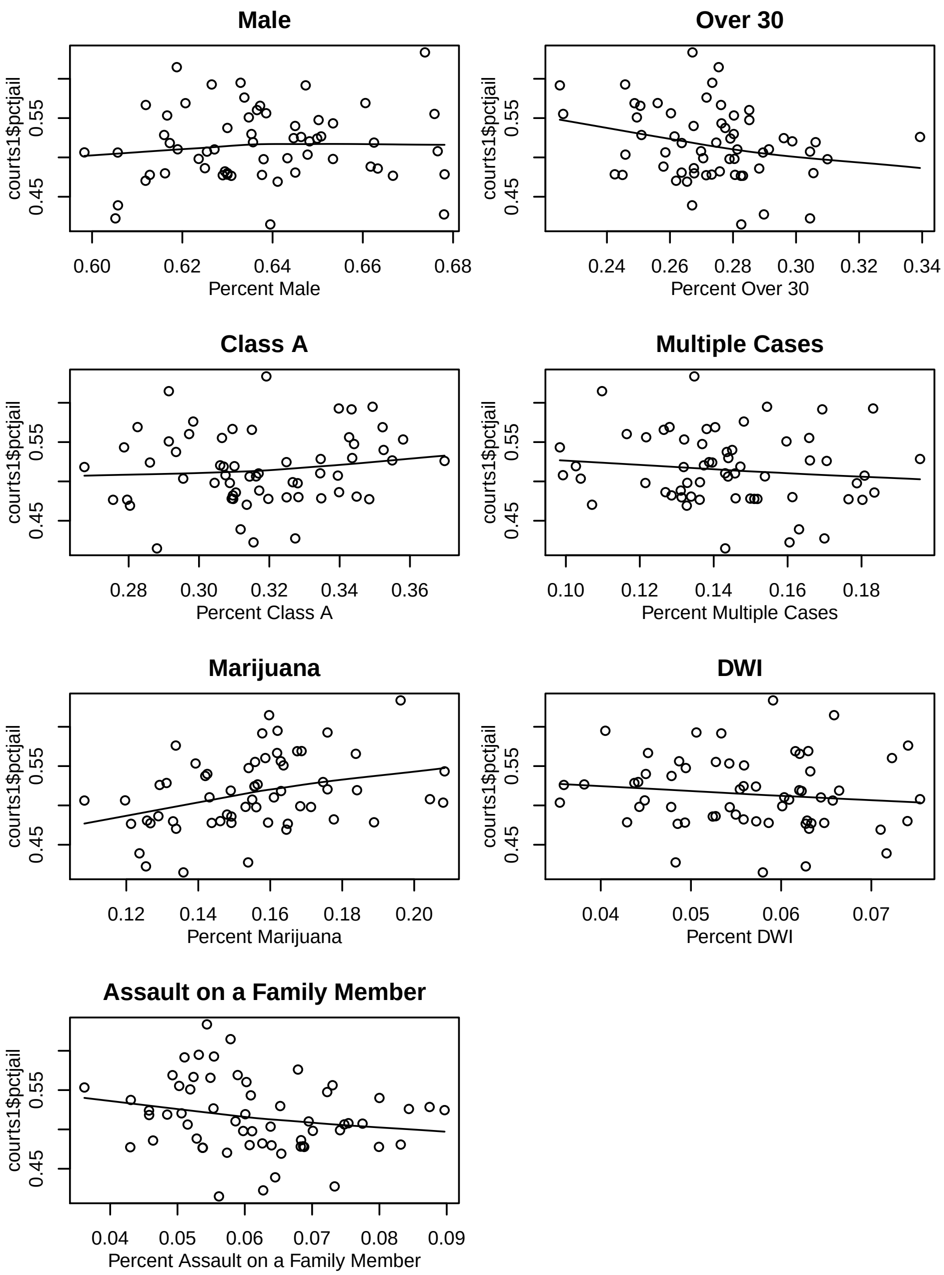


Figure A5: Scatterplots of pre-treatment case characteristics against courtroom incarceration rates, subsetting to black defendants. Each point represents one misdemeanor courtroom in a single year; lines are loess smoothers. Marijuana possession (0-2 ounces), driving while intoxicated (DWI), and assault on a family member are the most common charges in the dataset.

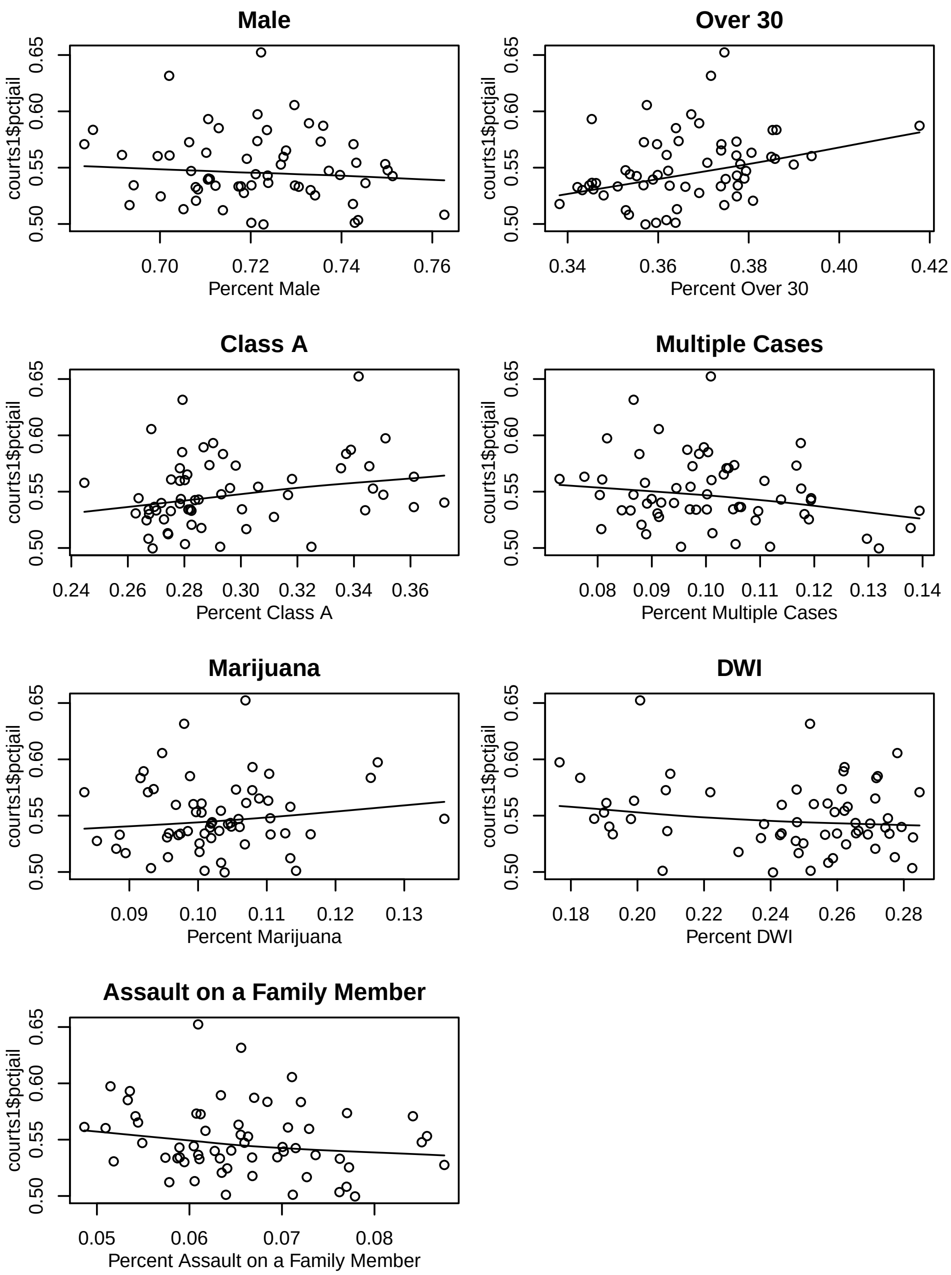


Figure A6: Scatterplots of pre-treatment case characteristics against courtroom incarceration rates, subsetting to white defendants. Each point represents one misdemeanor courtroom in a single year; lines are loess smoothers. Marijuana possession (0-2 ounces), driving while intoxicated (DWI), and assault on a family member are the most common charges in the dataset.

Table A10: Main IV estimates, dropping marijuana possession charges

	Dependent variable:	
	Voted2012	
	All Defendants	Black Defendants
	(1)	(2)
Jail	-0.067^{*}	-0.174^{***}
	(0.040)	(0.065)
Constant	0.158^{***}	0.286^{***}
	(0.023)	(0.032)
Year dummies	Yes	Yes
Observations	87,362	22,057
Adjusted R^2	0.023	0.034
Note:		*p<0.1; **p<0.05; ***p<0.01

3 More on Courtroom Caseloads

The first table in this section presents summary statistics about the defendants assigned to the various courtrooms over the time period examined. Similarly, Figure A7 summarizes various defendant and case characteristics by courtroom as a different way of demonstrating that caseloads are comparable across courtrooms as we would expect under random assignment. The random assignment of cases to courtrooms should mean that all fifteen courtrooms have similar caseloads, with similar numbers and types of cases as well as balanced defendant characteristics. Figure A7 shows the range of case and defendant characteristics in all 15 courtrooms; courtrooms' caseloads look quite similar on the pre-treatment covariates of sex, race, and age, as well as on charge severity (Class A versus Class B misdemeanor). Even the most extreme courtroom generally falls quite near the mean value of each of these variables. However, despite receiving similar caseloads, courtrooms then display very different sentencing behavior, as shown by the wide range of jail rates shown on the right-hand side of each panel. It is this variation that allows for the IV design used here.

Table A11: Defendant Characteristics by Courtroom, 2008-2012

Court	Total	Percent Male	Percent Black	Percent >30	Percent Jailed	Percent Voted 2012
1	7,602	0.697	0.268	0.338	0.517	0.131
2	7,556	0.695	0.277	0.342	0.587	0.121
3	7,447	0.697	0.285	0.341	0.513	0.125
4	7,600	0.701	0.278	0.348	0.533	0.128
5	7,566	0.706	0.280	0.340	0.537	0.128
6	7,541	0.697	0.282	0.356	0.502	0.123
7	7,440	0.702	0.274	0.343	0.497	0.125
8	7,589	0.691	0.273	0.333	0.551	0.132
9	7,671	0.691	0.283	0.341	0.528	0.130
10	7,613	0.698	0.275	0.344	0.545	0.129
11	7,688	0.687	0.277	0.348	0.530	0.119
12	7,509	0.694	0.286	0.341	0.527	0.127
13	7,509	0.691	0.268	0.341	0.534	0.125
14	7,563	0.693	0.284	0.346	0.555	0.129
15	7,473	0.692	0.279	0.353	0.528	0.130

As noted in the section above, the courtrooms have very similar caseloads on a number of dimensions, as would be expected under random assignment of cases to courtrooms. Next, I dig further into case types, to see whether some of the most common case types are evenly distributed across courtrooms. One way to do this is a table examining proportions of the most common case types across courtrooms in a given year; the table below does this exercise for 2011 as an example, and indeed courtrooms do appear quite similar. Another way is to plot courtroom proportions of the given case types across time. Figure A8 plots caseloads for all 15 courtrooms across years for three common case types in the data: DWI, marijuana possession, and assault on a family member. The plots demonstrate that courtroom caseloads are quite similar in any given year; to the extent there are over-time changes in the proportion of cases that fall into one charge category, they affect all courtrooms (as seen in the DWI plot). Note that the greater spread in 2008 values in these plots is due to the smaller amount of data included from 2008 (only the end of the year, after the presidential election).

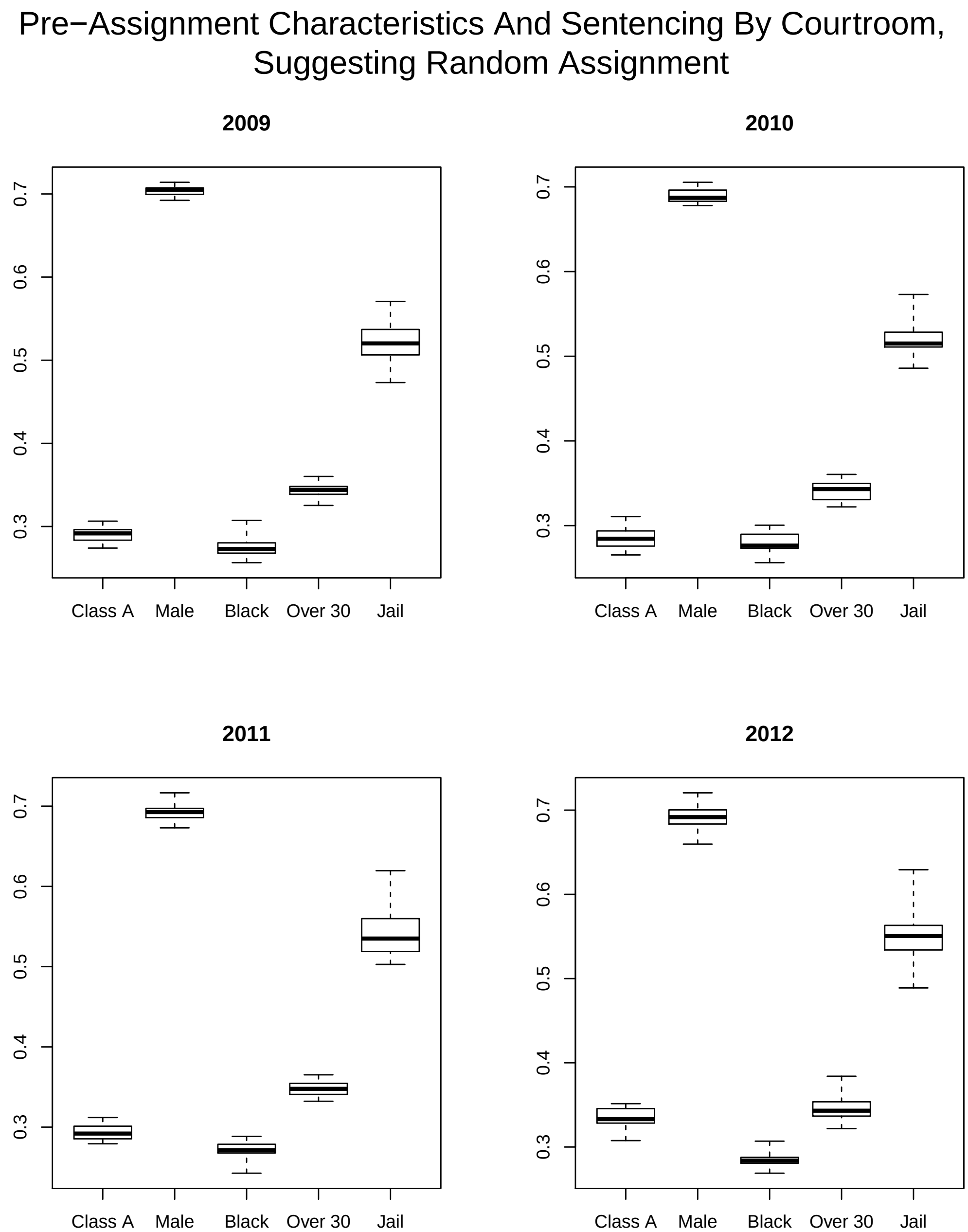


Figure A7: Box plot of the full range of several pre-treatment variables, as well as jail sentences, for the 15 county courtrooms. The box edges represent the 25th and 75th percentiles and the middle line the median value of the variable; the whiskers extend to the most extreme value of that variable among the 15 courtrooms in that year. The different courtrooms' values of pre-treatment variables such as age and race appear tightly clustered (reflecting the random assignment of cases to courtrooms), while the large spread on the "jail" variable demonstrates sentencing variability among the courtrooms.

Table A12: Common Charge Types Across Courtrooms, 2011

Court	Total	Marijuana Possession	DWI	Assault on a Family Member
1	1,822	0.109	0.211	0.066
2	1,798	0.115	0.198	0.064
3	1,783	0.107	0.201	0.068
4	1,811	0.116	0.193	0.057
5	1,856	0.119	0.206	0.067
6	1,818	0.097	0.211	0.068
7	1,786	0.108	0.213	0.058
8	1,837	0.116	0.214	0.053
9	1,870	0.119	0.209	0.068
10	1,846	0.108	0.217	0.066
11	1,847	0.115	0.193	0.068
12	1,753	0.126	0.201	0.062
13	1,837	0.122	0.210	0.054
14	1,815	0.112	0.210	0.057
15	1,747	0.129	0.203	0.058

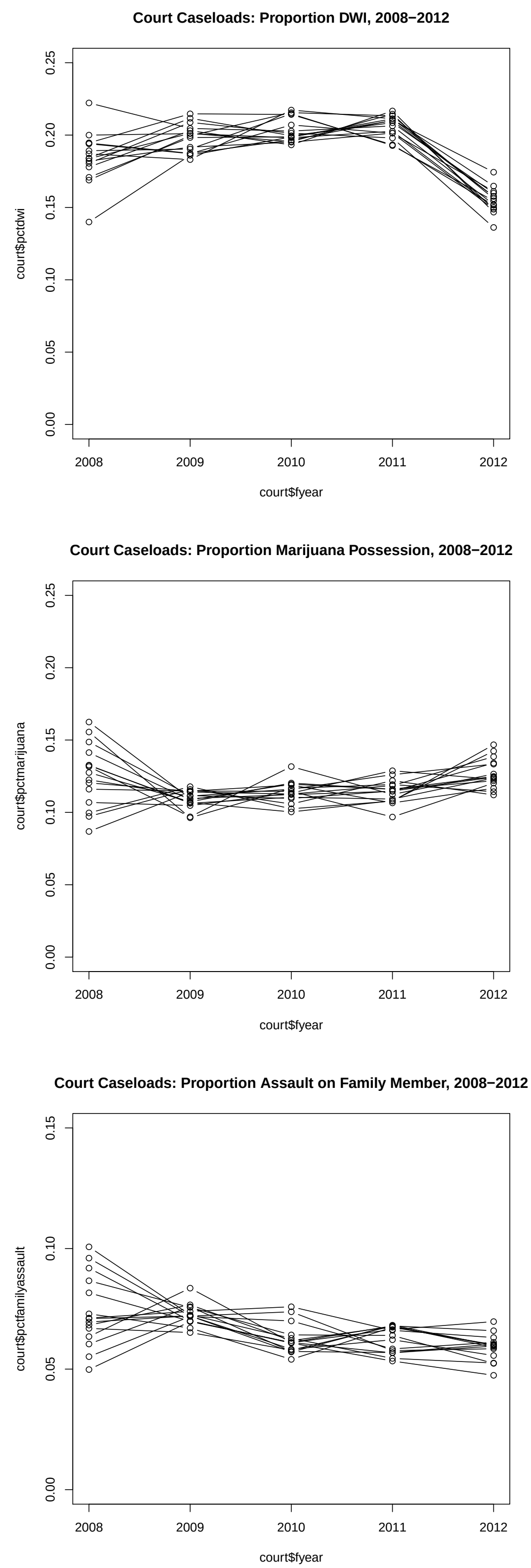


Figure A8: Plotting the prevalence of the most common case types across courtrooms and time.

4 Guilty Pleas and Trials

We might also wonder about whether defendants attempt to behave strategically, becoming more likely to plead guilty (rather than proceeding to a trial) if they are assigned to a harsher courtroom. Table A13 presents descriptive statistics of different case dispositions (dismissals, pleas, and trials). The proportions do not add up to 100%; omitted are cases decided with "deferred adjudication" (that may be dismissed after a period of good behavior, for example), or those that end in unexpected ways such as by the death of the person facing charges.

The main thing to note when evaluating questions of strategic behavior by defendants is that it is extraordinarily rare for misdemeanor cases to proceed to trial. This characteristic of the table is not caused by collapsing across multiple years of data; looking at courtroom-years yields a similar conclusion, with none of the courtroom-years in this dataset having more than two percent of cases proceed to trial.

It is true that harsher courtrooms (in terms of jail sentencing) are less likely to dismiss cases outright (and so are more likely to see defendants plead guilty rather than having their cases dismissed). I consider this to be part of the "story" of courtroom variation in jail sentencing, not a threat to inference or evidence of strategic behavior on the part of defendants (who obviously do not get to choose to have their case dismissed).

Table A13: Misdemeanor Case Dispositions Across Courtrooms, 2008-2012

Courtroom	Pled Guilty	Dismissed	Trial
1	0.472	0.343	0.006
2	0.556	0.314	0.006
3	0.460	0.353	0.006
4	0.486	0.337	0.010
5	0.487	0.339	0.006
6	0.455	0.370	0.004
7	0.469	0.369	0.003
8	0.502	0.325	0.008
9	0.480	0.355	0.004
10	0.503	0.310	0.005
11	0.478	0.351	0.003
12	0.482	0.338	0.007
13	0.477	0.341	0.007
14	0.497	0.310	0.006
15	0.488	0.336	0.004

4 Robustness Checks

1 Dropping Courtrooms

In this section, I sequentially drop courtroom-years and rerun the analysis, to ensure that the results presented in the main paper are not being driven by one particularly strange courtroom or caseload. Figure A9 drops each courtroom-year in turn and re-estimates the effect of jail on voting for Black defendants, obtaining extremely similar point estimates and p-values in all cases. The dark horizontal line represents the main estimate in the paper, and the jackknifed estimates cluster very near that value. Figure A10 takes this exercise a step further, dropping each courtroom (including all observations from that courtroom across all years of the data) in turn and repeating the analysis. As might be expected from an exercise that discards so much data, the estimates are somewhat noisier, but they remain quite consistent with the estimates reported in the paper.

2 Alternative specifications

In this section, I report a number of slightly different specifications. On the whole, the results reported in the main paper are robust to excluding various subsets of the data or including different covariates.

2.1 Limiting Age of Defendants

First, I limit the dataset to defendants whose recorded birthdates indicate that they were between 18 and 60 years old at the time their case was filed. This omits some people with extreme age values in the dataset, some of which are probably due to typographical errors in the court records. Table A14 presents the IV analysis of jail's effect on 2012 voting, restricting to defendants ages 18-60 (all defendants, and focusing on Black defendants). The results are quite similar to those reported in the main paper.

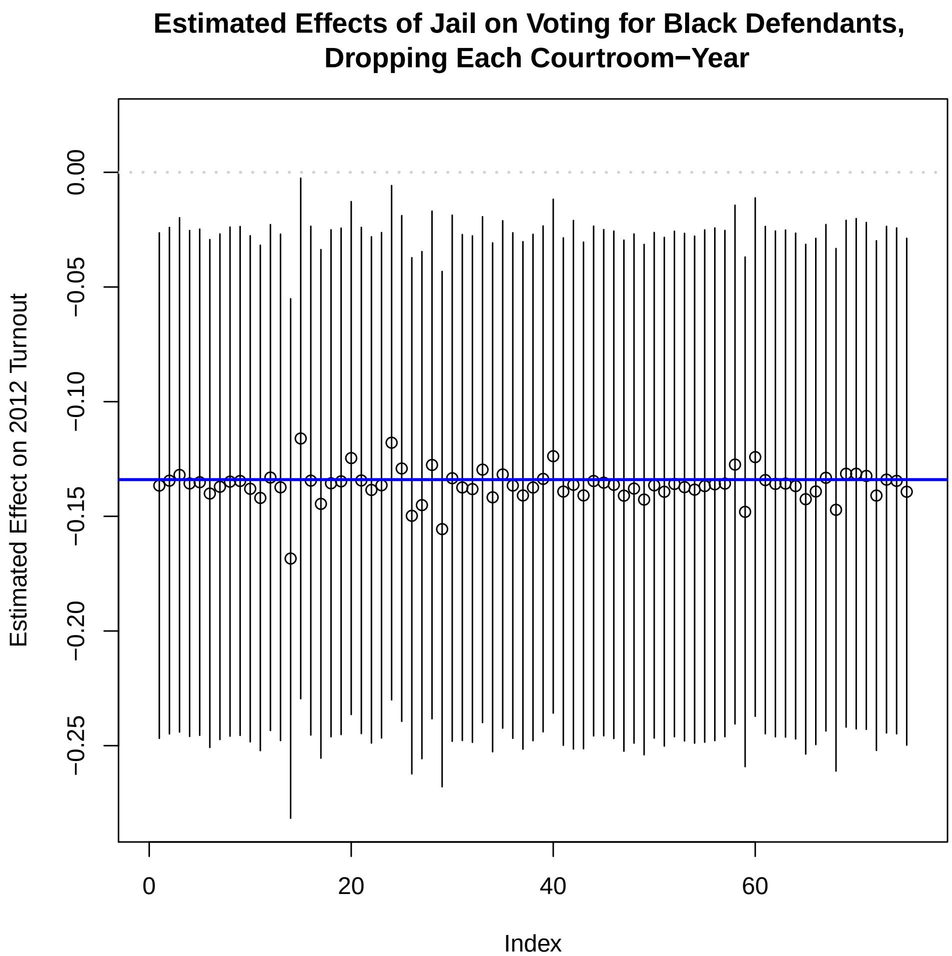


Figure A9: Results when sequentially dropping each courtroom-year

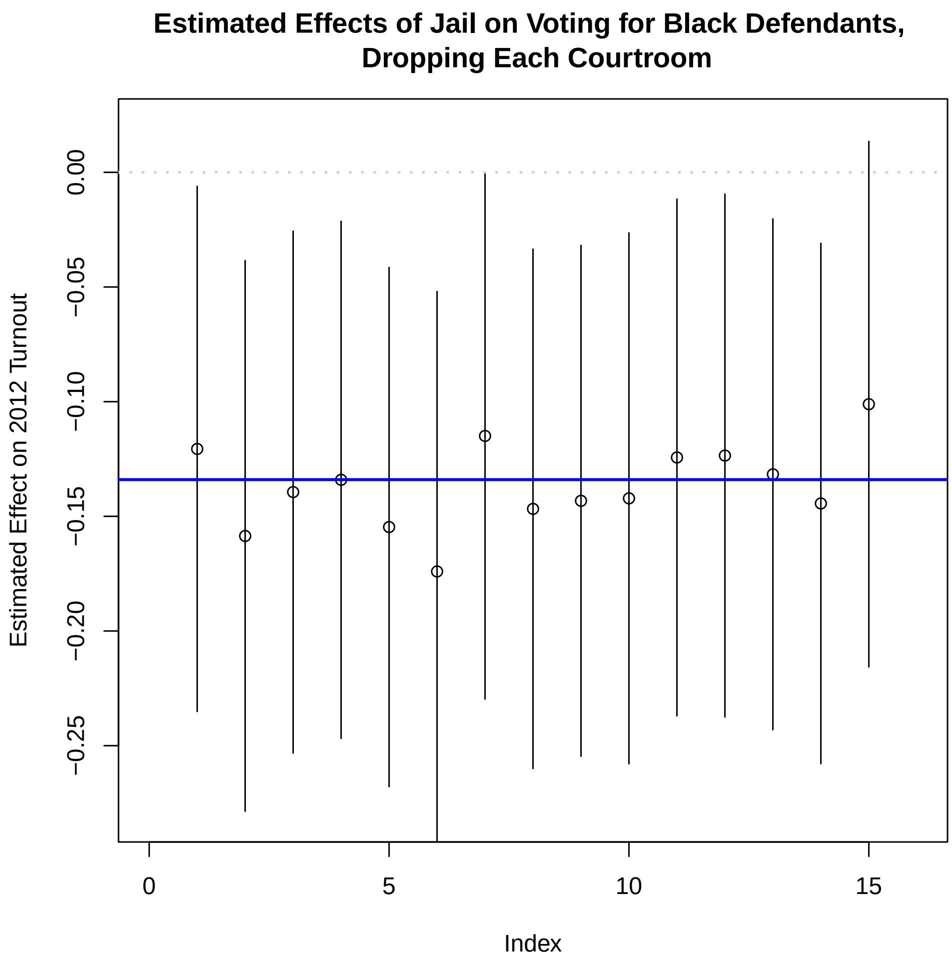


Figure A10: Results when sequentially dropping each courtroom (across all years)

Table A14: IV estimates: Jail sentences on voting, Defendants ages 18-60 only

	Dependent variable:	
	vote2012	
	All	Black
	(1)	(2)
Courtroom instrument	−0.050	−0.172***
	(0.036)	(0.061)
Jail	0.152***	0.301***
	(0.021)	(0.034)
Year dummies	Yes	Yes
Observations	101,694	27,484
Adjusted R^2	0.019	0.036
Note:	*p<0.1; **p<0.05; ***p<0.01	

2.2 Limiting Caseload of Defendants

Next, I limit the dataset to defendants with only one misdemeanor case filed at the time of their first misdemeanor charge (that is, I drop those of the first-time defendants who were charged with multiple misdemeanors at the same time).

Table presents IV estimates of jail on voting for this subset of defendants (overall and focusing on Black defendants in this subset); the estimates are quite similar to those reported in the main paper.

Table A15: IV estimates: Jail sentences on voting, Defendants with only one misdemeanor case

	Dependent variable:	
	vote2012	
	All	Black
	(1)	(2)
Courtroom instrument	-0.051	-0.134^{**}
	(0.036)	(0.059)
Jail	0.144^{***}	0.261^{***}
	(0.020)	(0.030)
Year dummies	Yes	Yes
Observations	100,519	26,935
Adjusted R^2	0.019	0.034
Note:	*p<0.1; **p<0.05; ***p<0.01	

2.3 Limiting Sex of Defendants

One concern about the match between court records and the voter file is that some defendants' names could change through time. This possibility is especially high for women, who sometimes change their names due to marriage. As a robustness check, I reproduce the main results of the paper for male defendants only.

Table A16 presents estimates of jail on voting for male defendants.

Table A16: IV estimates: Jail sentences on voting, Male defendants only

	Dependent variable:	
	vote2012	
	All	Black
	(1)	(2)
Courtroom instrument	-0.026	-0.148^{**}
	(0.039)	(0.065)
Jail	0.110^{***}	0.231^{***}
	(0.024)	(0.038)
Year dummies	Yes	Yes
Observations	78,836	20,098
Adjusted R^2	0.011	0.025
Note:	*p<0.1; **p<0.05; ***p<0.01	

2.4 Adding Covariates

In this section, I add a range of covariates to the main IV specification (both for all defendants and for only black defendants). As expected, including variables indicating defendant sex, the calendar month and day of week on which the charges were filed, defendants' age at filing, and charge severity do not substantively change the estimates of jail's effect on voting.

Table A17: IV estimates: Jail sentences on voting, adding covariates

	Dependent variable:			
	vote2012			
	Black	Black	All	All
	(1)	(2)	(3)	(4)
Jail	−0.145**	−0.131**	−0.049	−0.045
	(0.058)	(0.055)	(0.033)	(0.033)
male	−0.094***	−0.093***	−0.054***	−0.059***
	(0.011)	(0.010)	(0.006)	(0.006)
mostsevcharge	0.011**	0.010**	0.012***	0.012***
	(0.005)	(0.005)	(0.003)	(0.002)
ageatfile	0.00002***	0.00002***	0.00001***	0.00001***
	(0.00000)	(0.00000)	(0.00000)	(0.00000)
Constant	0.139	0.105***	−0.072	0.010
	(0.117)	(0.036)	(0.046)	(0.016)
Year dummies	Yes	Yes	Yes	Yes
Day-of-week dummies	Yes	Yes	Yes	Yes
Month dummies	Yes	Yes	Yes	Yes
Zip code dummies	Yes	No	Yes	No
Observations	29,406	31,501	104,288	113,226
Adjusted R^2	0.067	0.069	0.051	0.043
Note:		*p<0.1; **p<0.05; ***p<0.01		

2.5 Restricting to Registered Voters

In this section, I replicate the main IV analysis on a dataset restricted only to defendants who are registered voters (as of the time this voter file was collected in 2014). This yields estimates that are likely biased, because voter registration may well be post-treatment to incarceration. Section 7 below discusses this concern further.

The tables below present estimates for Black and White defendants separately. The estimates of jail's demobilizing effect on Black defendants are slighly noisier than those in the main table (as expected with a smaller sample) and slightly larger (as one might expect from a sample of registered voters). The point estimates of jail's effects on voting for white defendants are still extremely small and not statistically significant.

Table A18: IV estimates: Jail sentences on voting, Registered voters only

	Dependent variable:	
	vote2012	
	White	Black
	(1)	(2)
Jail	0.011	−0.202**
	(0.085)	(0.097)
Constant	0.304***	0.475***
	(0.042)	(0.047)
Year dummies	Yes	Yes
Observations	24,019	16,131
Adjusted R^2	−0.002	0.035
Note:	*p<0.1; **p<0.05; ***p<0.01	

2.6 Restricting to 2008 Voters

In this section, I replicate the main IV analysis on a dataset restricted only to defendants who are recorded as having voted in the 2008 election. As discussed in the main paper, 2008 voter turnout may actually be a post-treatment variable, since people who don't vote in 2012 (possibly due to jail sentencing) will be more likely to be purged from the voter file and so have no vote history record. Subsetting on it may introduce bias. Note also that there are relatively few black defendants recorded as having voted in 2008. All estimates here are large, noisy, and should be interpreted with extreme caution.

Table A19: IV estimates: Jail sentences on voting, All defendants with recorded 2008 turnout

	Dependent variable:	
	jail	vote2012
	OLS	*instrumental variable*
	(1)	(2)
Courtroom instrument	1.073***	
	(0.145)	
Jail		−0.139
		(0.139)
Constant	−0.234***	0.690***
	(0.084)	(0.055)
Year dummies	Yes	Yes
Observations	12,293	12,293
Adjusted R^2	0.008	0.007
F Statistic	20.688*** (df = 5; 12287)	
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A20: IV estimates: Jail sentences on voting, Black defendants with recorded 2008 turnout

	Dependent variable:	
	jail	vote2012
	OLS	*instrumental variable*
	(1)	(2)
Courtroom instrument	0.759^{***}	
	(0.160)	
Jail		-0.393^{*}
		(0.218)
Constant	-0.094	0.749^{***}
	(0.087)	(0.071)
Year dummies	Yes	Yes
Observations	5,317	5,317
Adjusted R^2	0.008	-0.034
F Statistic	9.181^{***} (df = 5; 5311)	
Note:		*p<0.1; **p<0.05; ***p<0.01

2.7 Using Sentence Length (Not Coarsening)

Another way of running this analysis would be to instrument for the length of the jail sentence received, rather than whether or not any jail sentence was assigned (binary). Doing this makes some assumptions about the distribution of treatment effects (we might not think, for example, that adding one day to a zero- or one-day jail sentence is the same as adding another day to a 20-day sentence).

I recalculate the courtroom assignment instrument as before, now using courtrooms' yearly average sentence length rather than their jail-sentencing rate, and use it to instrument for defendents' sentence length in days. Before calculating the instrument or running the IV analysis, I transform the sentence data to make it somewhat less skewed: I use log(sentence days + .01). Table A21 presents the analysis using this new instrument and endogenous variable for all defendants, while Table A22 focuses on black defendants. The results are noisier, but remain consistent with the idea that jail time diminishes voting.

Table A21: IV estimates: Jail sentence length on voting, All defendants

	Dependent variable:	
	sentencedays	vote2012
	OLS	*instrumental variable*
	First Stage	2SLS
	(1)	(2)
Courtroom instrument	1.000***	
	(0.042)	
Sentence Length (days, logged)		−0.00000
		(0.0002)
Constant	0.000	0.117***
	(1.257)	(0.007)
Year dummies	Yes	Yes
Observations	113,367	113,367
Adjusted R^2	0.008	0.00002
F Statistic	176.118*** (df = 5; 113361)	
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A22: IV estimates: Jail sentence length on voting, Black defendants

	Dependent variable:	
	logdays	vote2012
	OLS	*instrumental variable*
	First Stage	2SLS
	(1)	(2)
Courtroom instrument	1.000***	
	(0.075)	
Sentence Length (days, logged)		−0.019**
		(0.008)
Constant	0.000	0.175***
	(0.126)	(0.014)
Year dummies	Yes	Yes
Observations	31,507	31,507
Adjusted R^2	0.007	0.030
F Statistic	46.142*** (df = 5; 31501)	
Note:		*p<0.1; **p<0.05; ***p<0.01

2.8 Race-of-defendant Interaction

In the main paper (and section 1 above), I split the data by defendant race and run separate analyses for Black and White defendants (including calculating the instrument separately). Here, I present an analysis using the full dataset and a single non-race-specific instrument. This allows me to include dummy variables for defendant race, and easily test whether the treatment effects of jail on voting are different for Black and White defendants. As Table A23 indicates, the effect of jail on voting is much larger (in the negative direction) for Black defendants than for White defendants. However, note that the group-specific effect estimates may not be exactly the same as those presented in the main paper, because in this analysis I calculate the courtroom-harshness instrument across all defendants, whereas the main paper results calculate the instrument within each group (this yields a stronger first-stage relationship if courtrooms treat defendants of different races differently).

Table A23: Jail's Effect on Voting (Racial Interaction)

	Dependent variable:
	vote2012
Jail	0.003
	(0.041)
Black	0.178^{***}
	(0.032)
Jail x Black	-0.132^{**}
	(0.060)
Constant	0.085^{***}
	(0.024)
Year dummies	Yes
First Stage F-Statistic	30.24
Observations	113,367
Adjusted R^2	0.035
Note:	*p<0.1; **p<0.05; ***p<0.01

In section 2 above, I present separate analyses for Hispanic and non-Hispanic white defendants, again calculating the instrument separately within each group. Here, I present the interactive model from those subsets of defendants. Again, the estimates derived from this model are not directly comparable to the ones presented in the separate analyses due to the use of a different instrument. Table A24 suggests that Hispanic voters may show a larger effect of jail on voting, but I cannot statistically distinguish this effect from that measured for non-Hispanic voters.

Table A24: Jail's Effect on Voting for Hispanic Defendants (Interaction Model)

	Dependent variable:
	vote2012
Jail	0.014
	(0.043)
Hispanic	−0.047
	(0.045)
Jail x Hispanic	−0.025
	(0.079)
Constant	0.104***
	(0.024)
Year dummies	Yes
First Stage F-Statistic	35.21
Observations	77,750
Adjusted R^2	0.006
Note:	*p<0.1; **p<0.05; ***p<0.01

3 Other IV estimators

In this section, I present results using slightly different IV approaches.

3.1 Leave-one-out means

First, I instrument for jail using the "leave-one-out" means of courtroom jailing behavior; this calculates the courtroom mean separately for each defendant, dropping that defendant from the mean (so that the defendant's own outcome doesn't drive the value of the instrument). Table A25 presents these results for Black defendants. Column 1, without any covariates, shows that the estimate is quite similar to that in the main paper, though noisier. Column 2 adds in covariates for additional precision, and Column 3 limits to defendants ages 18-60 as in the analysis in Section 2.1 above.

Table A25: IV estimates (Leave-One-Out Means): Black Defendants

	Dependent variable:		
	vote2012		
	(1)	(2)	(3)
Jail	−0.130	−0.148*	−0.189**
	(0.090)	(0.086)	(0.094)
Male		−0.088***	−0.084***
		(0.015)	(0.015)
Charge Severity		0.009	0.009
		(0.006)	(0.007)
Constant	0.259***	0.277***	0.311***
	(0.047)	(0.036)	(0.036)
Year dummies	Yes	Yes	Yes
Observations	31,507	31,503	27,481
Adjusted R^2	0.034	0.045	0.045
Note:	*p<0.1; **p<0.05; ***p<0.01		

3.2 LIML, Fuller-k

Next, I present the estimates from other IV estimators, as estimated in Stata (using ivreg2). Column 1 presents 2SLS estimates of jail's effect on voting for Black defendants, as shown in the main paper. Column 2 estimates the same model with LIML (using the "liml" option in Stata 13), while Columns 3 uses a Fuller(4) estimator.

	(1)	(2)	(3)
	vote2012	vote2012	vote2012
jail	-0.146**	-0.146**	-0.146**
	(-2.67)	(-2.67)	(-2.70)
fyear	0.000940	0.000940	0.000941
	(0.44)	(0.44)	(0.45)
_cons	-1.608	-1.608	-1.610
	(-0.38)	(-0.38)	(-0.38)
N	31507	31507	31507

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3.3 Instruments by Race or by Charge Type

In the main analyses, the courtroom-harshness instrument is calculated within courtroom-years. In this section, I experiment with courtroom x year x race or courtroom x year x charge-type instruments. These could potentially achieve a better fit in the first stage, though there is a risk of overfitting and recovering the biased OLS estimates.

First, I present results from the full sample, calculating the courtroom-harshness instrument within each courtroom-year by race (allowing for the possibility that some courtrooms could be harsher to black than to white defendants). For this analysis, I drop several thousand defendants from the sample that are not categorized as black or white (these are listed as "Asian," "Indian," "Other," or no race) out of concern that these will produce very small courtroom x year x race cells.

Table A26 presents results from this analysis; the estimates look somewhat larger and more precise than the ones from the main analysis. The indicator for "White" is included in the model because it is incorporated in the construction of the instrument used; note that the coefficient here implies not that white defendants show more demobilization from jail time, but simply that whites have lower voter turnout overall (consistent with Section 4.3 of the main paper).

Next, I take a similar approach to constructing courtroom x year x charge-type instruments, allowing for the possibility that courtrooms could differ in their approaches to sentencing different types of charges. I again drop very uncommon values, in this case dropping all charges faced by fewer than 1000 defendants in my dataset. Then, I again construct leave-one-out means, as in Table A25 above, such that defendants cannot drive the mean of the cell they are in. Table A27 presents estimates of jail on voting using this courtroom-harshness instrument. Again, the estimates look somewhat larger than the ones from the main analysis (but they also represent a slightly different quantity, as I have omitted nearly 30,000 defendants with less-common charges).

Table A26: IV estimates: Jail on voting, Courtroom x Year x Race Instrument

	Dependent variable:	
	jail	vote2012
	OLS	*instrumental variable*
	(1)	(2)
Courtroom instrument	1.000***	
	(0.000)	
Jail		−0.066*
		(0.038)
White	−0.000	−0.107***
	(0.000)	(0.002)
Constant	−0.000***	0.232***
	(0.000)	(0.022)
Year dummies	Yes	Yes
Observations	109,257	109,257
Adjusted R^2	0.006	0.043
F Statistic	117.847*** (df = 6; 109250)	
Note:		*p<0.1; **p<0.05; ***p<0.01

Table A27: IV estimates: Jail on voting, Courtroom x Charge Instrument (Leave-one-out means)

	Dependent variable:	
	vote2012	
	All	Black
	(1)	(2)
Jail	-0.062^{*}	-0.190^{*}
	(0.009)	(0.024)
Constant	0.161^{*}	0.303^{*}
	(0.005)	(0.012)
Year dummies	Yes	Yes
Charge Dummies	Yes	Yes
Observations	100,003	26,801
Adjusted R^2	0.021	0.033
Note:		*p<0.05

3.4 Courtroom Dummies

Here, I present results from a slightly different IV approach: rather than constructing courtroom sentencing means, I simply included indicator variables for each courtroom, interacting those with case filing year due to non-monotonicity concerns. The results shown below, first for all defendants and then for Black defendants only, are extremely similar to those from the main specification.

Table A28: Jail Sentences on 2012 Voting

	Dependent variable:	
	vote2012	
	All	Black
	(1)	(2)
Jail	−0.045	−0.136*
	(0.034)	(0.056)
Constant	0.142*	0.263*
	(0.019)	(0.031)
Year dummies	Yes	Yes
Courtroom Dummies	Yes	Yes
Observations	113,367	31,507
Adjusted R^2	0.017	0.034
Note:		*p<0.05

5 Non-Focal Treatments

One possible threat to inference here are violations of the exclusion restriction from other courtroom "treatments." The main estimates assume that the only way courtroom assignment affects voter turnout is through jail sentencing. But if courtrooms do other things that could deter voting, and these other "non-focal treatments" are correlated with their jail sentencing tendencies, then the above estimates could be biased (Mueller-Smith, 2018).

Jail time seems like the most extreme punishment a misdemeanor courtroom can hand out, and so is likely to loom large. However, courtrooms make other decisions as well: defendants can be convicted or not, assessed fines, or put on probation.[8] Any of these non-focal treatments could matter for voting, but they only threaten the jail estimates if these treatments are correlated with jail sentencing. In that case, a person assigned to a given courtroom gets a "bundle" of treatments, which includes higher or lower risk of being sentenced to jail time, but also includes higher or lower risk of conviction, fines, probation, etc. Therefore, one way of assessing the threat posed by these other treatments is simply to examine whether they are correlated with jail sentencing tendencies.

I look at the correlations between courtroom-year-specific rates of different case outcomes. Courtrooms' tendency to assess fines is essentially uncorrelated with jail sentencing, at .05. Similarly, sentencing to probation is only slightly correlated with sentencing to jail, at -0.09. The negative correlation indicates that if probation did deter defendants from voting, my estimates of jail on voting would actually be understating the true effect.

However, courtrooms' conviction tendencies are more related to jail sentencing (correlation .45). If being convicted of a misdemeanor offense deters voting (either because people feel they have lost some part of their citizenship, or because they mistakenly believe such a conviction bars them from voting), then the main estimates for jail could be biased upwards. I address this concern both qualitatively and quantitatively below.

First, there are reasons to think that jail sentences are qualitatively more memorable than misdemeanor convictions. First-hand and journalistic accounts, along with qualitative social science research, bolster the idea that jail time is a formative and memorable experience for those sentenced to even short periods of confinement. Local jail conditions are often described as worse than prison conditions, marked by chaos, crowding, and a transient population (Irwin, 1985). Programs such as work opportunities or educational programs are essentially nonexistent. The social landscape is chaotic and sometimes threatening. The high suicide rate in local jails, which exceeds the prison suicide rate, is a testiment to the dire circumstances of inmates (Noonan and Ginder, 2013).

Harris County jails are no exception to this pattern of chaotic, under-resourced jail experiences. The county jail population has been increasing since the 1970's, and even after the construction of

[8] Courtroom experiences could theoretically matter, though time spent in the courtroom is brief and confusing for most defendants, and there is not much variation. Each courtroom handles dozens of cases per day, and defendants are rarely in front of the judge for more than a few minutes.

new jail facilities in the 1990's, the system rapidly approached maximum capacity again (Mahoney and Nugent-Borakove, 2009). Many people in the jail have mental health or substance abuse problems; the jail is the county's largest *de facto* mental health care provider. A 2009 letter from the Department of Justice following an investigation into the jail stated that "the Jail fails to provide detainees with adequate: (1) medical care; (2) mental health care; (3) protection from serious physical harm; and (4) protection from life safety hazards." (Division, 2009). In addition, there have been a number of high-profile unexplained deaths in county jail facilities (Hunter, 2009). Given these conditions, I find it plausible that even a short stay in jail could seriously change people's view of government and their willingness to vote.

Next, I account for any "conviction effects" by simultaneously instrumenting for jail and conviction (using the same approach as in the main analysis; the instrument used for conviction is the mean courtroom-year conviction rate). This approach results in noisy estimates, because jail and conviction are highly correlated. However, the point estimates (presented in SI Table A30) are substantively consistent with the main estimates presented here: jail still matters greatly for voter turnout.

Next, I subset the data to focus on courtrooms with similar conviction rates but variation in jail sentencing tendencies. I automatically construct subsets of the data from 10, 15, or 20 courtroom-years with the most similar conviction rates. Many of these subsets, despite their courtrooms having similar conviction rates, still show variation in jail-sentencing rates (my instrument). I rerun the main analyses on as many of these automatically-generated subsets as possible (dropping subsets where the first stage is too weak), and demonstrate that even in these smaller subsets, most estimates are still negative and comparable to the main results. That the estimated effects of jail on voting persist even when there is relatively little variation in conviction rates supports the idea that jail (not conviction) is the main causal pathway through which courtrooms affect voting.

Finally, I also present the reduced-form estimates of the courtroom-assignment instrument's effect on voting. Even if one does not believe the exclusion restriction that allows me to attribute the courtroom effect entirely to jail sentencing, these estimates of courtroom effects on voter turnout have a causal interpretation. These reduced-form estimates do not require us to assume that jail is the only causal pathway through which courtrooms affect voting. However, if we do believe the exclusion restriction, we can think of these effects as a mixture of the (large) effects for compliers, and the null effects for everyone unaffected by courtroom assignment.

For black defendants, these overall courtroom effects are significant and striking. Table A29 displays estimates from an OLS regression of 2012 voter turnout onto the courtroom-assignment instrument, demonstrating that courtroom assignment does have a clear effect on my outcome of interest.[9] Even if one isn't completely certain that jail is the only mechanism at play, it is clear that variations in one's randomly-assigned courtroom can shape later political behavior.

[9] The coefficients do not have a practical interpretation in this case, as they represent the change in turnout that would be expected if moving from a courtroom that jails 0% of defendants to one that jails 100%.

Table A29: Reduced-form: Courtroom assignment on voting

	Dependent variable:	
	Voted 2012	
	All Defendants	Black Defendants
	(1)	(2)
Courtroom Instrument	−0.045	
	(0.034)	
Courtroom Instrument		−0.136*
		(0.057)
Constant	0.142*	0.263*
	(0.020)	(0.031)
Year dummies	Yes	Yes
Observations	113,367	31,507
F Statistic	1.828 (df = 5; 113361)	2.073 (df = 5; 31501)
Note:		*p<0.05

1 Jail versus conviction

In this subsection, I report the results of a set of analyses that attempt to include both conviction and jail outcomes. I do this to address the concern that courtroom assignment actually influences voting through conviction and not only through jail sentences (a violation of the exclusion restriction that would bias my estimates of jail's effects). I cannot simply include conviction as a covariate because it is "post-treatment" in the sense that it occurs after courtroom assignment. Instead, I take two approaches here: instrumenting for both conviction and jail at once, and repeatedly subsetting the data to look at jail's effect within courtrooms with similar conviction rates.

Instrumenting for convictions Table A30 presents estimates from a series of models including both conviction and jail. This approach instruments for jail as in the main paper. The "conviction" instrument is constructed analogously to the instrument used for jail: it consists of the mean conviction rate for a given courtroom-year. The first column reports estimates from the full sample, while the second column focuses on Black defendants. Across the columns, the estimated effect of conviction is small (substantially smaller than the effect of jail), and positively signed. The estimated effects of jail are much more noisily-estimated than those reported in the main paper; this is not surprising, as conviction is highly correlated with jail sentencing. However, the point estimates remain comparable to those reported in the main paper.

Table A30: IV estimates: Jail sentences and Conviction on voting

	Dependent variable:	
	Voted 2012	
	All Defendants	Black Defendants
	(1)	(2)
Misdemeanor conviction	0.056	0.017
	(0.050)	(0.143)
Jail sentence	-0.081^{*}	-0.129
	(0.047)	(0.099)
Constant	0.118^{***}	0.246^{***}
	(0.032)	(0.077)
Year dummies	Yes	Yes
Clustered SE's	Courtroom	Courtroom
First Stage F-Statistic	3.56	64.63
Observations	113,367	31,507
Adjusted R^2	0.008	0.031
Note:		*p<0.1; **p<0.05; ***p<0.01

Iterated subsets

The challenge of sorting out jail versus conviction as possible mechanisms through which courtroom assignment could affect voting is that they are highly correlated. So one approach is to find subsets of the data in which jail and conviction are not so highly correlated.

To do this in a way that wasn't as researcher-driven as manually choosing which subsets to use, I automate the process. I sorted all courtroom-years by conviction rate, such that those with relatively similar conviction rates were grouped together. I then selected all possible subsets of 10, 15, or 20 courtroom-years from that ordered table (so it would be the 10 courtrooms that ranged from .635 to .648 conviction rates, or from .720 to .753, etc.– they'd be *consecutive* chunks of the whole dataset). I reran the main dataset's analysis on them, keeping track of the first-stage f-statistic, the actual range of conviction rates within the data, and the 2SLS estimates of jail on voting. In the three plots in Figure A11, I've plotted all feasible (first-stage f-statistic > 10) IV estimates of jail's effect on voting for Black defendants, along with their confidence intervals, based on these subsets. The estimates vary, as would be expected from such subsetting, but the bulk of them are still negative, consistent with the idea that even when we remove some of the variation in conviction rates, jail drives decreased voter turnout.

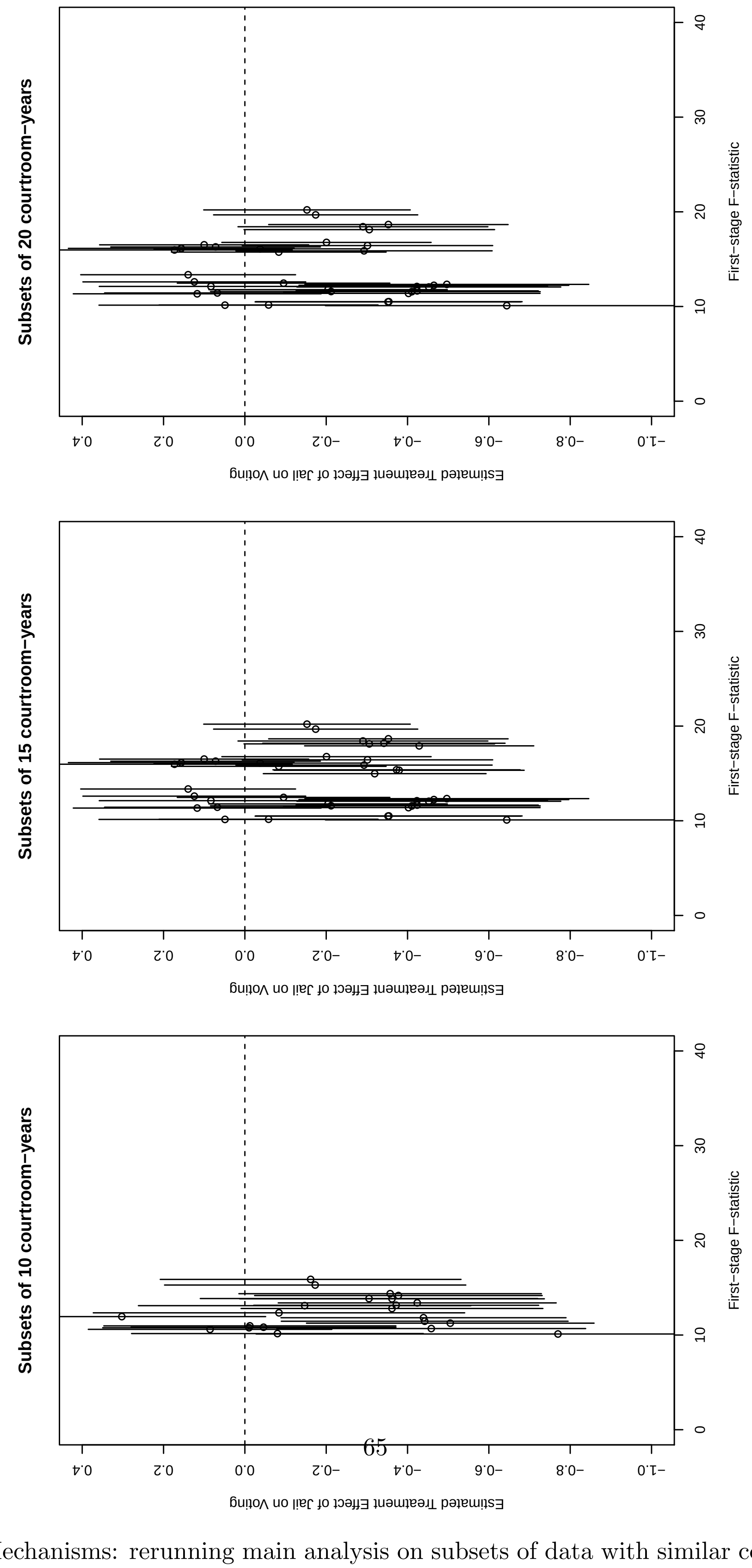


Figure A11: Mechanisms: rerunning main analysis on subsets of data with similar conviction rates (Black defendants).

6 Other Analyses

1 Timing and Effect Persistence

The main analysis presents results from several years of misdemeanor cases, and finds that jail decreases 2012 voter turnout among Black defendants. Do these short-term effects persist beyond the next presidential election? To answer this question, I use data from earlier years of misdemeanor cases (pre-2008), continuing to measure voting in 2012. If the effect is persistent, I should still see diminished 2012 turnout from misdemeanor charges filed in earlier years.

Using additional years of courtroom data comes with some concerns. First, it is possible that courtroom procedures have changed dramatically over time, such that it would be inappropriate to group together many years of data. However, Harris County's court system appears to have been relatively consistent over the past decade.[10] Second, the possibility of differential attrition (that people assigned to harsher courtrooms become more likely to move out of state and thus to not appear in the voter file due to their move, not to any political withdrawal) is an even bigger concern. Even regular attrition, in which people are equally likely to move out of state regardless of their courtroom assignment, could be a problem, as it would introduce noise that could attenuate the estimated effects. We should approach these estimates with caution.

In this section, I re-run the main analysis for all defendants and for Black defendants alone, this time including all first-time misdemeanor charges filed between 2000 and the 2012 election. Table A31 presents these 2SLS estimates. The first two columns of the table estimate the effect of jail on 2012 voting for all defendants; Column 1 is based on 2000-2012 data, while Column 2 is based on 2000-2008 only. Columns 3 and 4 present estimates for Black defendants only, from 2000-2012 and 2000-2008 respectively. For both sets of defendants, the estimates fall short of statistical significance when I restrict to the pre-2008 election period. However, the estimated coefficients remain large and negative, suggesting the possibility of persistent effects through time. As in the main analyses, Black defendants show a larger, clearer pattern of deterrence.

[10]Major changes, such as the creation of new courtrooms and the implementation of computerized case assignment, as well as the building of new jail facilities, took place in the 1990's, prior to the data I present here.

Table A31: IV estimates: Jail sentences on voting, 2000-2012

	Dependent variable:			
	Voted 2012			
	All defendants		Black defendants	
	(1)	(2)	(3)	(4)
Jail	−0.036	−0.025	−0.078*	−0.047
	(0.023)	(0.030)	(0.037)	(0.047)
Constant	0.171*	0.164*	0.285*	0.264*
	(0.016)	(0.021)	(0.025)	(0.032)
Year dummies	Yes	Yes	Yes	Yes
First Stage F-Statistic	5948.54	2454	135.51	88.62
2009-2012 data included	Yes	No	Yes	No
Observations	347,870	238,883	93,233	62,954
Adjusted R^2	0.012	0.008	0.023	0.015

Note: *p<0.05

2 Identifying Hispanic Defendants by Surname

The court records used for this project identify defendant race as Black/White/Asian/Native American/uncategorized/other, grouping Hispanic defendants into the White category. In this section, I attempt to identify Hispanic defendants using lists of spanish surnames from the US Census.

Taking a fairly simple approach to surname classification, I began with Census 2000 data on surnames belonging to over 100 people.[11] If this Census dataset indicates that 90% or more of people holding that surname identified as Hispanic or Latino on the Census, I use that name to indicate Hispanic/Latino identity in my dataset of defendants. Thus, this is a loose categorization: many people may identify as Hispanic or Latino but have surnames that are not on this list.

Using this surname list, I identify 29,582 defendants (of the 77,787 listed as "White" in the court records) as Hispanic, likely an undercount.[12] As I did in the main paper with White and Black defendants, I split the dataset to construct the courtroom-sentencing instrument and run the IV analysis separately on Hispanic and non-Hispanic defendants. When running the analysis this way, I find evidence of substantial demobilization among Hispanic defendants. The IV estimates in column 2 of Table A32 indicate that jail caused an almost 11-percentage-point drop in turnout for Hispanic defendants. Column 4 suggests a small, but insignificant positive effect for non-Hispanic defendants (or, to be more precise, White defendants without surnames that clearly indicate Hispanic identity). However, when I run an interactive model (using the instrument calculated in the full dataset, and adding an interaction term between jail sentencing and Hispanic identity), there is not a significant difference between the Hispanic and non-Hispanic defendants' jail effects.[13]

3 Other Subgroups

This section looks at other ways of splitting the sample into subgroups, similar to the racial heterogeneity explored in the main paper. I do not have strong theoretical predictions for these subgroup differences, and do not present them as "tests" of any theory. Instead, the intuition here is that if there are genuinely (African American) defendants being demobilized by jail time, they should appear in any other subsetting of the data as well. That is, if all other subgroup analyses yielded null effects, it might suggest that the effects observed among black defendants were simply a fluke. As such, Table A33 presents analyses that split the sample by gender, by age, and by charge severity. There do appear to be substantial demobilization effects within other subgroups, such as people over 30 and people facing Class A cases.

[11] Downloaded from http://www2.census.gov/topics/genealogy/2000surnames/names.zip in June 2015.

[12] A very small number of defendants classified as other races also had surnames from this list. I omit them from this analysis due to concerns about double-counting defendants by including them in multiple analysis groups.

[13] See SI section 4.2.8 for this table.

Table A32: IV estimates: Jail sentences on voting, Latino (Columns 1-2) and Anglo (Columns 3-4) defendants

	Dependent variable:			
	jail	vote2012	jail	vote2012
	OLS	*instrumental variable*	*OLS*	*instrumental variable*
	(1)	(2)	(3)	(4)
Courtroom instrument	1.000***		1.000***	
	(0.088)		(0.068)	
Jail		−0.105**		−0.014
		(0.042)		(0.044)
Constant	−0.000	0.117***	0.000	0.120***
	(0.058)	(0.028)	(0.039)	(0.025)
Year dummies	Yes	Yes	Yes	Yes
Observations	29,570	29,570	48,180	48,180
F Statistic	30.041*** (df = 5; 29564)		56.043*** (df = 5; 48174)	
Note:				*p<0.1; **p<0.05; ***p<0.01

Table A33: IV estimates: Jail sentences on voting (other subgroups)

	Dependent variable:					
	Voted 2012					
	Men	Women	Over 30	Under 30	Class A	Class B
	(1)	(2)	(3)	(4)	(5)	(6)
jail	−0.016	−0.083	-0.117^{**}	−0.016	-0.114^{**}	−0.011
	(0.037)	(0.053)	(0.057)	(0.035)	(0.054)	(0.036)
Constant	0.104^{***}	0.209^{***}	0.223^{***}	0.107^{***}	0.172^{***}	0.124^{***}
	(0.023)	(0.025)	(0.033)	(0.020)	(0.028)	(0.022)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-Statistic	68.28	53.97	43.05	73.95	48.56	83.13
Observations	78,836	34,411	38,960	74,310	33,859	79,508
Adjusted R^2	0.008	0.016	0.037	0.007	0.026	0.005
Note:	*p<0.1; **p<0.05; ***p<0.01					

4 Characterizing Compliers

Section of the paper discusses the extent to which we can generalize the LATE estimated here to the full set of people facing misdemeanor charges. Here, I discuss some of the characteristics of "compliers" from this design.

Describing Compliers

Though I cannot identify individual compliers, I can describe the distribution of their characteristics on the relatively few personal covariates that are available from court records. As noted by Angrist and Pischke (2008)(p. 171), the relative likelihood of compliers having a particular binary characteristic is represented by a ratio of the first stage for people with that characteristic to the overall first stage. So I can tell whether compliers are moe likely to be women by calculating the first-stage relationship for only women and dividing that by the first stage from all defendants.

When I do this focusing on black defendants, I find that compliers were more likely to be female, less likely to be facing more serious "Class A" misdemeanors, and less likely to be over 30 than the full sample of defendants. However, they were not any more or less likely to be recorded as having voted in 2008. In the next section, I present an analysis that "reweights" the complier population to look more like the full sample.

Reweighting the sample

Next, I run an analysis reweighting the complier population to resemble the main set of Black defendants, following Aronow and Carnegie (2013). I use their code for Inverse Compliance Score Weighting and bootstrapped standard errors. I dichotomize the courtroom-harshness variable, such that anyone who faced a courtroom with above-median incarceration rates receives a 1 and all others are set to 0. I reweight the population based on the available background covariates: age, gender, charge severity (class A versus class B misdemeanor), and past voter turnout (2008). I follow Aronow and Carnegie's approach to bootstrapping the standard errors for these estimates.

The resulting estimates (calculated separately for Black defendants in Column 1 and White defendants in Column 2) are shown in Table A34. Note that the estimated ATE from this approach (-.28, to a drop of 28 percentage points in turnout) is larger than the main result presented for Black defendants (-.13). This approach requires strong assumptions, but may help to address concerns that the LATE estimated in the main paper is based on a strange and especially easily-demobilized sample. Similarly, the persistent null results (small, insignificant positive coefficient) for white defendants suggest that the null effect found the paper isn't driven by a strange set of of compliers or the instrument operating differently among the white population.

Table A34: Reweighted IV estimates (ICSW): Effect of jail on voting

	Dependent variable:	
	Vote2012	
	(1)	(2)
Constant	0.259***	−0.003
	(0.062)	(0.020)
Male	−0.072***	−0.043***
	(0.024)	(0.012)
Age	0.005***	0.004***
	(0.0004)	(0.0004)
ClassA	−0.013	−0.018***
	(0.009)	(0.004)
Jail	−0.283**	0.058
	(0.130)	(0.067)
Note:	*p<0.1; **p<0.05; ***p<0.01	

5 Substantive Importance

In this section, I talk through the possible ramifications of the effects presented in the paper under certain assumptions about the misdemeanor-defendant population nationwide.

My estimates from Harris County, Texas indicate an overall demobilization effect of about 4 percentage points for all defendants in the 2012 election, though this is imprecisely estimated. And for Black defendants, the effect of jail on later turnout is a negative 13 percentage points. These estimates are drawn from first-time misdemeanor defendants in Harris County, and specifically from "compliers" whose jail sentencing outcomes depended on their courtroom assignment.

In order to generalize from these results to some estimate of how many potential voters stayed away from the polls in 2012 nationwide, we would need to make several assumptions. First, we need a guess at how many misdemeanor defendants served jail time between the 2008 and 2012 elections. We need an estimate of how many of these jailed people were Black, and of how many were first-time defendants (we might expect that repeat offenders would have a smaller treatment effect, as they'd be less likely to vote in the first place). We also need to make some assumptions about how the paper's Local Average Treatment Effect (LATE) generalizes to the full defendant population.

For the first few quantities, I turn to a recent report from the Bureau of Justice Statistics.[14] This report estimates that in one recent year (12 months 2011-2012), 11.6 million people were admitted to local jails. On any given day, 36.9% of local jail inmates were Black. And 39.4% of local jail inmates had been convicted of a crime (the rest had been arrested but not convicted of anything); many of these inmates will have been convicted of misdemeanor offenses, though some may be convicted felons either serving short sentences or awaiting transfer to prison. Using BJS data from this and another recent report, I estimate that about one-tenth jail inmates have been convicted of felonies, not misdemeanors, such that the actual proportion of local jail inmates with misdemeanor convictions should be something like 28%.[15]

However, the estimate of 11.6 million admissions to local jails includes the possibility of individuals being re-arrested within the year and double-counted. Estimates of jail return rates are scarce (especially for people who have been admitted to local jails but not necessarily convicted of any crime). One estimate from New York City data from 2009-2010 is that roughly 80 percent of yearly misdemeanor arrests are unique (and the other twenty percent are people being re-arrested within the year).[16] An analysis of Cook County, IL jail admissions from 2012 reaches almost exactly the same estimate, that 20% of jail admissions represent re-arrests of people who have already been admitted to jail in the past year.[17] However, at a national level, duplicate admissions can also take

[14] The report, "Jail Inmates at Midyear 2012 - Statistical Tables" was downloaded from http://www.bjs.gov/content/pub/pdf/jim12st.pdf in August 2015.

[15] See table 6 of this report for estimates of prisoners held in local jails: http://felonvoting.procon.org/sourcefiles/corrrectional-populations-in-2013.pdf

[16] `http://johnjay.jjay.cuny.edu/files/web_images/10_28_14_TOCFINAL.pdf`, p.16

[17] Source: calculated from Table 1 of this report: `http://ecommons.luc.edu/cgi/viewcontent.cgi?article=`

the form of people being arrested in multiple muncipalities (so they spend time in different jails). And further, people can be admitted to jail multiple times without being arrested multiple times, if they are transferred between multiple facilities (as sometimes happens in the face of overcrowding). For this reason, I multiply the 11.6 million jail admissions by .5 to account for these many sources of overcounting.

A very simple back-of-the-envelope calculation would multiply the total number of unique local jail admissions (11.6m x .5 = 5.8m) by the proportions of those admitted who were Black (36.9%) and had been convicted of misdemeanors (28%), to reach a total of roughly 600,000 Black convicted inmates admitted to local jails in 2012. However, these include repeat offenders. I do not have national data on what proportion of people admitted to local jails are first-time defendants. In Harris County, first-time defendants make up roughly two-thirds of misdemeanor cases, but this is likely an undercount: people who have never faced charges in Harris County could still have been convicted of misdemeanors in other jurisdictions. I make the slightly more conservative assumption that one-half of the 600000 jail inmates are first time offenders. This yields an estimate of about 300000 Black first-time convicted jail inmates in 2012. If we assume local jail populations have remained fairly stable over the last few years,[18] we can multiply this quantity by 4 to get an estimate of how many Black first-time defendants were sent to local jails between the 2008 and 2012 elections: about 1.2 million.

If I multiply this back-of-the-envelope estimate of the affected population by the main paper's estimate of jail's demobilizing effect on Black first time defendants (-.13), I estimate that roughly 156,000 people were deterred from voting in 2012 by misdemeanor jail sentences. Recall, of course, that this is based on extrapolating an imprecisely-estimated LATE to a larger population, so the estimates are certainly also consistent with a smaller number of affected people.

Another approach would be to extrapolate more directly from Harris County's experience: look at the proportion of Black residents who end up with first-time misdemeanor jail sentences over a four-year period, and scale that up to the entire Black population of the US. So we begin with the 16,192 people sent to jail in Harris County over the four years of the study, which represents nearly two percent of the Black population of Harris County (according to the U.S. Census). Applying this same rate of jail exposure to the full Black population of the U.S. yields an estimate of about 765,000 people affected; multiplying by the demobilization effect from the main paper would suggest that about 100,000 people stayed home in 2012 due to a misdemeanor jail sentence.

1015&context=social_justice

[18] Local jail populations actually declined slightly between 2008 and 2012, so this is a conservative choice.

7 2008 Vote: Placebo test, and Concerns

Another possible placebo test that one could run (in addition to the one shown in Section 2 above) would be to look at whether jail sentencing from after the 2008 election "affects" voter turnout in the 2008 election. This effect is logically impossible, making it a useful placebo test. However, the nature of the data used here make it a less compelling one: the voter file used for this analysis was collected in 2014, meaning that one's presence on the file could actually be post-treatment to jail time (if, for example, jail time made people less likely to vote and thus to be purged from the file by 2014). In this section, I present this placebo test, but also include information that calls into question its usefulness as a diagnostic tool.

Table A35 presents estimates of the "effect" of 2008-2012 jail sentences on 2008 voting (as recorded in the 2014 voter file). There is a large and statistically significant estimated effect of jail on pre-arrest voting among Black defendants, which certainly merits further investigation. The rest of this section investigates whether these placebo results call into question the main estimates in the paper, or simply reflect a problem with using voter files to capture long-past election behaviors.

Table A35: Placebo IV estimates: Jail sentences on voting, by race

	Dependent variable:	
	Voted 2008	
	Black Defendants	White Defendants
	(1)	(2)
jail	-0.182^{*}	0.051
	(0.056)	(0.036)
Constant	0.281^{*}	0.049^{*}
	(0.037)	(0.020)
Year dummies	Yes	Yes
Clustered SE's	Courtroom	Courtroom
First Stage F-Statistic	52.81	64.63
Observations	31,507	77,750
Adjusted R^2	0.023	-0.032
Note:		$^{*}p<0.05$

1 Possible interpretations of placebo test results

There are two possible interpretations of the results shown in Table A35. The first is that the test helps to diagnose a failure of random assignment of cases to courtrooms, perhaps suggesting that voters are better able to sort themselves into lenient courtrooms and thus to evade jail time. The second is as I have laid out above: the test actually demonstrates a problem with using a recent voter file to measure long-ago turnout, particularly when that file was collected post-treatment to the intervention of interest (that is, while actually voting in 2008 is not post-treatment to jail sentencing, "being observed to have voted in 2008" is post-treatment). Here, I present some additional evidence to adjudicate between these two interpretations.

First, I note that all other evidence is consistent with random assignment. From my personal interactions with Harris County employees, to the balance tests on (definitively pre-treatment) characteristics like age and race shown in the main paper and in Section 3.1 above, to other research that has used data from the same court system in the same years and concluded that cases appear to be randomly assigned (Mueller-Smith, 2018), all other available evidence suggests that cases are being genuinely randomly assigned to courtrooms. Next, I explore other observable implications of the idea that voters might be sorting themselves into more lenient courtrooms.

One prediction of randomization failure could be caseload size: if some courtrooms had a reputation for harshness that allowed well-connected defendants to know to avoid those courtrooms, we might expect that these harsher courtrooms would handle fewer cases per year than more lenient ones. But Figure A12 below demonstrates that harsher courtrooms do not seem to handle any fewer cases each year, as one would expect if voters were fleeing these courtrooms in search of leniency.

We might also wish to directly test the proposition that voters tend to be assigned to more lenient courtrooms. As discussed above, it is hard to do this with the main dataset, as the voter file was collected in 2014 (after possible purges of 2008 voters took place) and so its measure of 2008 voting is essentially post-treatment to courtroom assignment and sentencing. However, if one could either collect an older voter file, or newer court records, one could directly test the idea that voters are sent to more lenient courtrooms. I have been unable to find a Texas voter file from 2008, but have collected additional Harris County court records that run through 2016. This means that I can run a similar test: in cases filed after the 2012 election, are 2012 voters more likely to be sent to lenient courtrooms than 2012 non-voters? This is not a perfect test, because it is possible that there was only a failure of randomization in the years 2008-2012 and not later, but I note that there was no substantial shift in court organization in 2012 and I see no reason that there would be such a localized failure of randomization. (Also, see Section 7.2 below for evidence that the main estimates from the paper are replicable during this additional period.)

Figure A13 first looks at balance in courtroom case assignment for all cases filed after the 2012 election through the end of 2016. The line is essentially flat; there is no evidence that harsher courtrooms receive fewer voters. However, we might still worry that this pooled figure is obscuring year-by-year patterns. And because the voter file used here was collected in mid-2014, we are especially interested in the distribution of cases in 2015 and 2016, where our measurement of 2012 voting is unquestionably “pre-treatment”.

To this end, Figure A14 breaks out the data by year. As noted, the distribution of cases in 2015 and 2016 is likely the cleanest test of the proposition that voters are not randomly assigned to courtrooms, since these cases were filed after the voter file was collected and 2012 voting records were “frozen”. Particularly in these years (but also across the four panels), there is no clear pattern of courtroom harshness predicting past voter turnout– there is no indication that harsher courtrooms are receiving fewer voters. This should give us further confidence that case randomization processes are working as designed.

The second possible interpretation of the results presented in Table A35 is that being assigned to a harsher courtroom between 2008 and 2012 made people (who had voted in 2008) more likely to be purged from the voter file by 2014. I think this interpretation is more plausible than the failure-of-randomization story discussed above, given the other evidence. The contention here is not that election officials would deliberately attempt to purge jailed misdemeanants from the voter file, but that other downstream effects of jail time could make people more likely to drop off the voter file. That is, not voting due to jail time (as described in this paper) could lead to people being placed on the inactive list and becoming more likely to ultimately be purged, and becoming residentially unstable or homeless due to incarceration could make people less likely to respond to the sorts of address-confirmation mail that could keep them on the voter file. This could lead to people who voted in 2008 no longer appearing on the file (and thus appearing not to have voted) by 2014.

I do not have a fully pre-treatment voter file to test for the purging pattern described here, so I take a different approach to exploring this possibility. Rather than going back in time, I go forward. In the next subsection of this SI, I combine the 2012-2016 criminal cases used here with voter data from immediately after the 2016 election, demonstrating that a period with fewer concerns about purging (both because there were fewer large-scale purges of the voter file and because the file was collected nearer to the election date) yields extremely similar point estimates.

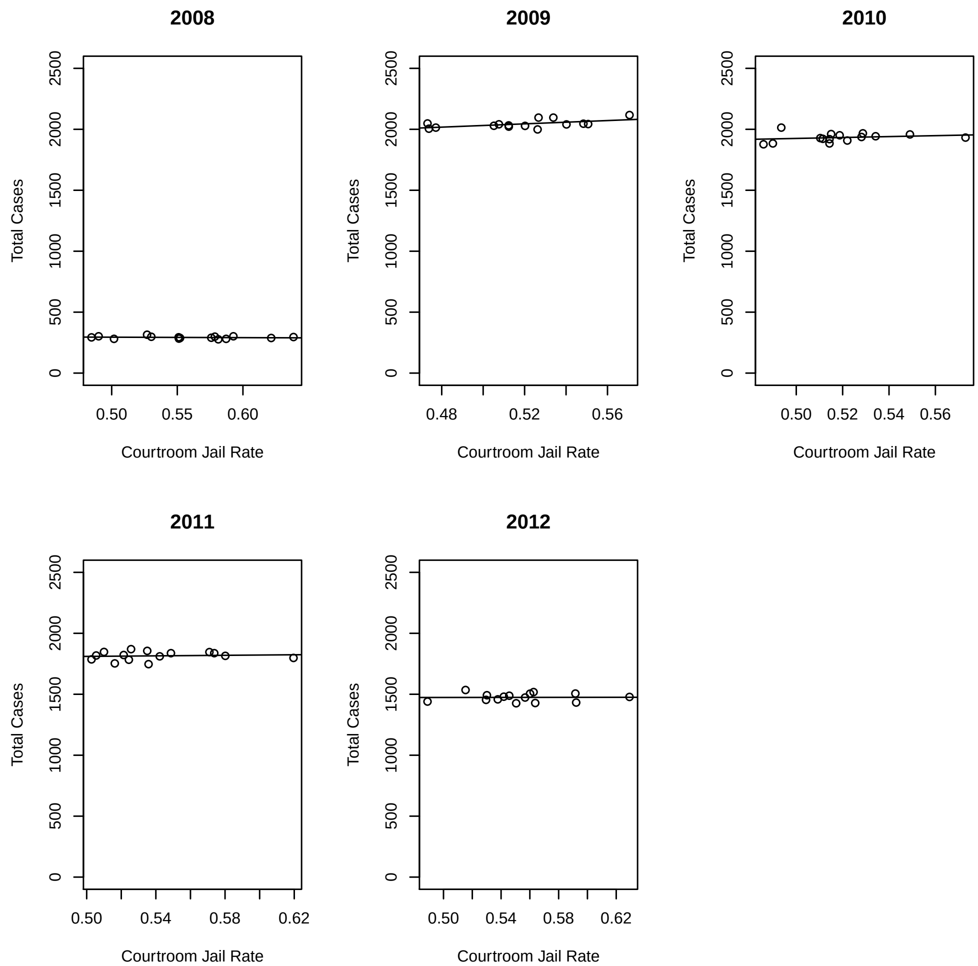


Figure A12: Checking whether harsher courtrooms appear to receive fewer cases (consistent with subversion of random assignment). Note that 2008 and 2012 contain fewer cases than other years because they are incomplete (the dataset focuses on 2008 post-election and 2012 pre-election).

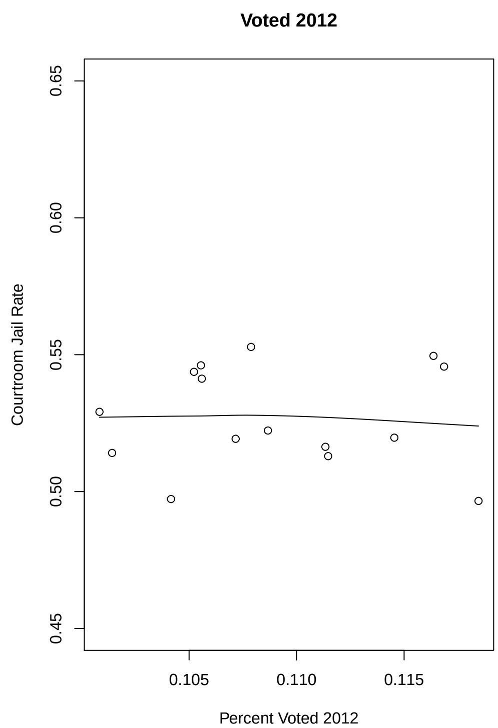


Figure A13: Proportion of each courtroom's caseload that had voted in 2012 (all cases filed from November 2012-2016).

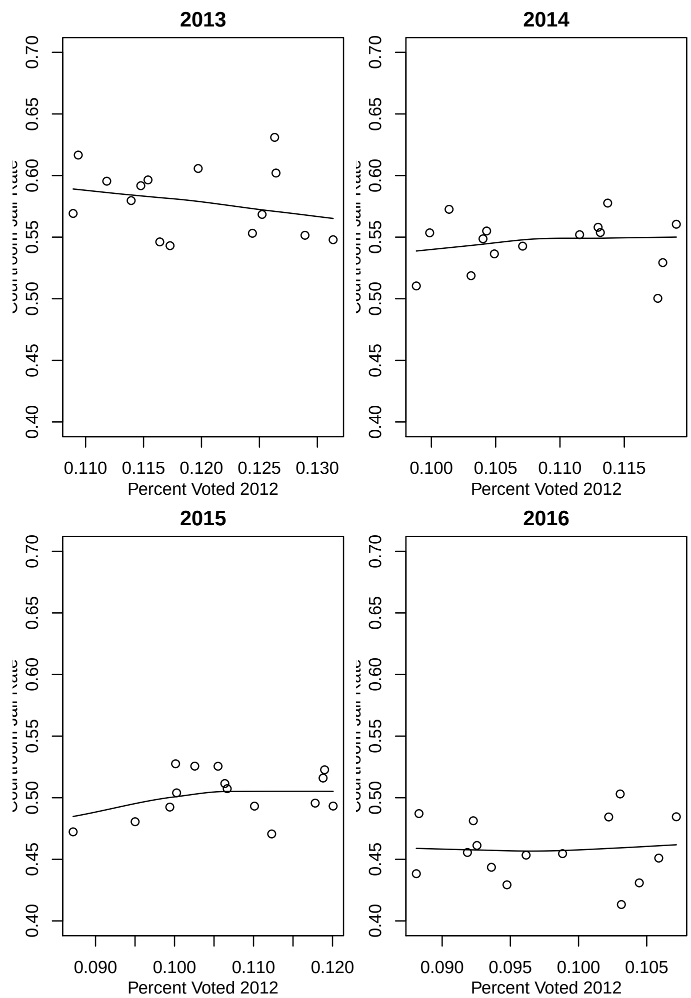

2013
2014
2015
2016
Percent Voted 2012
0.40
0.45
0.50
0.55
0.60
0.65
0.70
0.110
0.115
0.120
0.125
0.130
0.100
0.105
0.110
0.115
0.090
0.100
0.110
0.120
0.090
0.095
0.100
0.105

Figure A14

2 A Different Time Frame

As noted in subsection 7.1 above, there appears to be a logically impossible result in table A35: jail sentences received in 2009-2012 seem to be affecting previous voter turnout in 2008. I suspect that this relationship is due not to a problem with the analysis but a problem with the data used: if people receiving jail sentences were more likely to be removed from the voter file before it was collected in early 2014, that pattern of purging could yield the results shown. However, it is difficult to test for such purging without an earlier version of the voter file. Instead, this section takes a different approach: collecting additional court and voter data from a later period, and demonstrating that rerunning the analysis for a period and dataset with fewer purging concerns yields substantively similar estimates of the effect of jail on voting, and a much more intuitive placebo test result.

I begin with the set of court records used in subsection 7.1; these are all first-time misdemeanor cases from Harris County from between the 2012 and 2016 elections (just like the main paper's data, but for the succeeding four years). To these court records, I merge a copy of Harris County's voter file from 2017. This yields a dataset comparable to the one used for the main analyses, but for the next four-year period. The one shortcoming of this dataset is that it incorporates only Harris County voters, whereas the main dataset used the full state file (allowing me to see voters who were jailed in Harris County but subsequently voted elsewhere); this likely means that there are some missed matches that will tend to attenuate the estimates.

Table A36 presents analyses comparable to the main results shown in the paper, but for the four years following the period of the main analyses. The point estimates for Black defendants are strikingly similar (-12 percentage points, compared to -13 in the main paper). They are marginally significant ($p = .06$), not surprising given the incomplete voter data used and the resulting lower match rate in this dataset, as well as the slightly smaller sample from this period. It is also notable that the estimates for all defendants appear substantially larger than those in the paper, which I attribute to shifts in the composition of arrestees during this time period (the set of defendants appears to be more Black and substantially more Latino than in earlier years).

Table A36: IV estimates: Jail sentences on 2016 voting, by race

	Dependent variable:	
	Voted 2016	
	Black Defendants	All Defendants
	(1)	(2)
jail	-0.118^{*}	-0.144^{***}
	(0.063)	(0.041)
Constant	0.183^{***}	0.195^{***}
	(0.036)	(0.024)
Year dummies	Yes	Yes
First Stage F-Statistic	86.86	204.27
Observations	27,645	94,583
Adjusted R^2	0.011	-0.009
Note:		$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

It bears noting that these similar estimates arise from a dataset with far fewer of the purging concerns of the main dataset, both because there were fewer large-scale (and controversial) purges of the voter file in Harris County between 2012 and 2016, and because the voter file used here was collected in early 2017, almost immediately after the 2016 election, so there was very little time for 2016 nonvoters to be removed. That this dataset yields similar estimates helps to assuage concerns that the main results were somehow driven by data problems. This dataset also allows us to replicate the "pre-jail placebo test" approach from Table A35 above, this time looking at whether jail sentences given out after the 2012 election appeared to have "affected" 2012 voting.

Table A37 presents this placebo test, using a measure of 2012 turnout collected not from the 2017 voter file but from the 2014 file (this is not perfectly post-treatment but does appear to have nearly-complete 2012 turnout, so is unlikely to have any strange patterns from file purges). The null results on this placebo test are reassuring.

Table A37: Placebo IV estimates: Jail sentences on 2012 voting, by race

	Dependent variable:	
	Voted 2012	
	Black Defendants	All Defendants
	(1)	(2)
jail	0.073	−0.032
	(0.075)	(0.042)
Constant	0.149***	0.137***
	(0.043)	(0.025)
Year dummies	Yes	Yes
First Stage F-Statistic	86.86	204.27
Observations	27,645	94,583
Adjusted R^2	−0.031	0.006
Note:		*p<0.1; **p<0.05; ***p<0.01

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