

Misdemeanor Disenfranchisement? The demobilizing effects of brief jail spells on potential voters

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July 2017

Abstract

This paper presents new causal estimates of incarceration’s effect on voting, using administrative data on criminal sentencing and voter turnout. I use the random case assignment process of a major county court system as a source of exogenous variation in the sentencing of misdemeanor cases. Focusing on misdemeanor defendants allows for generalization to a large population, as such cases are extremely common. Among first-time misdemeanor defendants, I find evidence that receiving a short jail sentence decreases voting in the next election by several percentage points. Results differ starkly by race. White defendants show no demobilization, while Black defendants show a turnout decrease of about 13 percentage points due to jail time. Evidence from pre-arrest voter histories suggest that this difference could be due to racial differences in who is arrested. These results paint a picture of large-scale, racially-disparate voter demobilization in the wake of incarceration.

¹Political Science Department, MIT. Contact: arwhi@mit.edu. I thank Matt Blackwell, Ryan Enos, Julie Faller, Claudine Gay, Alan Gerber, Simo Goshev, Jennifer Hochschild, Greg Huber, Connor Huff, Gary King, Christopher Lucas, Marc Meredith, Michael Morse, Noah Nathan, Rob Schub, Anton Strezhnev, Kris-Stella Trump, and the participants of the Harvard Experiments Working Group and the Harvard American Politics Research Workshop, as well as conference and seminar participants at Boston University, Columbia, Dartmouth, NYU, MIT, Princeton, Stanford, SUNY Albany, the Harris School, UCLA, UCSD, University of Rochester, Vanderbilt, and Yale for helpful comments and thoughts. This research has been supported by the Center for American Political Studies and the Radcliffe Institute at Harvard. Replication materials for this project will be available from the author’s dataverse within six months of publication.

1 Introduction

The last few decades have brought historic levels of incarceration in the US. Rising prison and jail populations have been disproportionately drawn from poor and minority neighborhoods, with some cities seeing the emergence of “million dollar blocks” where incarceration is so concentrated that over a million dollars a year is being spent to incarcerate the residents of a single city block. Black men, especially those without high school diplomas or college education, now face incredibly high risks of conviction and incarceration. Of Black men born between 1965 and 1969, for example, nearly 60 percent of those without high school diplomas had spent time in prison by age 30 (Pettit and Western, 2004)

Rising incarceration has wrought major changes in the lives of people who come into contact with the criminal justice system (Soss and Weaver, 2016). Young men change the rhythms of their lives to avoid police encounters or apprehension on warrants; families jump through hoops to visit loved ones in prison; released felons find that they cannot get honest work (Comfort, 2008; Goffman, 2009; Pager, Western and Bonikowski, 2009; Kohler-Hausmann, 2013). Political behavior may also be affected. Recent work finds that interactions with the criminal justice system, and incarceration in particular, cause people to retreat from political participation (Fairdosi, 2009; Weaver and Lerman, 2010, 2014; Testa, 2016). Given the demographics of arrestees, such a retreat could mean that young men of color would be even more underrepresented in the electorate.

This paper brings a causal approach to the question of whether incarceration decreases voter turnout. Relying on random courtroom assignment in a major county court system, I use courtroom variability in sentencing as a source of exogenous variation in jail time. Defendants are randomly assigned to courtrooms, and some courtrooms are more prone to sentencing defendants to jail than others. First-time misdemeanor defendants in Harris County who are sentenced to jail time due to an “unlucky draw” in courtroom assignment are slightly less likely to vote in the next election than their luckier but otherwise comparable peers.

I estimate that jail sentences reduce voting in the subsequent election by about 4 percentage points. However, this overall estimate conceals starkly different effects by race. White defendants show small, non-significant positive treatment effects of jail on voting, while Latino defendants show a decrease in turnout due to jail, and Black defendants’ turnout in the next election drops by an astonishing 13 percentage points. I hypothesize that this is at least partly due to different approaches to arrest and prosecution: Black citizens are much more likely to face scrutiny and arrest, and so

black voters are more likely to be caught up in the legal system (while white arrestees were less likely to vote even before arrest). Vote history data provides some support for this theory: black defendants are much more likely to have voted in the presidential election before their arrest.

This paper’s findings are bolstered by the data sources used and the causal identification provided by random case assignment. Unlike past survey research on this question, this project relies on administrative records for information about both jail sentences and voting, and so is not subject to misreporting or memory lapses. The instrumental variables approach used here produces causal estimates of the effect of jail on voting for an interesting and important subset of the population, misdemeanor defendants who could hypothetically have received some jail time or none depending on the courtroom to which they were assigned.

Focusing on misdemeanor defendants for this analysis has several benefits. The results of this study can be generalized to an exceedingly large pool of people: millions of misdemeanor cases are filed in the US each year, with hundreds of thousands of people receiving short jail sentences. And the results presented here underscore how important even “minor” criminal justice interactions can be (Roberts, 2011). Finally, the focus on misdemeanors allows for a test of demobilization without legal restrictions on voting, as none of the defendants in my analysis will be legally disfranchised due to their convictions.

This paper presents new evidence that incarceration, even for short periods, can drastically reduce future political participation. These results raise normative concerns, especially given the racial makeup of the incarcerated population and the racial differences I find in jail’s demobilizing effects. The nation’s jails are not only sites of policy implementation, but have important effects on future elections and the inclusivity of American democracy.

2 Theory

2.1 Incarceration as a Demobilizing Force

The first goal of this paper is to test whether incarceration reduces voter turnout. Existing studies have proposed mechanisms by which incarceration could deter voters, and in this paper I test whether jail sentences have a negative causal effect on voting. I depart from previous work on the topic by focusing on misdemeanor cases, which are both common and non-legally-disenfranchising.

There are many reasons to expect that incarceration would deter people from vot-

ing. Weaver and Lerman (2010, 2014) describe a mechanism by which people learn to fear and avoid government through criminal justice interactions, and so do not vote (see also Brayne (2014)). This is similar to work on other negative interactions with government, such as applying for welfare (Soss, 1999; Bruch, Ferree and Soss, 2010), and builds on findings that incarceration is associated with lower levels of political efficacy (Fairdosi, 2009). Just as earlier work on policy feedbacks highlighted how government programs could empower and engage people, making them more politically-active, recent work describes how disempowering or punitive government interactions can deter participation.

An even simpler mechanism by which incarceration could prevent voting is through the many costs that incarceration imposes. Even short spells in jail can lead to job loss or major loss of income, loss of housing, and family disruption (Western, 2006). Any of these experiences could also prevent people from voting (Verba, Schlozman and Brady, 1995).

But one of the central challenges of prior research on the topic is that it is difficult to disentangle the effects of incarceration from confounders such as criminal behavior. Many authors have questioned whether people who engage in criminal behavior and are then incarcerated were likely to vote even if they hadn't been jailed, imprisoned, or barred from voting via felon disenfranchisement laws (Haselswerdt, 2009; Miles, 2004; Hjalmarsson and Lopez, 2010; Gerber et al., 2015).² Existing research has attempted to address this question using survey self-reports³ and various matching or time-series approaches, but it has proved difficult to demonstrate that incarceration itself causes lower turnout.

Further, many of the mechanisms by which incarceration is thought to reduce voting involve voluntary actions: people decide to stay home on election day due to their past experiences with government. But in practice, looking at the voting behavior of the previously-incarcerated generally conflates voluntary actions with legal fact: many people are incarcerated for felony convictions, and are ineligible to vote for at least some period of time in most states. In many states, they will be purged from the voter rolls, and so face an additional hurdle to voting. In some states, they will need to apply to be reinstated as voters; in a few, they will most likely remain ineligible for life (The Sentencing Project, 2013).

Focusing on misdemeanor defendants allows me to measure voluntary withdrawal

²Such a concern might be less pressing for misdemeanor cases than for felonies, given how much more widespread these cases are and the failures of due process described by Natapoff (2011).

³Some recent work has used administrative records to measure contact with the criminal justice system (Burch, N.d., 2011; Meredith and Morse, 2015, 2014; Gerber et al., 2015).

from politics, rather than legal restrictions on voting such as felon disenfranchisement laws. But misdemeanor cases are also interesting in their own right, and have been understudied. They are extremely common: although exact national counts of misdemeanor cases are not available, one source estimated that there were 10.5 million misdemeanor prosecutions in 2006 (Boruchowitz, Brink and Dimino, 2009). And although they carry fewer legal and social consequences than felonies, there are still collateral consequences to misdemeanor convictions, as well as the possibility of jail time, probation, and fines (Roberts, 2011; Howell, 2009).

From the existing literature on incarceration and voting, and this understanding of misdemeanor cases, I derive the first hypothesis of this study: jail sentences will render misdemeanor defendants less likely to vote (all else being equal).

2.2 Racial Differences in Incarceration’s Effects

Most existing work on incarceration and voting has focused on the average effect within the population, but there are reasons to expect that effects could differ by race.

Criminal cases (especially misdemeanors) are subject to concerns about racial discrimination at nearly every stage of the process, from policing to arrest to charging to sentencing. Black men, especially those without college education, are disproportionately likely to be arrested, convicted, and incarcerated (Pettit and Western, 2004). There is an ongoing debate about how much of the racial difference in arrest and conviction is due to underlying differences in criminal activity, and how much are driven by racial discrimination. In lower-level crimes, discretionary behavior by police and prosecutors may become more important, and racial bias could more easily come into play (Spohn, 2000; McKenzie, 2009). In drug cases in some jurisdictions, for example, people of color make up a high proportion of defendants despite not using drugs at higher rates than whites (Beckett, Nyrop and Pfingst, 2006; Golub, Johnson and Dunlap, 2007). This is often attributed to greater scrutiny of black neighborhoods by police and discretionary charging behavior by prosecutors.

A sizeable body of academic research, as well as many first-hand accounts in mainstream media and literature, documents black Americans’ exposure to policing and arrest. Qualitative studies have described heavy-handed police behavior in minority neighborhoods (Brunson and Miller, 2006; Rios, 2011), while quantitative studies have analyzed the targeting of black citizens through traffic stops or programs like New York’s “Stop-and-Frisk” (Meehan and Ponder, 2002; Gelman, Fagan and Kiss, 2007; Antonovics and Knight, 2009). As such, we might expect racial differences in defendants’ pre-existing characteristics, as well as their post-release voting behavior.

If arrest patterns differ by race, black defendants could differ from white defendants in their pre-arrest voting habits; black voters could be more likely to be arrested and ultimately demobilized, while white arrestees might not have been likely voters to begin with. Alternatively, black misdemeanor defendants sentenced to jail could experience different treatment in jail than white inmates. Or, black defendants sentenced to jail could interpret the sentence differently, perceiving the court system’s treatment as more unfair than a white defendant in similar circumstances (Fagan and Meares, 2008; Tyler, 2001; Hurwitz and Peffley, 2005; Walker, 2016). Any of these mechanisms could lead to larger effects for black than white defendants.⁴

Because this paper uses administrative records rather than survey responses, I have enough observations to look for racial differences in jail’s effect on voting. I test the hypothesis that black defendants will show more demobilization than white defendants.

3 Data and Methods

3.1 Misdemeanor Case Data

I use a dataset from Harris County, Texas, of first-time misdemeanor defendants whose cases were filed in the Harris County Criminal Courts at Law between November 5, 2008 and November 6, 2012.⁵ This dataset was provided by the Harris County District Clerk’s office. For each defendant, I have identifying information (name, birthdate, address, and unique identification number), some demographic data (sex, race, age), a description of the charges faced (the exact charge, as well as the charge severity), courtroom assignment, and sentencing outcomes (disposition, any fines/probation/jail).⁶ This time window yields a dataset of 113,423 defendants.

Harris County is the third largest county in the US, located in the southeast corner of Texas. It contains the city of Houston, and is home to just over 4 million people. Its misdemeanor court system is, accordingly, large, with 15 courtrooms hearing about 45,000 cases per year.

⁴The prediction is less clear for other racial or ethnic groups. Latinos, for example, have certainly had fraught interactions with police in some places (Rios, 2011). But with lower residential segregation and a somewhat different history of police encounters, Latinos may not consistently face the same kinds of police targeting that could lead to larger effects for Black defendants. Results found in Harris County may not be completely generalizable to other contexts.

⁵I begin with cases filed immediately after the 2008 election and omit records for defendants whose cases were filed on or after the date of the 2012 election for the main analysis; the post-election data is later used for a placebo test.

⁶A few defendants likely have incorrect ages recorded, as evidenced by the extreme minimum and maximum values of the age variable (6 and 92 years old). These outliers represent a small fraction of the overall caseload, and the results are robust to omitting extreme ages.

First-time misdemeanor cases filed with the Harris County District Clerk are randomly assigned to one of fifteen courtrooms by a computer program.⁷ Each courtroom in the misdemeanor court system consists of a single judge and a team of prosecutors at any given time; judges face re-election every four years, while prosecutors are assigned to the courtroom by the District Attorney’s office and can remain in the same courtroom for months or years (Mueller-Smith, 2014). Common case types for these courtrooms include driving while intoxicated, theft, possession of small amounts of marijuana, and certain types of (non-aggravated) assault.

Misdemeanor charges in Texas carry penalties of up to one year in jail, along with the possibility of fines or probation. These cases are generally handled with a minimum of courtroom time, as county courts handle scores of misdemeanor cases per courtroom per day. Jury trials are extremely rare, and most defendants plead guilty (often on the advice of their time-strapped court-appointed attorney).

The Harris County defendants dataset includes information on the verdicts and sentences in each case. For this analysis, I focus on the first case or cases faced by a defendant. For defendants with multiple charges filed the same day, I collapse those observations to calculate whether they received a particular sentencing outcome in *any* of their cases. Cases filed at the same time for the same individual would be heard by the same courtroom.⁸ For cases with deferred adjudication, I ignore anything that happens after the first sentencing decision. If someone is sentenced to probation, for example, and later ends up being sent to jail because they violated that probation agreement, I do not count this as a jail sentence, only as a probation sentence. I also drop eight cases with clearly impossible sentence lengths (over 100 years), which I attribute to data entry errors.⁹

Table 1 presents summary statistics on a range of possible sentencing outcomes. These outcomes are not mutually exclusive: one can receive a jail sentence and be assessed a fine for the same charge. About half of people who face misdemeanor

⁷Defendants with prior convictions, such as those still on probation from a prior case with a given court, can be sent back to their original courtroom. This is a primary reason for focusing on first-time defendants (*RULES OF COURT, Harris County Criminal Courts at Law*, 2013). Based on a conversation with the Harris County District Clerk’s office, I identified first-time defendants using historical county records: any defendants whose unique court ID number appeared in a case filed between 1980 and 2008 were omitted from the dataset. Records were not available for cases filed before 1980, so it is possible that a very few defendants included in this dataset were actually repeat arrestees. However, given the age distribution of the defendants in my dataset, this should be extraordinarily rare.

⁸Results are also robust to dropping defendants with more than one misdemeanor case.

⁹Some other sentences in the dataset appear implausibly long (> 1 year) but could be the result of multiple misdemeanor charges being sentenced at once; results presented below are robust to including or omitting these observations.

charges in Harris County are ultimately sentenced to some jail time. Even including several implausibly long sentences, the mean sentence is under one month. Conditional on receiving some jail time, the median sentence is 10 days.

Table 1: Criminal Sentencing, 2009-2012

Statistic	Mean	St. Dev.
Conviction	0.697	0.459
Fine	0.297	0.457
Probation	0.240	0.427
Jail	0.532	0.499
Total Sentence Length (Days)	23.966	57.998
Sentence > 1year	0.008	0.091
Sentence > 1month	0.198	0.399

3.2 Merging Court Records to Voting Records

In order to examine incarceration’s impact on voting, I needed to measure voter turnout among all first-time defendants. In the main analysis presented here, voter turnout data comes from the Texas voter file.¹⁰

Defendants’ court records were linked to the voter file using defendant/voter names and birthdates. I first merged the files by last name, first initial, and birthdate. Then, I adjudicated “ties” between potential matches using string distance: I calculated how dissimilar the first names were in all possible matches and dropped potential matches that fell below a certain distance threshold. Of remaining potential matches, I retained the one where the first names were most similar.¹¹

The voter registration and turnout rates in the resulting dataset are relatively low, as one would expect for a sample of people who recently faced criminal charges. Roughly a third of first-time defendants with cases between 2009 and 2012 showed up as registered voters after the 2012 election, and about 13 percent of them were marked as having voted in the 2012 general election.¹²

¹⁰The voter file was generously provided by NationBuilder. The file was collected from the state prior to the 2014 election (so it contained turnout history for 2012 and earlier elections for voters registered as of 2014). The Supplementary Information (SI) Section 2.1 presents a comparison between vote turnout totals derived from this file and the Secretary of State’s official reported turnout; the 2012 voter file turnout totals are less than 3% off of the SOS counts.

¹¹For this approach, I used R’s stringdist package, with the “jaro-winkler” option. In Section 2.3 of the SI, I demonstrate that changing the cutoff value does not substantively change the results.

¹²If a defendant was not matched to the voter file, I consider them a 2012 nonvoter. I calculate

Because names and birthdates could be recorded differently in different datasets or shared by multiple people, it is possible that this merge could either under- or over-report the rate of voter registration among previous defendants. An unregistered defendant could be matched to some other person’s voter record (false positives), or a registered defendant could be left unmatched due to name or birthdate errors (false negatives). I follow Meredith and Morse (2014) in conducting a permutation test to check for false positives: I add 35 days to each defendant’s actual birthdate and attempt to merge this permuted dataset to the voter file. Finding many matches for this permuted data would suggest that false matches are common.

When I permute the birthdates of the actual dataset and attempt to match it to the voter file, fewer than 100 (of over 100,000 defendants) match: a match rate of less than one percent. These results suggest that my actual match rate of roughly 1 in 3 of the defendants matching to voter records is unlikely to be driven by incorrect matches.

Assessing the rate of false negatives (missed matches) is more difficult. The fuzzy string matching of first names allows for some small typographical errors across files. However, errors in birthdate or last name, or extreme variation in first names, could certainly result in missed matches. If there were such missed matches, they would likely bias my estimates toward zero, making the results presented in this paper a conservative estimate of the effects of jail on voting.¹³

4 Results

4.1 Preliminary Approach

Before using the instrumental variables (IV) approach of the main analysis, I report the simplest specification: ordinary least squares regression of 2012 voter turnout on having been sentenced to jail in the four years prior. The results of this analysis appear in Table 2. These estimates may be biased: defendants who go to jail are probably different from those who don’t in a number of unobserved ways (Turney, 2013). But they provide a descriptive understanding of the data, and a baseline for comparison with the IV estimates. And these estimates invite further investigation: the negative

turnout, not turnout conditional on registration, for two reasons. First, the difficulty of registering when one’s life has been upset by a jail sentence is one possible mechanism by which jail could reduce voting. Also, I cannot be sure that people who were registered as of 2014 had been registered prior to the 2012 election.

¹³In Section 2.2 of the SI, I explore this point further by deliberately discarding some of the matches from my main dataset. The estimates shrink towards zero and become more uncertain as I discard more and more actual matches.

coefficient on jail in the first column suggests that jail is associated with lower voter turnout in the next election, while the interaction term between Black identity and jail in the third column suggests that that negative relationship is more pronounced for Black defendants.

Table 2: OLS estimates of jail's effect on voting

	<i>Dependent variable:</i>		
	Voted 2012		
	(1)	(2)	(3)
Jail	-0.105* (0.002)	-0.097* (0.002)	-0.080* (0.002)
Voter Birth Year		-0.005* (0.0001)	-0.005* (0.0001)
Black		0.115* (0.002)	0.146* (0.003)
Male		-0.043* (0.002)	-0.043* (0.002)
Jail*Black			-0.060* (0.004)
Constant	0.183* (0.001)	9.464* (0.175)	9.403* (0.174)
Observations	113,415	113,285	113,285
R ²	0.025	0.073	0.074
Adjusted R ²	0.025	0.072	0.074
<i>Note:</i>			*p<0.05

4.2 Main IV Results

Hypothetically, we could measure the effect of incarceration on voting by randomly assigning some people to go to jail and others not, and then observing the different turnout behavior between those two groups. This real-world experiment would not be

ethical for social scientists to run. But the random assignment of cases to courtrooms in Harris County has some things in common with that experiment. Cases are assigned at random to courtrooms that are more or less likely to jail defendants that come before them. Some defendants would always get jail time, and some defendants would have seen their cases dismissed (or been convicted but not sentenced to any jail time) no matter what courtroom assignment they received. But for some subset of those defendants—compliers, in the language of Angrist, Imbens and Rubin (1996)—we can imagine a coin flip: if they are assigned to a “harsher” courtroom, they will receive some jail time, but in a “more lenient” courtroom they would not. The instrumental variables design allows me to capture this random variation in sentencing to measure the effect of jail time on voting for these defendants.

I use courtroom assignment to instrument for incarceration (Kling, 2006; Green and Winik, 2010; Nagin and Snodgrass, 2011; Loeffler, 2013; Mueller-Smith, 2014). In order for this approach to identify the effect of incarceration on voting, the exclusion restriction must hold. In this case, that means that assignment to a particular courtroom cannot affect voting *except through* incarceration. In many ways, this seems reasonable: judges are not in the habit of talking about voting during sentencing, and most defendants will spend very little time in the courtroom for a misdemeanor case. However, one possible concern is that other sentencing decisions besides incarceration (such as probation or fines) could also affect voting, and that courtrooms that give out harsher sentences are also harsher on one of these dimensions. I discuss this concern in Section 4.4.

This IV approach also requires several other assumptions to be met. First, courtroom assignment (the instrument) must be truly exogenous, not determined by some defendant or case characteristics. And there must be sufficient courtroom-level sentencing variation: if all courtrooms sentenced defendants in the same way, being randomly assigned to a particular courtroom wouldn’t change one’s probability of a jail sentence.

Figure 1 summarizes various defendant and case characteristics by courtroom as a first step towards 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 1 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.¹⁴ 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.

My main IV results instrument for jail (whether a defendant is sentenced to jail or not) using courtrooms' incarceration propensity. The instrument is constructed as the courtroom's mean incarceration rate over any given year: how many of the people who came before that courtroom ended up sentenced to jail?¹⁵ For example, a person who faced charges in 2011 and was assigned to courtroom 7 would receive a value of .50, as courtroom 7 sentenced half of defendants to jail that year. In practice, the incarceration instrument calculated yearly ranges from .47 to .63, demonstrating that courtrooms display substantial variation in their sentencing decisions.

I recalculate the instruments over time because of concerns that courtroom changes could render a courtroom more or less prone to incarceration. The monotonicity assumption for this IV setup requires that being assigned to a "harsher" courtroom (one with a higher overall incarceration rate) makes one more likely to be sentenced to jail. If courtrooms' incarceration propensities shift over time, the monotonicity assumption could be violated. For example, Courtroom 3 incarcerated 52% of defendants with cases filed in 2011, while in 2012 it incarcerated only 49% of defendants. Courtroom 6 changed from a 51% incarceration rate in 2011 to 56% in 2012. Looking over this entire period, Courtroom 6 looks like a harsher courtroom. But in cases filed in 2011, defendants were actually slightly more likely to be jailed if they were assigned to Courtroom 3. Recalculating the instruments over time allows courtrooms to change.¹⁶

Results Table 3 presents 2-stage least squares (2SLS) results from this approach. The first column presents the first-stage regression of jail sentences onto the courtroom-jail-rate instrument, demonstrating that the instrument is relevant. The first-stage F-statistic is large, suggesting that concerns about weak instruments are not merited (Stock, Wright and Yogo, 2002). The second column presents the 2SLS estimates of jail's effect on voting, estimated for all defendants. The negative coefficient suggests that a jail sentence decreases one's probability of voting in the 2012 election by 4

¹⁴Table A9 in the Supporting Information presents each courtroom's values of these variables over this time period. In Section 3 of the SI, I also test more formally for patterns suggesting non-random assignment to courtrooms. I find no indication of randomization failures.

¹⁵With few instruments in play, this approach is analogous to simply using courtroom indicator variables as instruments, interacting them with filing-year indicators. See section 4.2.4 of the SI for a demonstration.

¹⁶These changes in courtroom behavior could be due to personnel changes (new judges or prosecutors entering a courtroom) or to within-person behavioral shifts.

Pre-Assignment Characteristics And Sentencing By Courtroom, Suggesting Random Assignment

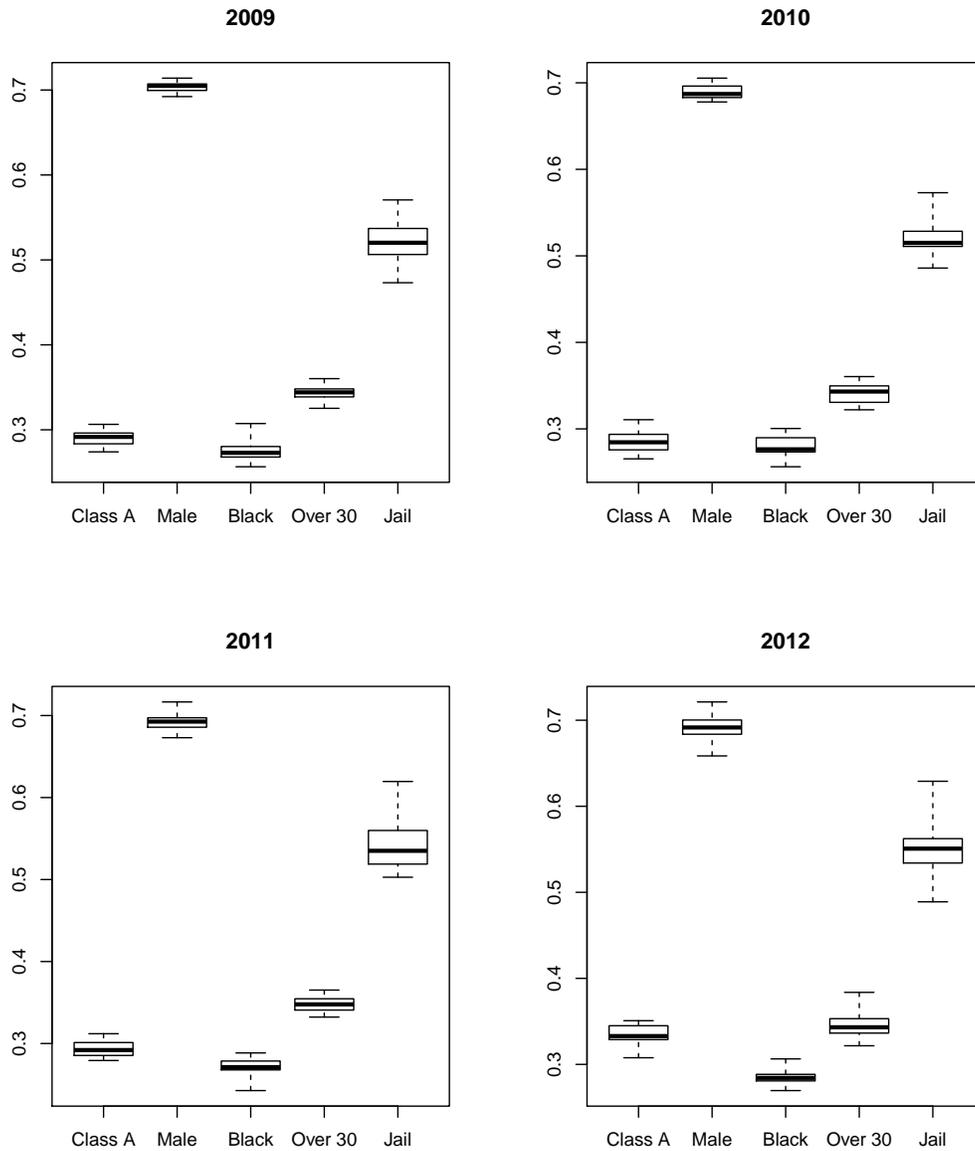


Figure 1: Box plot of the full range of several pre-treatment variables, as well as jail sentences, for the 15 county courtrooms. The box edges represent the 25th and 75th percentiles and the middle line the median value of the variable; the whiskers extend to the most extreme value of that variable 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.

percentage points, though it is imprecisely estimated in this simple specification.¹⁷ This estimate provides some evidence for the first hypothesis, that jail sentences reduce voter turnout in the subsequent election, but I cannot rule out the possibility that jail has no effect on turnout.

Table 3: Jail Sentences on 2012 Voting

	<i>Dependent variable:</i>	
	Jail (1)	Voted 2012 (2)
Court Jail Average (Yr)	1.000* (0.051)	
Jail		-0.045 (0.034)
Constant	-0.0001 (0.029)	0.142* (0.019)
Year dummies	Yes	Yes
Observations	113,415	113,415
Adjusted R ²	0.004	0.017
F Statistic	98.033* (df = 5; 113409)	
<i>Note:</i>		*p<0.05

Next, I split the sample to explore whether the deterrent effect of jail differs by race.¹⁸ Figure 2 presents 2SLS estimates of the effect of jail on voting for black and white defendants separately (table in SI Section 1). The estimates are strikingly different. The treatment effect of jail on voting for black defendants is substantively large and statistically significant, about 13 percentage points' decrease in voter turnout. The estimate for white defendants is small (one tenth of a percentage point) and statistically indistinguishable from zero. The SI (Section 4.1.8) presents a model including

¹⁷In the Supporting Information (Tables A25-A26), I present more precise estimates, using courtroom-harshness estimates calculated within-race or within-charge-type, but here I present a simple specification both for exposition and to avoid dropping observations with missing or rare case types or racial identities.

¹⁸Race, unlike the few other personal characteristics available from court records, is an obvious choice for subgroup analysis. Existing literature has established African-Americans' high levels of criminal justice contact and system mistrust, both of which could lead to different treatment effects from jail sentencing.

IV estimates by Race: Jail on 2012 Voting

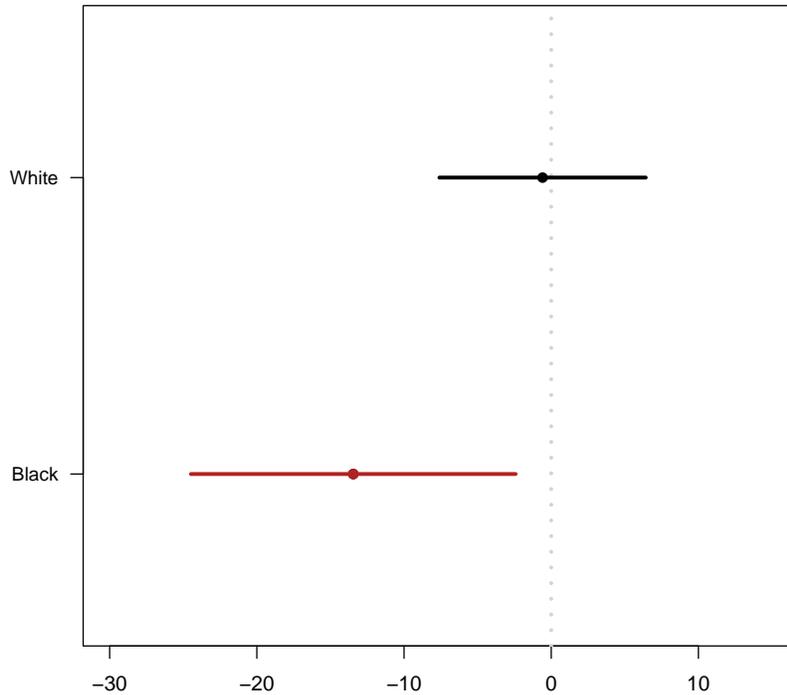


Figure 2: Jail’s effect on voter turnout (2SLS estimates), by race of defendant. A coefficient of -0.13 indicates a turnout decrease of 13 percentage points (among compliers).

both groups of defendants and interacting race with jail to test whether these effects are significantly different from one another, and they are statistically distinguishable. Black defendants and white defendants respond to jail sentences differently. One possible interpretation of these racial differences is as evidence of overpolicing and black criminalization, explored further in Section 4.3.

Harris County’s court database includes a “defendant race” variable that only indicates whether a defendant is Black, White, Asian, Native American, uncategorized, or “other”. This database classifies Hispanic defendants as white, so the above analysis discussing “white” defendants includes both Hispanic and Anglo defendants. However, in Section 5.2 of the SI, I discuss an approach using surname matching to identify Hispanic defendants. Hispanic defendants (as identified by surname, undoubtedly with some errors) do seem to show a negative effect of jail on voting, but I cannot say for certain that there is a difference between Hispanic and Anglo defendants.

In the SI, I also present results from a longer time range (Section 5.1). They provide preliminary evidence that these effects may persist beyond a single election cycle.

Interpretation These estimates are not of the average treatment effect of jail on voting for all defendants; instead, they represent a local average treatment effect (LATE) for “compliers,” defendants who could have been jailed or not depending on courtroom assignment.

This local effect is interesting from a policy standpoint. The people who are being jailed and ultimately deterred from voting in this study are not repeat violent offenders who clearly must be incarcerated for public safety. They are first-time misdemeanants who may face some jail time, or may not, because a computer randomly assigned them to face one judge or another. That judges’ exercise of sentencing discretion in these minor cases has such large downstream effects on voting is both surprising and alarming. However, the fact that this study’s estimates are drawn from a specific pool of compliers does not mean that they cannot be generalized to a broader set of defendants. If compliers are similar to other defendants on characteristics that shape voting propensity, and they experience jail and the court system as equally arbitrary and degrading, the effects measured here should be generalizable to many other defendants.¹⁹ I discuss the generalizability of these results further in Section 4.5.

These are causal effects of jail on voting, but they do not identify the precise mechanism by which this demobilization occurs. I interpret these results as a measure of individuals choosing to withdraw from political participation after being jailed. This could happen because their time in jail taught them to avoid government and decreased their sense of personal efficacy, as suggested by Bruch, Ferree and Soss (2010), Weaver and Lerman (2014) and others.

A related mechanism is resource-related: rather than convincing voters to avoid government, it could produce many practical barriers to voting. We know that incarceration (even in short stints) can lead to job loss, family disruption, and housing and economic challenges. And although misdemeanor convictions carry fewer legal sanctions than felonies (for example, they don’t bar people from voting), they still can carry collateral consequences like restricted access to public benefits or occupational licenses.²⁰ It is possible that individuals still believe in the value of voting (contrary

¹⁹One notable feature of this design is that defendants are unlikely to know whether or not they are compliers. The criminal justice system is opaque, especially to first-time defendants, and few compliers will even know about random courtroom assignment, much less think (any more than other defendants do) that they would have fared better or worse in another courtroom.

²⁰For state-by-state data on such consequences, see the American Bar Association’s project at <http://www.abacollateralconsequences.org/>

to the theory of Weaver and Lerman (2014)), but that they find it too difficult to vote when they are dealing with other problems (Verba, Schlozman and Brady, 1995).

Either mechanism would speak to the lasting impact of jail on people’s lives and political engagement, even in the absence of legal restrictions on voting. But the two mechanisms (jail socialization and resource constraints) are slightly different, and I cannot thoroughly distinguish between them with the data at hand. In Section 1.3 of the SI, I present some preliminary findings that suggest the mechanisms may reach beyond economic disruption. I use tax appraisal data to identify a subset of defendants who own their own homes, and find that these defendants actually show a much *larger* demobilizing effect of jail than the main sample. Given that these defendants should be partially shielded from some of the most extreme and immediate economic outcomes of jail (such as eviction and homelessness), that they show an even larger effect of jail on voting suggests that political socialization may be at work (Weaver and Lerman, 2014). However, the relatively small size of the sample here (6,000 homeowners) means that these analyses should be approached with caution.

There are two other possible mechanisms that I find less likely. First, would-be voters might still want to vote, but mistakenly think they were ineligible. For this to explain the above results, they would need to know that an arrest did not make them ineligible, but think that jail time served for a misdemeanor barred them from voting.²¹ Prior research has shown that there is substantial misinformation among ex-felons about voting eligibility, and that notifying them of their right to vote can boost turnout in some cases (Meredith and Morse, 2015). But Drucker and Barreras (2005)’s survey of adults with a history of criminal justice involvement did not show substantially *more* misinformation around past jail terms than around past arrests. It is possible that misinformation is in play, but I do not think it is likely to drive all of the results presented here.

Another apparent possibility is that would-be voters were still in jail at the time of the election, but this is unlikely. The vast majority of these defendants would have been free at the time of the 2012 election regardless of the sentence they received, as most misdemeanor jail sentences in this data last a week or two.²² Dropping all cases filed in 2012 yields similar results, and rules out this possibility for nearly all

²¹Simply believing that an arrest *or* jail time prevents voting would not produce this pattern of results, since everyone in my sample was arrested and so would be equally deterred. To create the difference we see between arrestees sent to jail and those not sent to jail, there must be additional misinformation about jail time (or at least convictions) preventing voting.

²²Technically, misdemeanants can still vote even if jailed at the time of the election, and the county jail’s handbook for inmates instructs those wanting to vote to contact the county clerk. In practice, it would be surprising if jail inmates managed to request and return an absentee ballot.

defendants.

A related mechanism would be re-arrest: if people sentenced to jail become more likely to be re-arrested, the next election might find them in jail due to another set of charges, or barred from voting due to a new felony conviction. This does not appear to be the case in my data. In additional analysis in Section 1.4 of the SI, I instrument for felony convictions or additional jail time that occurs after the first case but before the 2012 election (using the same IV setup as in the main analysis with these new outcome variables). I find no evidence that people sentenced to jail in their first cases become significantly more likely to be convicted of a felony or sentenced to jail in a second case prior to the 2012 election. This is somewhat contrary to existing work that has found recidivism effects from jail sentences, but I believe this is due both to the nature of my sample (first-time defendants, not all criminal defendants) and the brief time frame of my analysis (defendants charged in 2011, for example, would have had little time to serve a jail sentence, be released, and then be re-arrested prior to the 2012 election).²³

4.3 Voter History

The results presented in the previous section show very different effects of jail on black and white defendants. This could be due to differing arrest patterns by race, with black citizens more likely to face arrest than white ones. If black people face elevated risks of arrest across the board, then black *voters* could be more likely to get swept into the criminal justice system. It is possible that zealous policing tactics in black neighborhoods mean that there are a higher proportion of regular voters among black defendants than white defendants. In this section, I look for evidence of such a difference.

I use data on voting in prior elections, as recorded in the Texas voter file. As noted above, this file has complete voter turnout data for all registrants as of the 2012 election. But prior election data may be less complete, as voters could have voted in those earlier elections but then been purged from the voter file for various reasons (such as inactivity or death). This file provides a conservative measure of turnout in 2008, in the sense that anyone who is reported as voting in 2008 almost certainly did, but some people who did vote may not appear as voters in the data. Barring complex patterns of voter purging (such as white voters being disproportionately likely to be dropped from the voter file after having voted in 2008)²⁴, this data provides a useful test of

²³Relatively few of the defendants in my sample receive further jail sentences (12%) or felony convictions (5%) by the 2012 election.

²⁴In fact, a 2012 lawsuit filed by LULAC (the League of United Latin American Citizens) claimed that the county was disproportionately purging minority voters from the voting rolls. So this file

whether black defendants are more likely to have been voters before their arrest.²⁵

Table 4: Differences in pre-arrest voter turnout by race

	<i>Dependent variable:</i>	
	Turnout 2008	Turnout 2008
Black	0.084* (0.002)	0.090* (0.002)
Male		-0.042* (0.002)
Over 30		0.101* (0.002)
Charge severity		0.013* (0.002)
Constant	0.085* (0.001)	0.006 (0.012)
Observations	113,415	113,274
R ²	0.014	0.042
Adjusted R ²	0.014	0.042
<i>Note:</i>		*p<0.05

Table 4 presents descriptive regression results that allow us to compare previous voter turnout across race. Black defendants are more likely to have voted in 2008, before their arrests, than white defendants. The estimated difference, of about 8 percentage points, is substantial: in the full dataset, 11 % of defendants had voted in 2008. Black defendants are nearly twice as likely as white defendants to have voted prior to their arrest. This difference underscores the racial differences in exposure to the criminal justice system that have been pointed out by Pettit and Western (2004) and others. White people are less likely to be arrested overall, and arrests are confined mainly to

may provide an even more conservative measure of past voting for black voters than for white ones.

²⁵Due to the possibility of voter file purges, I do not include this measure of 2008 voter turnout in my main analyses, because I consider it to be a post-treatment variable that could introduce bias. In the SI (Section 4.1.5), I present IV estimates of jail’s effect on voting for the subset of defendants who are recorded as having voted in 2008, and as expected, these voters show a very large (though noisy) demobilization effect. However, these results should be interpreted with extreme caution due to sample size and the aforementioned measurement/post-treatment issue.

people who do not regularly vote. But with more police presence and higher scrutiny of black neighborhoods, black people are more likely to be arrested. With such high arrest rates, the pool of arrestees includes not only socially-isolated, civically-detached people, but also more politically-engaged people. Black voters get arrested and charged, and so it is possible for them to be demobilized by jail.

This table does not show deliberate discrimination on the part of police or prosecutors; I do not have data to assess why arrest rates differ. And this section’s analysis does not have the same causal interpretation as the previous section. The IV estimates of jail’s effect on voting (for both black and white defendants) are well-identified causal effects. The evidence presented here about *why* the effects differ is observational and does not rule out other possible mechanisms. However, it is consistent with a narrative in which targeted policing brings many black defendants into court, including some voters (so they can be deterred), while lower arrest rates among whites mean that the white defendant pool rarely includes voters (so there’s no demobilization, because the people jailed were unlikely to vote anyway). These differences in vote history persist even when adjusting for other defendant characteristics, such as age, gender, and charge severity.

4.4 Robustness Checks

Placebo Test: post-election sentencing 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 results into question. I do not; these results are reported fully in Section 1.2 of the SI.

Different specifications The SI also reports results from a range of different specifications: limiting the sample (e.g. to omit people with multiple cases, or to limit to men only, or to voting-age defendants), constructing the instrument differently (constructing instruments within-race or within-charge type, constructing leave-one-out means, using courtroom dummies rather than courtroom means, or not coarsening sentence length). The estimates and standard errors vary across these specifications, but all remain consistent with the main findings of this paper: that (Black) misdemeanor defendants are deterred from voting by jail sentences.

Non-focal treatments One possible threat to inference here is the violation of the exclusion restriction presented by other courtroom “treatments.” The estimates presented above 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, the above estimates could be biased (Mueller-Smith, 2014).

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.²⁶

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, with a correlation of .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 above 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 oppor-

²⁶Courtroom experiences could theoretically matter, though time spent in the courtroom is brief and confusing for most defendants: there is unlikely to be 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.

tunities 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 testament 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 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 somewhat noisy estimates, because jail and conviction are highly correlated. However, the point estimates (presented in SI Table A30) are consistent with the main estimates presented here: jail still matters a great deal for voter turnout.

Next, I subset the data to focus on courtrooms with similar conviction rates but variation in jail sentencing tendencies. In a set of analyses reported in the SI (Section 5.3), 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 5 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.²⁷ Figure 3 presents first differences based on the reduced form. 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.

4.5 Substantive Importance

The main results point to a large decrease in voter turnout for black defendants sentenced to jail. The question remains of how substantively important this effect is, and how many voters could actually be deterred by jail terms. This question has two components: first, how does the Local Average Treatment Effect (LATE) estimated for compliers in this sample generalize to the rest of the sample, or to defendants outside Harris County? And second, how many first-time misdemeanor defendants, in Harris County and nationwide, could face demobilization from jail sentencing?

Generalizing LATE I begin by characterizing compliers—people whose jail sentence depends on their courtroom assignment—using the few pre-treatment characteristics available from court records. In an analysis in the SI (Section 4), I dichotomize the courtroom instrument (split it at the median value into high-jail-rate and low-jail-rate courtrooms) and present some of the characteristics of compliers relative to the whole sample. Black compliers are somewhat more likely to be female, are younger than the average defendant, and are less likely to have been charged with a class A (more serious) misdemeanor. They are not any more or less likely to have a record of voting in 2008.

Then, I reweight the complier population to resemble the entire population of black defendants (Aronow and Carnegie, 2013).²⁸ With some distributional assumptions,

²⁷The 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%.

²⁸I use the following background covariates to reweight the sample: age, gender, and charge severity (class A or class B misdemeanor).

Table 5: Reduced-form: Courtroom assignment on voting

	<i>Dependent variable:</i>	
	All Defendants (1)	Voted 2012 Black Defendants (2)
Courtroom Instrument	-0.045 (0.034)	
Courtroom Instrument		-0.134* (0.057)
Constant	0.142* (0.020)	0.263* (0.031)
Year dummies	Yes	Yes
Observations	113,415	31,524
F Statistic	1.797 (df = 5; 113409)	2.016 (df = 5; 31518)

Note:

*p<0.05

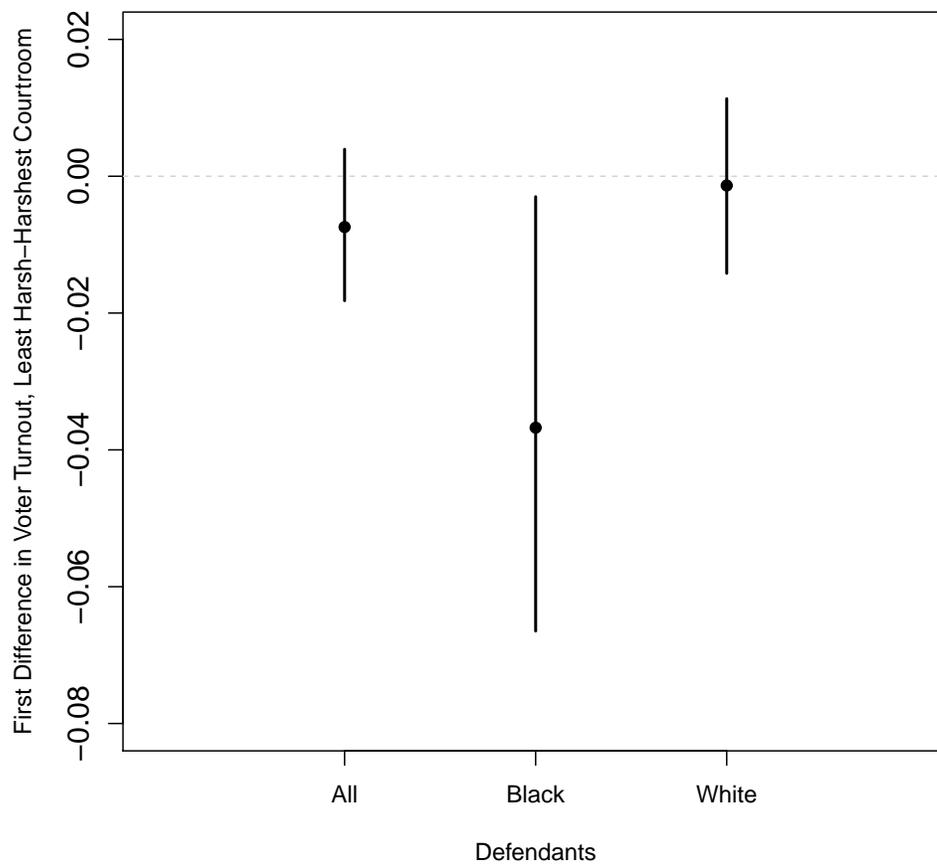


Figure 3: Simulated first differences based on the reduced form: these show the predicted change in voter turnout for defendants if they were to be moved from the courtroom with the lowest to the highest incarceration tendency.

along with ignorability of compliance (the idea that the treatment effect for a given covariate profile should be the same across compliers and non-compliers), this approach should return an Average Treatment Effect (ATE) for the entire sample, rather than a complier-specific LATE. This analysis is presented in full in the SI (Section 4). Using this approach, I estimate an ATE of $-.28$ for black defendants in Harris County (substantially larger than the complier-specific LATE estimated in Section 4.2). This cuts against concerns that the “complier” population is more likely to be deterred from voting than the full population of defendants.

Another approach to generalizing the LATE here would be to find an entirely different identification strategy, either by finding another instrument with a different complier population, or by using a different design entirely. In Section 5.3 of the SI, I present a different set of estimates based on case timing (comparisons of people arrested before and after the election), and find treatment effects that are comparable in magnitude to the local estimates presented here. In particular, white defendants do not show large or significant demobilizing effects from jail, as I find in the main analyses, while black defendants show large, significant demobilization (on the order of ten percentage points). That a completely different research design finds an average treatment effect that is so similar to the LATE estimated here should bolster our confidence in the generalizability of these results.

On the question of how Harris County defendants differ from those in other jurisdictions, there is very little concrete data available. There is no national source of data on misdemeanor defendants and jail sentencing (Boruchowitz, Brink and Dimino, 2009). Qualitative reports suggest that the experience of going to jail in Harris County is not atypical for local jails anywhere in the country, though the Harris County jail system is particularly large.

Eligible Population If we think the LATE estimated from the Harris County sample (or the reweighted ATE presented above) can be reasonably applied beyond compliers, the question remains: how many people could be affected? I examine this question first for Harris County, then make some nationwide estimates.

In Harris County, the sample of black defendants consists of about 30,000 black first-time misdemeanor defendants whose cases were filed between the 2008 and 2012 election, of whom just over 16,000 were sentenced to jail. If the LATE estimated above holds for all of these defendants, then roughly 2,100 black defendants were deterred from voting in 2012, due to jail sentences received in the four years prior. This is a significant number of voters for local elections, even in a large county. In the November 2012 election, for example, two of the judgeships in the Harris Civil Courts at Law

(different from the Criminal Courts at Law discussed in this paper) were on the ballot. These were both tight elections; the Republican candidate for Courtroom 1 won the race by under 4,000 votes. If we assume that most black voters in Harris County vote for Democrats, the decision of several thousand black voters to stay home could sway tight elections like this one. And even without reversing election outcomes, the withdrawal of thousands of black voters from the electorate could lead to different patterns of representation and policy outcomes (Griffin and Newman, 2005).

It is harder to know how many people could be affected by misdemeanor jail sentences nationally. There is little national data on misdemeanor charges or jail sentencing, so I present a back-of-the-envelope calculation based on two approaches: one using jail admissions data from the Bureau of Justice Statistics, and another extrapolating from Harris County data. The assumptions made are discussed in the SI (Section 5.5).

Estimates of the affected population (black first-time misdemeanor defendants sent to jail during this presidential election cycle) range from 765,000 to 1.2 million depending on the data used. If they faced the same rates of demobilization estimated in the main analysis (a drop of 13 percentage points), this would mean somewhere between 100,000 and 156,000 black Americans stayed home from the polls in the 2012 election due to jail sentences served during that election cycle.²⁹ These are loosely-estimated quantities, but they suggest that a staggering number of black potential voters stayed home in 2012 due to misdemeanor jail sentences.

5 Conclusion

Jail sentences arising from misdemeanor cases decrease voter turnout in the next election, especially for black defendants. The effects presented in this paper are strikingly large, and have a causal interpretation. Further, jail sentences disproportionately deter black voters, suggesting that seemingly minor criminal cases could have major racial implications for democratic representation.

Although this analytic setup depends on a criminal court system with random assignment to courtrooms, the results generalize beyond Texas' county courts. In court systems with only one judge or without random assignment, we can imagine that small differences in a judge's mood or calendar could lead to sentencing variation that deters voting. And even in the absence of such arbitrary variation—even in cases where multiple judges would likely agree on the jail sentence imposed—the result that jail deters voting could well hold. The “compliers” in this IV analysis differ from the

²⁹For comparison, this is similar in size to the entire black voting population of Washington, DC.

general defendant population in that they fell into a realm of sentencing uncertainty (though they themselves might not know this). But to the extent they are similar to other defendants on characteristics that drive voting propensity, the effects identified for these compliers should hold for many other defendants as well. In this case, the impact on voter turnout could be massive: misdemeanor cases are incredibly common across the country, and hundreds of thousands of short jail terms are given out each year.

As noted above, the jail sentences distributed to misdemeanor defendants in Harris County are usually quite short: most range from a few days to several weeks. That these sentences shape voter turnout in the next election is quite striking. That the effect may persist through multiple election cycles implies that such sentences could have immense effects on voter turnout. If some voters simply drop out of the electorate for years after receiving such a sentence, then the political effects of sentencing could build up over time.

Finally, jail's disproportionate effect on black turnout has major implications for the makeup of the electorate. African-Americans are already disproportionately represented in the criminal justice system. A larger estimated effect for black defendants (in addition to their being more likely to face such jail terms) means that demobilization will be even more pronounced for black voters. In areas with extremely high levels of criminal justice contact, this could lead to major drops in voter turnout. As noted above, the persistence of jail's effect on voting mean that misdemeanor sentencing could be producing lower black turnout in such areas for years to come.

Further research is still warranted on how defendants view these misdemeanor jail sentences, and how short stints in local jail differ from longer prison terms in their political effects. Another avenue of investigation is the possible "spillovers" of such sentences: do defendants' family members or neighbors also reduce their political participation in the wake of short jail sentences (Lee, Porter and Comfort, 2013; Sugie, 2015)?

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