Voter Behavior in the Wake of Punitive Policies

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Abstract

Millions of people in the US have direct experience with the machinery of immigration enforcement or criminal courts, and millions more have seen family members, friends, or neighbors face these experiences. What do these experiences mean for political behavior in the United States? Do these proximate observers decide that government is a dangerous and capricious force to be avoided, and withdraw from political participation entirely? Or is there sometimes a mobilization response, where some people organize to push back against what they see as unjust government actions?

This is an important policy feedback story. Large-scale punitive policies could either "lock themselves in" via community disengagement, or hasten their own demise by fueling political responses. The three papers of this dissertation examine policies at varying distances (people living in an area where the policy is introduced, those directly affected, and those living with people directly affected), and with different timeframes and geographic coverage. The results of these papers, and the approach of using administrative datasets and finding causal leverage from "natural experiments," point us toward a new understanding of policy feedbacks.

In the first paper, I find that Latino voters living in counties where a new deportation program was introduced before the 2010 election became more likely to vote. This effect seems driven not by personal experience seeing deportation activities, but by activists mobilizing voters in affected counties.

In the second paper, I use random courtroom assignment to measure the causal effect of short jail sentences (from misdemeanor cases) on voting. I find that even short jail sentences can deter people from voting in the next election, with particularly large effects among black

voters.

In the third paper, I find that the household members of incarcerated people also become several percentage points less likely to vote. This finding is particularly striking given the narrow scope of the effect measured: this is only the additional effect of seeing a household member *jailed* for a short period, among a set of people that have already seen their household member arrested and charged with a crime.

Contents

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Introduction

Large numbers of people are targeted for punitive government actions in the US. Deportations have reached record levels in the past few years, with hundreds of thousands of people deported each year. Incarceration rates have plateaued, but are still higher in the US than at any other point in history and far surpass those of other developed countries. Millions of people have direct experience with the machinery of immigration enforcement or criminal courts, and millions more have seen family members, friends, or neighbors face these experiences.

What do these experiences mean for political behavior in the United States? Do these proximate observers decide that government is a dangerous and capricious force to be avoided, and withdraw from political participation entirely (Soss 1999; Weaver and Lerman 2010)? Or is there sometimes a mobilization response, where some people organize to push back against what they see as unjust government actions (Moore 1978; Klandermans 1997)?

This question matters not only because of the scale of punitive systems in the United States, but also the concentration of these experiences: many Americans live in neighborhoods where arrests are rare, while others experience police searches and arrests. For many, immigration enforcement is a distant, abstract concept, while others live in daily fear that non-citizen family members could be deported. These punitive experiences are disproportionately concentrated in low-income neighborhoods and those with many minority residents (Western 2006; Burch 2013).

There is a crucial policy feedback story here. If punitive policies cause a political and civic withdrawal among those most affected, it may become harder and harder for these communities to mount a political response. Policies may become more entrenched as the people they target drop out of the political system. Conversely, if punitive policies mobilize people in opposition, they may not last as long as otherwise expected. Do large-scale punitive policies “lock themselves in" via community disengagement, or do they instead sow the seeds of their own demise by fueling political responses?

A general answer to this question probably does not exist, and research in this area has found mixed results (Bowler, Nicholson, and Segura 2006; Walker 2014; Burch 2013). Policy effects likely differ based on a few dimensions: who is targeted by the policy, and how many political resources do they have already? How close are people to the experience (presumably seeing a neighbor or acquaintance arrested is different from seeing a husband arrested, both in the strength of reaction evoked and the personal costs taken on)? How are the policies themselves implemented, and how successfully do they stigmatize the people targeted? How individualized or collective does the punishment feel, and do people who see others affected think that they might be next?

This dissertation looks at different policy arenas and at different levels of analysis, as a first step towards a broader theory of political response. The three papers that follow look at two different policies (incarceration for misdemeanor crimes, and one component of federal immigration enforcement), at different distances (people living in an area where the policy is introduced, those directly affected, and those living with people directly affected), and with different timeframes and geographic coverage.

The first paper of this project measures voter turnout in response to immigration enforcement. I examine how Latino voters respond to the implementation of a stricter immigration enforcement regime in their county. I use the staggered rollout of the federal Secure Communities program, which increased immigration-status checks in local jails and ultimately increased deportations in affected counties, as a source of exogenous variation in immigration enforcement. I then examine the voter turnout of Latino citizens, who are by definition not the targets of deportation policies (though they are likely to know or be related to people who could be deported). Still, I find a sizeable short-term effect in turnout, with Latino citizens actually becoming more likely to turn out to vote in the immediate wake of program implementation. This (possibly counterintuitive) effect seems to be driven not by personal experience with deportation activities, but by activists mobilizing to get out the vote in affected counties. At a distance and when dealing with the short-term threat of such a policy change, people can indeed be mobilized by punitive policies.

The second paper of the dissertation measures individual-level political effects of incarceration, focusing on individuals who are sent to jail for short periods in misdemeanor criminal cases. By linking administrative databases on criminal sentencing and voter registration and turnout from Harris County, Texas, I observe people's court experiences and their later voting behavior. I use the random assignment of cases to different courtrooms (with varying degrees of sentencing harshness) as a source of exogenous variation in jail sentencing (Kling 2006; Green and Winik 2010; Mueller-Smith 2014). This allows me to identify the causal effect of jail on voter turnout, avoiding concerns about omitted variables. I find that even short jail sentences can deter people from voting in the next election. The effects differ starkly by race, with essentially no demobilization of white defendants and a large (about 13 percentage points) drop in voter turnout among black defendants sentenced to jail. I find observational evidence that at least part of this difference in the effect of jail is driven by racial differences in who is arrested. In this case, punitive contact with government can lead to political withdrawal on a large scale.

The third paper focuses on household-level spillover effects of incarceration. Using criminal court records from Harris County, along with the geographic information provided in the voter file, I construct a dataset of household-level experiences with the criminal justice system, following voters whose household members have faced misdemeanor charges. As in the second paper, random assignment to criminal courtrooms adds a source of exogenous variation to these sentences, allowing me to determine whether exposure to harsher sentences (meted out to one's family or roommates) makes one less likely to turn out to vote in the next election. I find that people who saw a household member incarcerated for a short period (for a misdemeanor, not a felony crime) become several percentage points less likely to vote in the next election. This finding is particularly striking given the narrow scope of the effect measured: this is only the additional effect of seeing a household member *jailed* for a short period, among a set of people that have already seen their household member arrested and charged with a crime.

The second and third sections of the dissertation tell a story of political withdrawal, while the first paper finds evidence of political mobilization (both voting and activist activity) in the wake of punitive policies. This raises more questions about the types of policies examined and the time frame of the studies, but also helps us begin to determine the bounds of punitive policies' political effects. First, the project reinforces the idea that the effects of punitive policies depend on context, and can run in either direction. But it also allows us to theorize about when these policies will mobilize. Taken together, these results suggest that some distance is needed from the policy before the mobilizing effects of threat begin to outweigh the personal costs of the policy. And they point to the role of activists in structuring political responses. Future work will examine other policy realms and other possible spillover effects.

The contributions of this project are threefold. First, it examines a classic question of American politics–why do some people participate while others don't?–from a different angle, taking into account punitive experiences with government. I am able to do this through the use of previously unavailable or unmanageable administrative data. This project uses novel sources of data on both participation and government interactions. Large individual-level datasets on turnout allow us to know more than ever before about the characteristics, relationships, and geographic locations of voters, while public records from some states trace "treatments" such as incarceration and deportation. These new approaches allow me to answer questions about political experiences that were previously unobservable, and to use natural variation in these experiences to find causal effects.

Finally, this project is timely not only because of the current availability of data, but because of the current policy climate in the United States. The experiences I study (deportation, arrests, incarceration) are taking place at historically high levels. If they shape the political behavior of not only the people directly targeted, but also their families, friends, and neighbors, the total political impact of these policies could be enormous.

1 When Threat Mobilizes: Immigration Enforcement and Latino Voter Turnout

1.1 Introduction

The United States has deported over 360,000 people each year since 2008. Research from sociology, law, public health, and other disciplines suggests that these deportations affect the lives of the families, friends, and neighbors deportees leave behind in numerous ways. This paper seeks to test whether deportations, or the policies that permit them, also affect the *political* lives of citizens who live near potential deportees.

It may seem counterintuitive to study the effects of deportation policy on voters, who by definition are citizens and cannot be deported. However, many Latino citizens live in families or communities with mixed immigration status, so voters could still view enforcement as a threat.[1] A recent Pew survey found that one in five Latino registered voters (20%) knew someone who had been deported or detained in the past year. Further, a majority of Latino registered voters surveyed disapproved of the Obama administration's deportation policies (Lopez, Gonzalez-Barrera, and Motel 2011). This suggests that a large number of potential voters are being impacted by enforcement policies with which they disagree. For voters in families or neighborhoods that include undocumented residents, the threat or actual experience of seeing their family members, friends, or neighbors face detention or

[1] Latino citizens are not the only ones who could be exposed to fears of deportation second-hand. But it seems like a fairly common experience for Latinos, and focusing on a smaller group rather than all voters makes it easier to see small changes in turnout. Further, Latinos (including registered voters) report highly unfavorable views of deportation, which should make the immigration enforcement "treatment" more straightforward for this group of voters than for others with more mixed views (Lopez, Gonzalez-Barrera, and Motel 2011).

deportation could affect their political behavior. Further, activist mobilization in the wake of policy changes could turn out voters who are not personally aware of them. However, there is little research linking this experience to voter turnout.[2]

This paper seeks to measure the short-term impacts of stricter immigration enforcement measures on Latino voter turnout, using variation in the implementation of the Secure Communities program. I show that this program, which ultimately led to large increases in deportations and removals in counties where it was implemented, also immediately increased Latino voter turnout in treated jurisdictions by several percentage points. I supplement the simplest approach – comparing turnout changes in treated places to those in untreated places – with a quasi-experimental approach that takes advantage of exogenous variation in the timing of program rollout. When I restrict the analysis only to jurisdictions that were not enrolled in the Secure Communities program as of the 2010 general election, as well as those jurisdictions that were enrolled by a state decision rather than any local willingness to participate in the program, the effect remains: Secure Communities enrollment increases Latino voter turnout by several percentage points.

My design isolates the short-term effects of enrollment in Secure Communities, focusing on jurisdictions that were enrolled in the program only a few months before the 2010 election. This limits the direct effects of the program, as very few people would face deportation before the election. Instead, this design allows me to capture just the "threat" effects of Secure Communities, such as voters' response to hearing about program implementation or activists' responses to enrollment. I present additional survey evidence that Latino voters in "treated" jurisdictions (those enrolled in Secure Communities by their state) were more likely to be personally asked to vote, and to report turning out to vote, than Latinos in untreated places.

This result contributes to our understanding of how groups can be mobilized in response

[2]There is some work linking policy and treatment of Latinos to voter behavior, such as Bowler, Nicholson, and Segura (2006) and Barreto and Woods (2005). But my contention is that immigration policies could be shaping political behavior even in the absence of political rhetoric about one party's hostility toward Latinos, simply because of actual government actions.

to threat, demonstrating that political responses can arise even when voters themselves are not the targets of threatening policy. Immigration policy not only has unintended consequences, but has "second-hand" effects on people who were not actually targeted by the policy. These effects appear to be at least partly driven by activist mobilization such as volunteer get-out-the-vote efforts. Although long-term implementation of deportation policies could also have demobilizing effects, there is a reservoir of Latino political power for activists to draw on in the face of such programs.[3]

This finding also adds to our knowledge about immigration enforcement specifically, by demonstrating that this enforcement has political implications. These results would seem to suggest that politicians stake out strict immigration stances at their own peril. But there is an interesting paradox at play: the unprecedented levels of deportation discussed in this paper, and much of the expansion of programs such as Secure Communities, have mainly taken place under a Democratic administration. Many new Latino voters are casting votes for Democrats. The partisan dynamics of immigration enforcement are not straightforward, and this paper raises questions of how immigration policy debates and a growing Latino citizen population will reshape the electoral landscape in the years to come.

1.2 Background

1.2.1 Immigration Enforcement as a "treatment"

Over two million people have been removed from the US since 2008, many of them under the auspices of relatively new police-driven programs such as Secure Communities and 287(g) agreements (Kohli, Markowitz, and Chavez 2011). These federal programs have created, or at least exacerbated, a sense among undocumented residents that they are at risk of detention or deportation whenever they go out in public, and that police officers are now looking for excuses to stop and possibly arrest them (Capps 2011). If these programs

[3] Having family members face deportation, for example, could mean that voters have less time, energy, or money available for electoral activities. Further, having negative interactions with an uncaring and bureaucratic government could turn off voters (Soss 1999; Bruch, Ferree, and Soss 2010).

have raised the profile of immigration enforcement actions, greatly increased distrust of government in targeted communities, and created a sense of unfair deportation (of people with families in the US and no history of criminal behavior, for example), then they should have larger effects on political behavior than earlier enforcement actions.

Over the last two decades, immigration enforcement has become more interior-focused, removing many people who are not near any borders and have lived in the U.S. for many years (Waters and Simes 2013). These people are more likely to have established family and community ties in the U.S. than recent migrants, and their removal is more likely to affect the political behavior of citizens. Removals have also drastically increased overall: in 1986, there were 24,592 removals, while by 1996 there were 69,680. In 2006, there were 280,974 removals, and in all years since 2007 that number has exceeded 300,000 (US. Department of Homeland Security 2012). Some of these represent repeated deportations of the same individuals, but it still appears that many people are being deported from an increasingly broad geographic area.

One of the programs that have fueled this huge increase in deportations, and the shift from border- to interior-focused enforcement, is the Secure Communities program.[4] Under Secure Communities agreements, the fingerprints of people arrested by local law enforcement are shared with ICE (Immigration and Customs Enforcement) and checked against their immigration database. If the arrestee is "removable" (does not have legal status or has a criminal record that includes crimes for which even legal residents are deportable) ICE can then decide whether to issue a detainer and begin removal proceedings (Kohli, Markowitz, and Chavez 2011). This approach means that ICE checks the immigration status of many more people than before. ICE will be able to begin removal procedures for people who previously would have gone unnoticed by agents, such as those that are arrested but are not ultimately imprisoned in the state or federal prisons where agents had been checking inmates' status. The Secure Communities program was first piloted in several major cities in

[4]Another such program was the 287(g) program, which trained and deputized local police to perform immigration enforcement duties. Having such a program in place neither prevented nor guaranteed Secure Communities implementation in a city or county.

2008, and has since been rapidly expanded to include most of the country. It was expected to cover the entire country by the end of 2013 (Hampton 2012).

The Secure Communities program is expected to have several relevant effects on communities where it is implemented. Most obviously, it will lead to more immigration detention and more deportations in the long term. But other things are likely to happen in the immediate wake of program implementation: the local media (particularly Spanish-language outlets) may report on the program's implementation, and word may also spread through informal social networks (Hagan, Rodriguez, and Castro 2011). Immigration activists may also publicize the program as a threat to the community, and local churches and civic organizations can provide sites for this publicity, as has happened in the wake of other immigration crackdowns (Hagan, Rodriguez, and Castro 2011). In some places, activists have organized to oppose the program, forming national networks of protesters and holding meetings, rallies, and conferences (Strunk and Leitner 2013). It is worth considering this entire "treatment" when discussing mechanisms by which the program could affect turnout. This paper focuses on a relatively short window of time between Secure Communities enrollment and the 2010 election, during which the program is unlikely to have led to a large number of completed (or even begun) deportations. Therefore, the main impact of the policy during this time frame will be due to other components of the policy, such as public awareness of enrollment and activist responses to the program.

1.2.2 Immigration enforcement and voter turnout

The literature on Latino voter behavior and turnout contains several results that might predict an increase in turnout after the introduction of a program like Secure Communities, although there is little work on the effects of immigration enforcement in particular. Several studies of Latino turnout in response to threatening policy environments, in particular, have found that contentious policy proposals can lead to changes in political behavior. Barreto and Woods (2005) examines voter turnout (among registered Latino voters) in Los Angeles after several years of policy proposals and hostile public discourse targeting Latinos, finding that voter turnout rose over this period. Bowler, Nicholson, and Segura (2006) argues that ethnically-divisive politics in California during the 1990's also led to changes in partisanship for both Latino and Anglo voters. The finding that non-Latino voters were affected by policies (such as ending affirmative action and bilingual education) that didn't directly target them foreshadows this paper's finding of Latino citizens responding to a policy that targets only noncitizens.[5] This paper builds on prior work by looking at a new federal immigration policy, using broader geographic data, and focusing on short-term policy effects in the absence of heated rhetoric.

Other related studies focus on first-generation immigrants. Pantoja, Ramirez, and Segura (2001) suggests that Latino immigrants who naturalized in a state context of threat (California in the 1990's) were more likely to vote. The naturalization process–and the idea of naturalizing in response to threat–cannot explain this study's results, as the time frame is too short for immigrants to have responded to policy changes by naturalizing and voting. However, these results are consistent with the idea that people could be mobilized to vote by threatening government action. Similarly, Ramakrishnan (2005) finds that state contexts of threat (measured by discussion of anti-immigrant measures) are associated with higher self-reported turnout by naturalized immigrants of all backgrounds. Because I focus on public records of voting, I cannot address the question of whether first-generation Latino immigrants drive the turnout effect I find among all Latino voters; I do not have information on people's place of birth. But recently-naturalized immigrants could be more responsive to some of the mechanisms discussed below, and might drive the effects I find. This is especially likely in the realm of immigration policy: Branton (2007) notes that first-generation immigrants prefer less restrictive immigration policies than other Latinos, so they may be more likely to object to the Secure Communities program. Further, Branton et al. (2015) found that responses to the 2006 immigrant rights protests varied by generational status, with first-generation immigrants the most affected. Any such mobilization among

[5] Other related work focuses on the mobilizing effects of threat on other ethnic groups, such as Cho, Gimpel, and Wu (2006)'s discussion of high-SES Arab-Americans' political mobilization after 9/11.

first-generation immigrants could vary by citizenship status or national origin (Fraga et al. 2012), as well as political attachments in their country of origin (Wals 2011, 2013).

The studies just described suggest that Secure Communities could increase aggregate voter turnout. There are several mechanisms by which individual voters could be induced to turn out. First, they might respond to the policy of their own accord, either because they see deportations taking place (unlikely in this study), or because they hear about the policy's implementation. Alternatively (or additionally), voters might be mobilized by activist groups working in response to the policy.

At an individual level, voters could view Secure Communities' implementation as a threat for a variety of reasons, and respond by turning out to vote. Social psychological theories of protest suggest that the permeability of identity group boundaries is an important determinant of protest behavior (Klandermans 1997). If Latino citizens feel they are being "lumped in" with undocumented Latinos by the Secure Communities program, they could become more likely to identify with potential deportees. They might become more likely to turn out to vote in hopes of changing policy. Indeed, there is some evidence that Latino citizens fear being painted with a broad brush by immigration policy. In the wake of Arizona's passage of the immigrant-targeting law SB1070, a survey of Latino registered voters in Arizona by the firm Latino Decisions found that 85% of respondents expected that police would use their power under the law to stop or question legal immigrants or U.S. citizens as well as undocumented immigrants.[6] Under Secure Communities, people in some areas have been deported after arrests for relatively minor traffic violations, leading activists to claim that they were being pulled over for "driving while Latino" (Ordonez 2011). If Latino citizens feel that the government is singling out Latinos for punitive treatment, that facet of their identity may become more salient, making them more responsive to mobilization efforts by Latino political groups (or simply more likely to vote, as noted by Stokes (2003)). [7]

[6]SB1070's requirement that police determine the immigration status of anyone arrested or detained is broader than the Secure Communities program, but Latino voters could feel targeted nonetheless.

[7]This could shape their vote choice as well as their turnout, which is beyond the scope of this paper. See Bowler, Nicholson, and Segura (2006) for consideration of partisanship in the face of anti-Latino policies.

A related consideration is whether Latino voters feel “devalued" by the implementation of Secure Communities. Pérez (2015a) draws on social identity theory to predict that elite rhetoric that devalues a racial or ethnic minority group can drive high-identifying members of that group to take political action and push back against the threat to their group. Like the proposed mechanisms of Barreto and Woods (2005) and Bowler, Nicholson, and Segura (2006), Pérez (2015a) predicts that elite rhetoric will threaten and mobilize certain potential voters.[8] In this research design focused on short-term effects, there is relatively little elite rhetoric: most national- and state-level politicians were quiet about Secure Communities until after the 2010 election, and a national debate about the program flared up only in 2011 when some states tried to opt out of the program. Most people talking about the program in 2010 were either federal employees tasked with implementing it, or local immigration activists seeking to prevent its implementation. However, I posit that even in the absence of elite rhetoric, such a program could politicize Latino identity. The program itself may be seen as discriminatory against Latinos, with voters fearing that the police will target Latinos to arrest, fingerprint, and potentially deport. As such, knowledge of the Secure Communities program could be enough to increase the salience of Latino identity (Armenta and Hunt 2009) and potentially increase participatory behavior (Cronin et al. 2012).

All of the individual-level mechanisms just described require that potential voters are aware of SC implementation, either from directly observing deportation processes or hearing about the policy. Another set of possible mechanisms centers on the mobilization done by activists in the wake of the policy's implementation.

The communities in my sample contained a lot of immigration-related organizing potential, both at the grassroots and national levels. Locally, Zepeda-Millan (2014) gives one example of immigrant organizers opposing threatening legislation, describing how members of a soccer club in Fort Myers, Florida became immigration activists during the spring of 2006. Meanwhile, a number of national and regional organizations have begun to focus on political mobilization around immigration issues in recent years (Cordero-Guzman et al.

[8] Also see Pérez (2015b) for a discussion of how elite rhetoric shapes political trust among Latinos.

2008). Groups such as the Hispanic Federation and United We Dream have worked with local activists in a number of states (including Florida and Virginia) to encourage a variety of activities, including voter drives.[9] In the "treated" states in my sample, groups with an interest in immigration issues, like Virginia New Majority and Democracia Ahora, worked during the 2010 election to mobilize Latino voters.

It is possible that local or state-level activist groups focused on immigrant rights became more active in voter mobilization after SC's implementation in their area, either because they thought that voting was more important, because they had more active members, or because some national group reached out to them in the wake of the policy's implementation. This mechanism does not necessarily depend on individual voters' knowledge of the Secure Communities policy: the turnout effects of voter mobilization efforts such as personal contact have been demonstrated, particularly in the case of Latinos reaching out to mobilize their coethnics (Shaw, de la Garza, and Lee 2000; Ramirez 2007; Barreto and Nuno 2009; Bedolla and Michelson 2012). Latino voters could have turned out more in the wake of SC implementation simply because they were more likely to be asked to do so. Section 1.5 explores the role of activist mobilization.

1.3 Data and Methodological Approach

Methodological approach

The Secure Communities program was first implemented voluntarily in several pilot cities beginning in 2008, and then in other jurisdictions mainly along the southwestern border of the US. Then, ICE sought to gradually expand the program across the country, still focusing on jurisdictions that were willing to voluntarily sign up for the program. This is clearly a source of selection bias: if I simply examined differences in turnout between places with and without the Secure Communities program, my estimates of the causal effect of the program

[9]These efforts join the GOTV activities of groups focused solely on Latino civic engagement, such as Mi Familia Vota.

could be biased because places that volunteered to take part in the program might differ in unobservable ways from other places.[10]

However, some jurisdictions received the program without selecting into it. Besides local law enforcement agencies, ICE also negotiated with state law enforcement agencies to try to implement the program across large swaths of the country. Some state-level agencies signed memoranda of agreement (MOAs) with the ICE. Depending on the structure of the state's law enforcement bodies and databases, some of these MOAs brought all jurisdictions within the state into the program at the same time, without any affirmative action on the part of those jurisdictions. This meant that anyone booked into the county jail in the affected places would have their fingerprints checked against the ICE's database, without the county government having taken any action to make this happen.

These MOAs mean that some jurisdictions were treated (had the Secure Communities program implemented within their borders) by the time of the 2010 general election without having selected into treatment.[11] Other jurisdictions in states without such MOAs, who also took no action to enroll in Secure Communities, were left unenrolled. Comparing the "reluctant enrollees" to non-enrolled jurisdictions allows me to find an unbiased estimate of the causal effect of the SC program on Latino turnout for this subset of jurisdictions. I omit from this analysis any jurisdictions that seem to have voluntarily enrolled in the program without state intervention. The remaining number of treated and untreated units appears in Table 1.3.[12]

[10]As an example: some places might select into the program *as a response to* growing Latino turnout rates or the expectation of future turnout increases, perhaps because existing political elites felt threatened by growing Latino political power. If this were the case, a simple comparison of turnout rates in treated and untreated places could show a positive "treatment" effect on turnout even if the Secure Communities program did nothing.

[11]I focus here on the 2010 general election because it was the only federal election for which this research design is possible. At the time of the 2008 presidential election, only a handful of jurisdictions had been enrolled in the program as a pilot. By the 2012 presidential election, nearly the entire country was enrolled. Only in 2010 was there useful variation in enrollment.

[12]I also omit about 120 jurisdictions nationwide for which there is not reliable turnout data, due to a combination of incomplete population estimates and missing or unreliable vote data from Catalist. About 80 of these jurisdictions are dropped due to implausible Latino turnout estimates when the two data sources are combined (i.e., over 100%); the results presented are robust to simply including these places and their

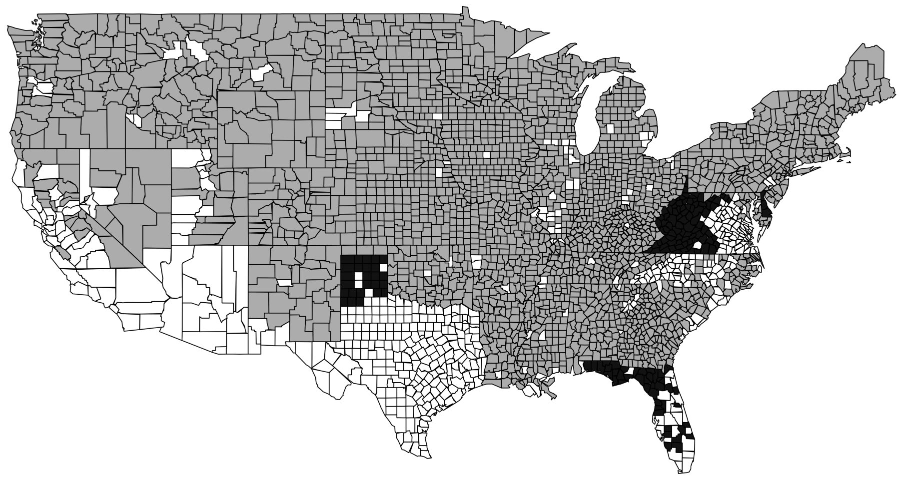

Figure 1.1: Jurisdictions considered treated (black), untreated (gray), and omitted (white) for the main analysis.

I operationalize the treatment of "reluctant enrollment" as follows: for units in the states that opted for universal enrollment (Delaware, Florida, Virginia, Texas, West Virginia), I count units as reluctantly treated if they are in the very last block of jurisdictions to be enrolled in the Secure Communities program. For example, of Florida's 67 counties, 43 of them were enrolled in the Secure Communities program on June 22, 2010, shortly after the state signed an agreement with ICE. The other Florida counties had already enrolled in the program beginning in 2009, and so are excluded from this analysis due to concerns that they selected into the program and might systematically differ from non-enrollees. In states that did not enroll all jurisdictions in the program ("non-treated" states), I also omit all jurisdictions that voluntarily enrolled in the program by the time of the 2010 general election. Figure 1.3 shows treated, untreated, and omitted jurisdictions.

I run a simple difference-in-differences analysis, which compares the 2006-2010 change in

estimated turnout. This is a very small proportion of all units in the analysis, and represents places with extremely small Latino populations.

voter turnout in reluctantly-enrolled jurisdictions to the change in non-enrolled jurisdictions. This requires a parallel trends assumption: if the treated units had not been treated, their Latino voter turnout rates would have followed the same trend as the untreated units actually showed. Thus any difference in the time trends of the two groups is taken to be the treatment effect. However, the identification assumption does not require perfect equivalence between groups: this does not assert that treated and untreated units looked exactly the same before treatment, or that they would have had the same *levels* of Latino turnout absent treatment, but simply that their *trends* over time would be the same. I use available data to test this assumption in an appendix.

This approach allows for a clear causal estimate of the effect of SC enrollment on Latino turnout, but it also restricts the set of places for which the estimate is valid: I am estimating a Local Average Treatment Effect for the places in my sample. However, these places are not a small or unimportant part of the overall picture: my sample contains treated units from states with large Latino populations and ongoing immigration debates, such as Texas and Florida. Table 2 compares the units in my restricted sample to the entire country. The places included are indeed smaller and less dense on average, but they still contain notable Latino populations.

Finally, it is worth noting that all of the "reluctant enrollees" in the sample were enrolled in the program during the summer and fall of 2010. Only a few months elapsed between their enrollment in SC and the 2010 election. As discussed in the literature review, there are reasons to expect that the program could have other impacts over the longer term, as residents observe actual deportations. However, this approach allows me to isolate the short-term political impacts of enrollment, capturing the immediate response of activists and voters in the few months after the program was announced.

Data Sources

Information on the timing of Secure Communities implementation in over 3,000 jurisdictions is drawn from ICE records.[13] I have also gathered information on the date that state officials signed MOAs (memoranda of agreement) with ICE officials.

Estimated Latino vote counts for general elections from 2006-2010 are drawn from the Catalist database. Catalist, LLC is a data vendor that collects voter records from each state and maintains a database of nearly 200 million registration records. They merge state voter files with other publicly-available information and commercial information (from advertising databases) to create individual-level records of people's vote histories and other characteristics. They then use name matching, age, consumer information, and census block demographic data to impute each voter's racial/ethnic background in states that do not record race in the voter file (Fraga n.d.). Their database has reliable vote history data from 2004 onward.

These are vote counts, but in order to calculate voter turnout rates, I need a denominator as well: for this, I use CVAP (citizen voting-age population) estimates of Latino eligible voters from the American Community Survey. Using CVAP estimates for each election year allows me to calculate Latino voter turnout as a percentage of the total number of eligible Latino voters in an area, not just the percent of registered voters that turn out.[14] This is important because the effect could operate through previously-unregistered people being mobilized to register and vote.

1.4 Results

I first present observational results from the entire country, without dropping jurisdictions that may have selected into Secure Communities. Table 1.3 shows the results of an OLS

[13]For the purpose of Secure Communities implementation, "jurisdictions" are generally counties, but in some states they may also include county-equivalents, such as the independent cities of Virginia.

[14]Using ACS data provides intercensal estimates, so population estimates can change across the two election years.

regression of 2006-2010 change in Latino turnout onto enrollment in Secure Communities (by the time of the 2010 election) and a set of election dummies. These dummies indicate whether there was a senatorial or gubernatorial race on the ballot in the jurisdiction during either of these election years, as these high-profile elections are expected to boost turnout in midterm elections (Smith 2001). This first-cut analysis suggests that enrollment in the Secure Communities program as of the 2010 election led to a greater increase in turnout from 2006-2010 than would otherwise have been expected. Even in this basic model, Secure Communities appears to increase voter turnout by about 1.1 percentage points.

Next, I restrict the dataset as discussed above, dropping jurisdictions that selected into the Secure Communities program. The main analysis is conducted on this smaller dataset, estimating a local average treatment effect of the program for these jurisdictions.

The Secure Communities program's implementation resembles a cluster-randomized natural experiment. The treatment is assigned at the state level, not at the individual counties. So treating each county as an independent unit in the analysis would seriously understate the standard errors of the estimates and make the results look more significant than they truly are (Bertrand, Duflo, and Mullainathan 2004). I analyze the data in a more conservative way: I cluster standard errors at the state level, and also run a hierarchical model that allows the intercepts and SC treatment effects to vary by state. Both approaches yield substantively similar and statistically significant results.

Table 1.4 presents both approaches. The first two columns show estimates from a simple OLS model with robust clustered standard errors. Column 1 displays the simplest specification, regressing the 2006-2010 change in turnout rates onto the treatment variable. Column 2 includes dummy variables for the electoral calendar: whether there was a senatorial or gubernatorial election occurring in each cluster in a given year. Voter turnout varies depending on what races are at the top of the ballot, and states have different schedules for senatorial and gubernatorial elections, so leaving these out makes the parallel-trends assumption about turnout over time somewhat less tenable.

Another approach is to allow the intercept and slope estimates to vary by state.[15] Columns 3 and 4 of Table 1.4 present the fixed effects from hierarchical linear models with varying intercepts and slopes, and just varying intercepts, respectively.

Figure 1.4 plots the treatment coefficients from both approaches. In both cases, I estimate that the implementation of the Secure Communities program increased Latino turnout in the treated counties by 2-3 percentage points. This is a sizeable effect. Turnout has a possible range of 0 (none of the eligible Latino voters turned out) to 100% (all eligible voters turned out). The average 2006 Latino turnout rate for all counties in the dataset was 15%: that is, 15% of Latino voting-age citizens turned out to vote.[16] So a turnout increase of 2.4 percentage points in the treated counties (relative to the untreated ones) represents a large jump in turnout. This increase in turnout is comparable to the treatment effect of receiving three pieces of direct mail encouraging one to vote in the classic turnout experiment reported in Gerber and Green (2000).

1.4.1 Stability of results

These results are consistent under various model specifications and data subsets.[17] The coefficient estimates from the model presented in column 4 of Table 1.4 remain quite similar when I drop jurisdictions with very small numbers of Latino residents (all those below 207 eligible voters, the median in the dataset), and are statistically significant at $p < 0.05$. Lower-population jurisdictions may have less reliable population and vote estimates, making the main estimates noisier.

I also restrict the dataset to a smaller set of states where the treatment counterfactual

[15] One other approach that might otherwise be desirable, adding in state fixed effects, is not possible in this study because there is no within-state variation in treatment in the dataset.

[16] This may seem quite low. Note that this is based on all eligible voters, not just those who have registered. It is also a midterm election, and Latino turnout has been observed to be quite low during midterm elections (Cassel 2002). Validated vote studies that are not prone to the over-reporting problems of survey self-reports find low Latino turnout in both presidential and midterm elections. (Shaw, de la Garza, and Lee 2000; Cassel 2002)

[17] Regression tables available upon request.

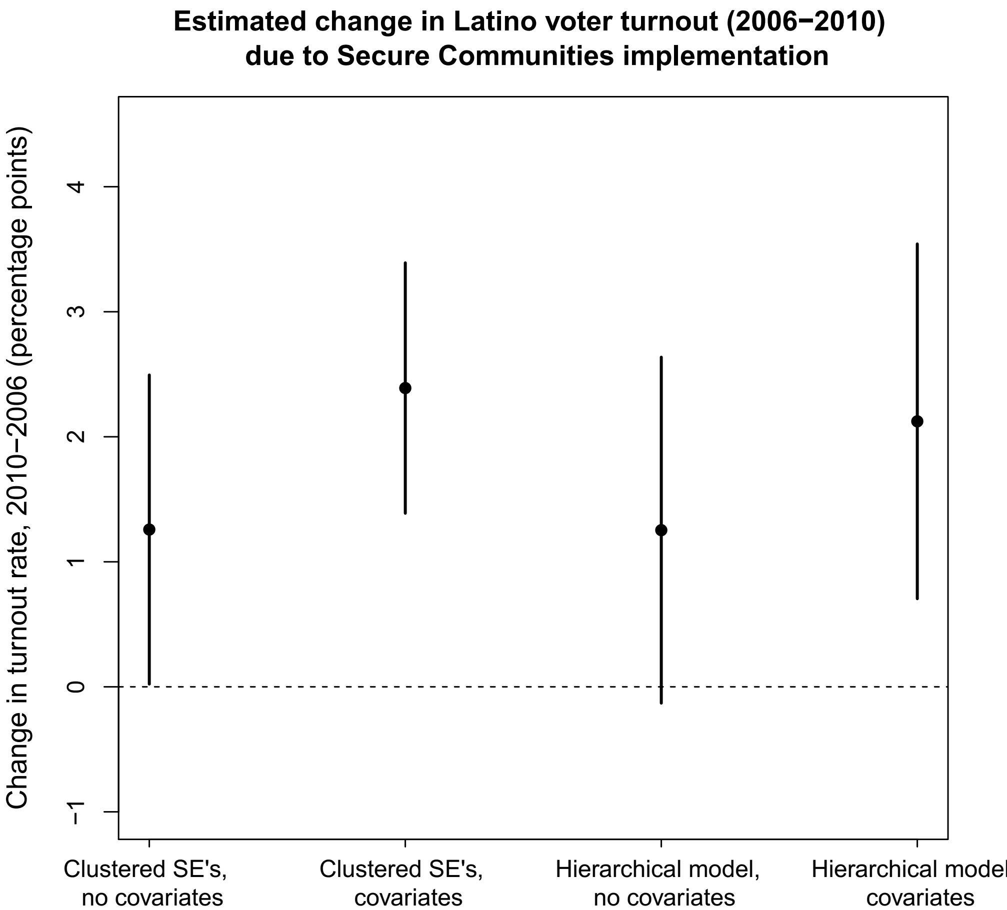

Figure 1.2: Estimates of SC treatment effect. “Covariates" indicates the inclusion of indicator variables for whether the cluster had senatorial or gubernatorial races on the ballot in 2006 or 2010. Lines represent 95% confidence intervals.

is more conservative. Perhaps some of the untreated places in the dataset could never have been treated due to some unobservable differences in state-level politics, and so they might make a bad comparison group to the treated units. So I restrict to a.) units within states that had at least one jurisdiction enrolled in Secure Communities prior to the 2010 election, and b.) units in states that actually signed a memorandum of agreement with ICE prior to the 2010 election.[18] In both cases, the logic is that these states didn't have any clear opposition to the program itself; they were prepared to allow jurisdictions to be enrolled, but some of them didn't happen to enroll all their jurisdictions at once by the time of the 2010 election. In these limited datasets, the treatment coefficient in the main hierarchical linear model with election covariates remains substantially the same.

The results are also robust to using a more conservative analytic approach on the main dataset, following Green and Vavreck (2007) in aggregating the data to the level at which treatment was assigned. This yields a dataset with 49 state clusters rather than thousands of individual jurisdictions.[19] Regressing the cluster-level change in Latino turnout from 2006 to 2010 onto the SC treatment variable and the set of election-timing covariates, as in the main analysis (weighted by the number of jurisdictions in each cluster, as in Green and Vavreck (2007)), yields a treatment effect estimate of 2.9 percentage points (p=.034).

Next, I ensure that my results are not being driven by one state with poor data or uncommon events. If one of the treated places had a particularly contested 2010 election, or if some of the untreated places had uncharacteristically high Latino turnout in 2006, there could be a difference in 2006-2010 slopes that was not actually due to my treatment. For example, it is possible that Robert Menendez's 2006 Senate campaign in New Jersey mobilized Latino voters there to turn out to vote at higher-than-average levels, such that 2010's return to ordinary turnout levels looks like a drop in turnout. In cases like this, my difference-in-difference

[18] States besides my 5 "treated" states signed MOAs; however, not all units in these other states became enrolled in the program by the 2010 election. This seems to have been due to differences in agreement timing, the structure of state criminal justice information systems, and possibly ICE field office resources.

[19] Arizona does not appear in this dataset because all of its counties were enrolled individually in the SC program before the 2010 election. This does not seem to have occurred as a result of any state action, as in the "treated" states, but simply as a gradual voluntary enrollment.

analyses could find an apparent "treatment effect" even if Latino turnout in treated places were exactly the same before and after Secure Communities implementation.

To address such concerns, Figure 1.3 shows the estimated treatment effect (from the preferred specification) when each state cluster is omitted from the analysis. The resulting changes in effect size are minor, suggesting that no one state's political landscape is driving the results.

Finally, I run a placebo test to make sure that the effect estimated isn't capturing something else about the 2010 election season. Running the same analysis on a dataset of non-Latino white turnout in the 2006 and 2010 elections does not yield a significant Secure Communities treatment effect. White voters are less likely to feel threatened by the Secure Communities program, are less likely to know deportable immigrants, and are less likely to be targeted by activists seeking to get out the vote in the wake of program implementation. That they do not respond to the Secure Communities "treatment" is reassuring, as it suggests that the Latino effect measured here is in fact threat-related and not spurious. Similarly, Appendix A reports results from another placebo test, checking for a treatment effect on Latino turnout prior to the implementation of Secure Communities.

1.5 Possible Mechanisms

Secure Communities' implementation could cause the voter turnout effects shown in Figure 2 in several ways. First, voters could have direct personal experiences with immigration enforcement, seeing family members or neighbors face deportation. As noted in the introduction, the design of this paper explicitly precludes that possibility by focusing on the *immediate* effects of the program in places that implemented it only a few weeks or months before the election. In Appendix B, I provide further evidence that even with fairly generous assumptions about the number of deportations that could have happened in this short period, the number of people directly affected by SC does not seem to predict voter turnout. Comparing places that had high numbers of fingerprint submissions to ICE

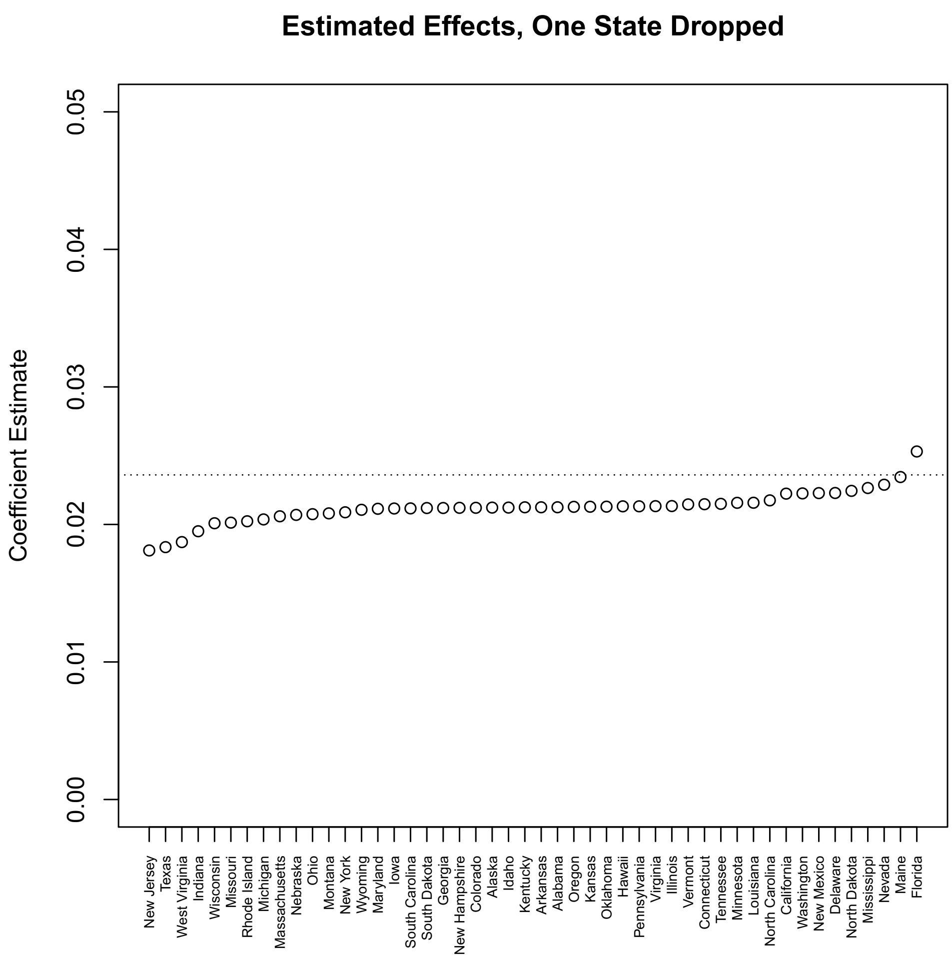

Figure 1.3: Treatment estimates when sequentially dropping each state cluster from the analysis. Dotted line represents the estimated treatment effect from the full sample.

(the first thing to change after program implementation, and the first step on the path to deportation) to those with low numbers of submissions, I do not observe any difference in treatment effects. If direct personal experiences were driving these effects, we would expect the effects to be concentrated among high-submission places where more fingerprints were submitted and therefore more people could face the threat of eventual deportation. That they are not suggests that something other than direct personal experience with the program is driving the observed treatment effect.

The "threat" mechanisms by which Secure Communities could shape turnout fall into two categories: individual voters' responses, and activist mobilization. These are not mutually-exclusive: for example, individuals could be aware of the program and feel threatened by it, but not link that to political action until prompted to do so by activists. In this story, both threat and activist mobilization would be necessary for turnout. I cannot fully distinguish between these individual and mobilization mechanisms, as there is no good measure of individual voters' knowledge of the Secure Communities program at the time of enrollment. However, I can use survey data on mobilization to look for evidence of activists' role in increasing voter turnout in affected places. In this section, I find that Latino voters in places with Secure Communities were more likely to report being asked to turn out to vote than those in other places.

1.5.1 Survey Data on the "Mobilization" Mechanism

Despite national reports of low interest and projections of low Latino voter turnout in the 2010 midterm elections, there were vigorous on-the-ground voter mobilization efforts in some places, including some of the "treated" jurisdictions in my sample. In central Florida, volunteers with Democracia called Latino voters and encouraged them to turn out.[20]. Mi Familia Vota partnered with other national and local organizations to launch a multistate

[20]Source: Democracia Ahora's 2010 election-day liveblog, accessed December 2014 through the Internet Archive:
`https://web.archive.org/web/20101123133606/http://democracia-ahora.org/blog/election_day_live_blog/`

get-out-the-vote effort that included Texas and Florida.[21] However, there is little available data on these mobilization efforts, so it is difficult to tell whether they were more likely to take place (or to be successful) in treated areas. Could these efforts have led to the turnout results shown in Figure 1.4? For more systematic evidence on voter mobilization, I turn to survey data from the Cooperative Congressional Elections Study (CCES) from 2010.

The question is whether Latino eligible voters in treated jurisdictions were more likely to report being asked to vote than eligible voters in non-treated places. This appears to be the case. Table 1.5 shows the results of regressing answers to the question "During the November election campaign, did a candidate, party organization, or other organization contact you to get you to vote?" onto various predictors for Latino respondents in 2010.[22] Results presented are from OLS models, with standard errors clustered at the county level. Column 1 reports the bivariate relationship between living in a place with SC implementation and reporting contact, which is positive and significant. It remains fairly large and marginally significant even when including other factors that should predict activist or campaign contact, like party identification or being a registered voter. Table 1.6 presents equivalent results for respondents' self-reported voting behavior in the 2010 general election. In this case, the coefficient estimates are not always statistically significant, but are always positive, again suggesting that Latinos living in counties with Secure Communities in place were more likely to turn out to vote in 2010.

Living in a place where Secure Communities was implemented before the 2010 election was associated (observationally) with more voter mobilization efforts for Latinos in 2010. I ran two placebo tests to make sure that this wasn't simply due to underlying differences in mobilization across treated and untreated places. I find no comparable effect for Latino CCES respondents in 2006, which is reassuring: the Secure Communities treatment should not have an effect on the 2006 election, as it hadn't yet happened. I also find no comparable

[21] `http://latindispatch.com/2010/10/14/hispanic-groups-aim-to-mobilize-voters-before-november/`

[22] For this analysis, I omit responses from jurisdictions that may have selected into the SC program, so my geographic coverage is comparable to the main analysis. That is, responses are included from "reluctant enrollee" counties and unenrolled counties as of the 2010 election.

effect (neither substantively nor statistically significant) for non-Latino CCES respondents in 2010. Both these results should give us confidence that the results presented in table 1.5 are not simply a coincidence, but are due to specific mobilization of Latinos in the wake of Secure Communities implementation.[23] These results do not rule out the possible importance of individuals' "threat" responses (learning about Secure Communities and deciding to vote either without any mobilization, or because of both the policy and the mobilization), but they demonstrate that mobilization is at least one pathway through which the policy affected turnout.

[23] See appendix for regression tables.

State	Units	State	Units
Delaware*	3	Mississippi	77
Florida*	43	Missouri	113
Texas*	25	Montana	53
Virginia*	46	Nebraska	91
West Virginia*	54	Nevada	11
Alabama	67	New Hampshire	10
Alaska	28	New Jersey	21
Arkansas	72	New Mexico	28
California	20	New York	58
Colorado	64	North Carolina	41
Connecticut	7	North Dakota	53
Georgia	151	Ohio	82
Hawaii	1	Oklahoma	75
Idaho	42	Oregon	32
Illinois	80	Pennsylvania	64
Indiana	92	Rhode Island	4
Iowa	98	South Carolina	39
Kansas	105	South Dakota	60
Kentucky	119	Tennessee	91
Louisiana	59	Utah	18
Maine	16	Vermont	14
Maryland	20	Washington	39
Massachusetts	12	Wisconsin	72
Michigan	79	Wyoming	23
Minnesota	87		

Table 1.1: Units in dataset, by state. Asterisks indicated treated states (that is, all units in the dataset from this state are treated. Other units from the state are excluded as they may have self-selected into the program prior to state enrollment. Similarly (see text), units from “untreated" states that voluntarily enrolled in the Secure Communities program are excluded from these counts.)

Table 1.2: Mean values of Census/ACS characteristics for restricted sample, entire country.

	Sample	All jurisdictions
Latino citizen population, 2006	2143	5721
Total population, 2010	62594	96085
Population density, 2010	148	211

Table 1.3: Observational approach: Comparing all jurisdictions enrolled in SC to all unenrolled jurisdictions

	Dependent variable:
	Turnout change, 2006-2010
Enrolled in SC by election 2010	0.011^{***}
	(0.003)
Senate election 2010	-0.027^{***}
	(0.003)
Senate election 2006	0.001
	(0.003)
Governor election 2006	0.002
	(0.013)
Governor election 2010	-0.009
	(0.014)
Constant	0.025^{***}
	(0.005)
Observations	3,044
R^2	0.034
Adjusted R^2	0.033
Note:	*p<0.1; **p<0.05; ***p<0.01

	Turnout change, 2006-2010			
	OLS		*linear mixed-effects*	
	(1)	(2)	(3)	(4)
Treatment (Involuntary SC enrollment)	0.013**	0.024***	0.021***	0.019*
	(0.006)	(0.005)	(0.007)	(0.012)
Senate election 2006		−0.031***	−0.025***	−0.024***
		(0.005)	(0.008)	(0.008)
Senate election 2010		0.004	0.009	0.011
		(0.007)	(0.007)	(0.008)
Governor election 2006		−0.003	0.004	0.005
		(0.004)	(0.026)	(0.026)
Governor election 2006		−0.010*	−0.014	−0.013
		(0.006)	(0.026)	(0.026)
Constant	0.006	0.029***	0.021**	0.018
	(0.004)	(0.009)	(0.010)	(0.012)
Observations	2,478	2,478	2,478	2,478
Note:			*p<0.1; **p<0.05; ***p<0.01	

Table 1.4: **Main analysis: Jurisdiction-level difference-in-differences.** OLS models include standard errors clustered at the state level, using the RMS package in R (command robcov, specifying the Efron method.)

Table 1.5: Reported campaign/activist contact, CCES 2010 (Latinos)

	(1)	(2)	(3)
SC Treatment	0.158**	0.130*	0.129*
	(0.071)	(0.074)	(0.072)
Registered Voter		0.247***	0.192***
		(0.019)	(0.019)
Gender: Female		0.010***	0.009***
		(0.001)	(0.001)
Age		−0.051**	−0.043*
		(0.024)	(0.023)
Party ID:Republican			0.030
			(0.032)
Party ID: Independent			−0.080**
			(0.031)
Party ID: Other			0.215***
			(0.061)
Party ID: Not Sure			−0.267***
			(0.029)
Senate Election 2010			0.063*
			(0.035)
Governor Election 2010			−0.007
			(0.040)
Constant	0.454***	−0.138***	−0.052
	(0.018)	(0.027)	(0.051)
Observations	1,460	1,460	1,460
R^2	0.004	0.194	0.242
Adjusted R^2	0.003	0.192	0.237
Note:	*p<0.1; **p<0.05; ***p<0.01		

Table 1.6: Reported general election turnout, CCES 2010 (Latinos)

	(1)	(2)	(3)
SC Treatment	0.141**	0.116	0.059
	(0.070)	(0.073)	(0.066)
Registered Voter		0.093**	0.063
		(0.045)	(0.041)
Gender: Female		0.011***	0.010***
		(0.001)	(0.001)
Age		−0.115***	−0.097***
		(0.023)	(0.022)
Party ID:Republican			0.043
			(0.035)
Party ID: Independent			0.015
			(0.040)
Party ID: Other			0.052
			(0.085)
Party ID: Not Sure			−0.404***
			(0.039)
Senate Election 2010			0.105**
			(0.048)
Governor Election 2010			−0.016
			(0.052)
Constant	0.586***	0.069	0.130*
	(0.017)	(0.054)	(0.077)
Observations	1,334	1,334	1,334
R^2	0.004	0.144	0.227
Adjusted R^2	0.003	0.142	0.221
Note:			*p<0.1; **p<0.05; ***p<0.01

1.6 Discussion/Conclusion

This paper finds evidence that Latino voters in places where Secure Communities was implemented turned out more than they would otherwise have been expected to do. This turnout seems to have been accompanied by more contacts asking Latinos to vote. This suggests mobilization in response to threat of a specific kind: people being mobilized by (or in the wake of) policies that by definition did not target them personally. These findings open many avenues for future work.

First, time may yield better data with which to test this process, as more years of data are available to test parallel trends and other immigration enforcement policies emerge. Next, the specific process of mobilization merits close examination. Who asked Latino citizens to turn out in the wake of policy changes? How do places differ in their capacity for this sort of mobilization? What is the role of elite actors in organizing the response to such policies? Finally, when does this result (of increased turnout) hold, and when does it disappear or even reverse? Other punitive or paternalistic policies are associated with diminished turnout, even for those who experience them secondhand (Burch 2013; Weaver and Lerman 2014; Bruch, Ferree, and Soss 2010). Why should the realm of immigration enforcement differ?

The case of immigration enforcement is different from policies like welfare policy or incarceration in several ways. The intervention studied here, the Secure Communities program, was a distinct policy change that affected entire counties at once; work on policy feedbacks of welfare or prison has usually focused on the contact that a specific person or family has with the government, not with major changes in broad policy. Further, deportation policies might be thought to target a less stigmatized population than welfare or incarceration, though this is debatable. Deportation also differs from most policies in that it cannot be expected to happen to voters, no matter what: voters have no reason to fear retaliatory deportation (at least of themselves) if they become politically involved. Finally, the potential voters studied in this paper, Latino citizens across the US, span a wide range of ages, classes, education and income levels. Which of these differences matter most for mobilization have yet to be determined.

These results on immigration policy also have implications for party competition. In the case of Secure Communities, a Democratic president implemented a program that was unpopular with Latino voters, but that actually prompted these voters to turn out at higher rates–and quite possibly to vote for Democrats. This pattern is both counterintuitive and different from the rest of the policy feedback literature. Time will tell whether this pattern of Democratic politicians benefiting from Democrats' restrictive immigration policies) will persist, with Latino voters becoming increasingly "captured" by the Democratic party (Frymer 1999), or whether the Republican party will be able to successfully compete for Latino votes.

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

2.1 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. 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). 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). 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 voter deterrence without legal restrictions on voting, as none of the defendants in my analysis will be legally disfranchised due to felony 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. Racial differences in treatment effects will further amplify the representational impact of jail on voting: black citizens were already more likely to have criminal justice contact, but my results suggest jail will also have more of demobilizing effect on black defendants. The nation's jails are not only sites of policy implementation, but have important effects on future elections and the inclusivity of American democracy.

2.2 Theory

2.2.1 Incarceration as a Demobilizing Force

The first goal of this paper is to test whether incarceration reduces voter turnout. A number of 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 voting. 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. 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 disenfranchisment laws (Haselswerdt 2009; Miles 2004; Hjalmarsson and Lopez 2010; Gerber et al. 2015). Existing research has attempted to address this question using survey self-reports[1] 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 who are incarcerated have been convicted of felonies, and are thus ineligible to vote for at least some period of time in most states. In many states, they will actually be purged from the voter rolls, and so will face an additional hurdle if they do try to vote when released. 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 from politics, rather than legal restrictions on voting such as felon disfranchisement laws.[2] But

[1]Some recent work has used administrative records to measure contact with the criminal justice system (Meredith and Morse 2013, 2014; Gerber et al. 2015).

[2]A related concern here is that some potential voters might mistakenly believe they are ineligible to vote after a misdemeanor conviction. I do not think this can fully account for the effects found; this issue is discussed further in the results section.

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 many collateral consequences to misdemeanor convictions, as well as the possibility of jail time, probation, and fines (Roberts 2011).

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).

2.2.2 Racial Differences in Incarceration's Effects

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) have been 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 now-defunct "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.

This existing literature suggests several possible reasons for differing effects. 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. Any of these mechanisms could lead to larger effects for black than white defendants.[3]

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.

2.3 Data and Methods

2.3.1 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

[3]The 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.

and November 6, 2012.[4] 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).[5] 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.

First-time misdemeanor cases filed with the Harris County District Clerk are randomly assigned to one of fifteen courtrooms by a computer program.[6] 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

[4]I 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.

[5]A 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 all extreme ages (such as restricting to only ages 18-60).

[6]Defendants 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 quite rare.

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 exceedingly 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.[7] 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.[8]

Table 2.1 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 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.

2.3.2 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

[7] Results are also robust to dropping defendants with more than one misdemeanor case.

[8] Some 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.

Table 2.1: 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

comes from the Texas voter file.[9]

Defendants' court records were linked to the voter file on defendant/voter names and birthdates. I first merged the files on the last name, first initial, and birthdate columns. Then, "ties" between potential matches were adjudicated using string distances between the first names reported in both files. I calculated how dissimilar the first names were in all possible matches, and then 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.[10]

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.[11]

[9]The voter file was generously provided by NationBuilder, which collects voter files as part of their campaign services business. The file was provided in late 2014, but had been 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) 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.

[10]For this approach, I used R's stringdist package, with the "jaro-winkler" option. I used .2 as an absolute cutoff for match quality, but show plots in the SI demonstrating that changing this cutoff does not substantively change the results.

[11]If a defendant was not matched to the voter file, I consider them as a nonvoter in 2012. I calculate turnout, not turnout conditional on registration, for two reasons. First, the difficulty of registering when

Because names and birthdates could be recorded differently in different datasets or could be 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, and would cast doubt on the actual matching process.

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.[12]

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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.

[12]In the SI, I explore this point further by discarding some of the matches from my main dataset and seeing what the addition of these "missed matches" does to the analysis. The estimates shrink towards zero and become more uncertain as I discard more and more actual matches.

2.4 Results

2.4.1 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 for a first-time misdemeanor charge in the four years prior. The results of this analysis appear in Table 2.2. These estimates are likely biased: defendants who go to jail are probably different from those who don't in a number of unobserved ways. 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 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.

2.4.2 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 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 have committed crimes that would always lead to some jail time, and some defendants would have been acquitted (or 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.

Table 2.2: OLS estimates

	Dependent variable:		
	vote2012		
	(1)	(2)	(3)
Jail	−0.105***	−0.097***	−0.080***
	(0.002)	(0.002)	(0.002)
Voter Birth Year		−0.005***	−0.005***
		(0.0001)	(0.0001)
Black		0.115***	0.146***
		(0.002)	(0.003)
Male		−0.043***	−0.043***
		(0.002)	(0.002)
Jail*Black			−0.060***
			(0.004)
Constant	0.183***	9.464***	9.403***
	(0.001)	(0.175)	(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
Note:			*p<0.1; **p<0.05; ***p<0.01

I use courtroom assignment to instrument for incarceration (Kling 2006; Green and Winik 2010; 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 (sometimes described as "omitted treatments") in section 2.4.4.

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 2.1 and Table D.1 address these requirements. Figure 2.1 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.[13] Figure 2.1 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. 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-

[13] A very small fraction of cases do not appear in this dataset due to records being sealed, which could hypothetically lead to some imbalance.

hand side of each panel. Table E.1 in the Appendix presents each courtroom's values of these variables over this time period. In Appendix A, I also test more formally for patterns that would suggest non-random assignment to courtrooms.

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?[14] 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.[15]

Results Table 2.3 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,

[14] With few instruments in play, this approach is analogous to simply using courtroom indicator variables as instruments, interacting them with filing-year indicators.

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

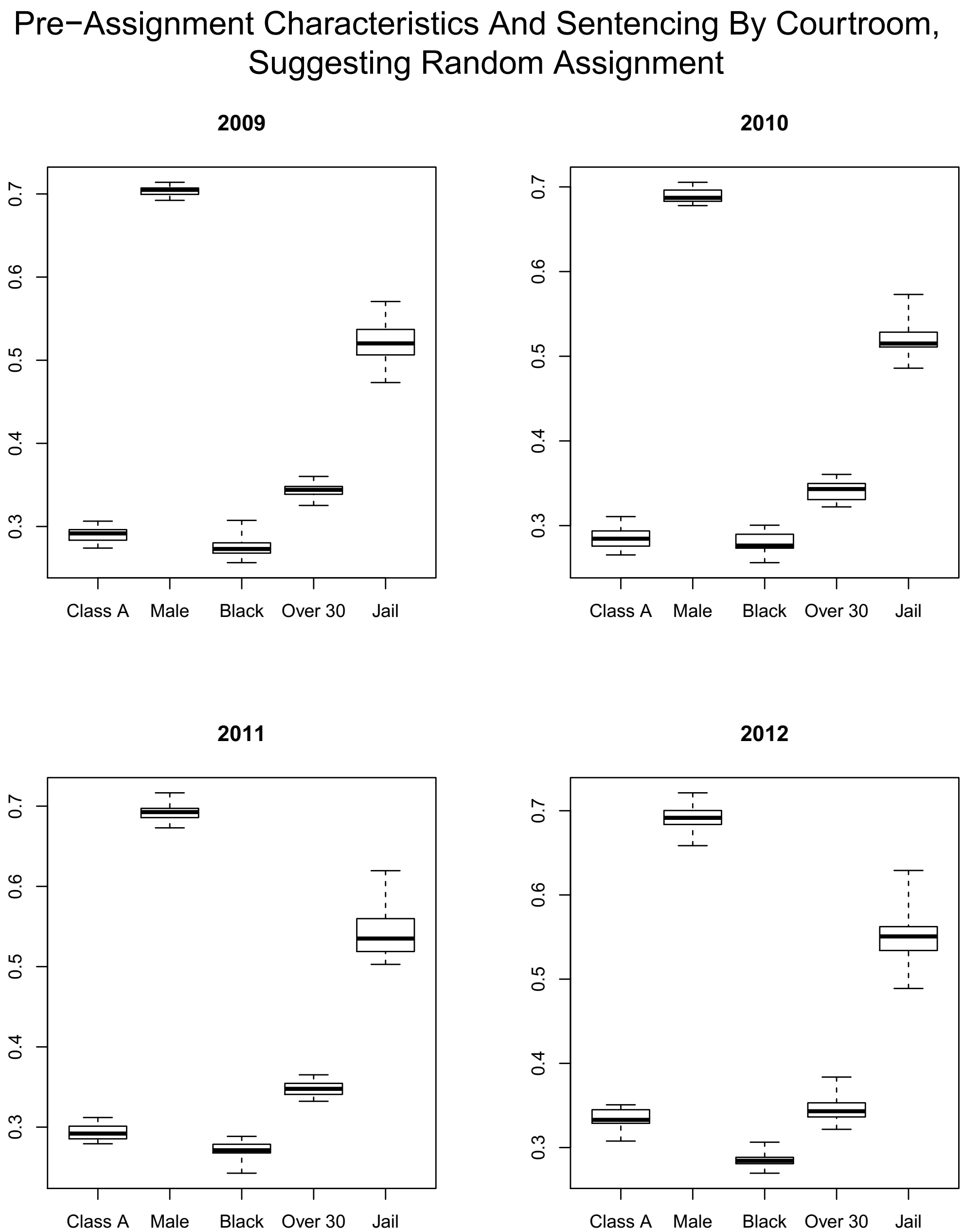

Figure 2.1: Box plot of the full range of several pre-treatment variables, as well as jail sentences, for the 15 county courtrooms. The box edges represent the 25th and 75th percentiles and the middle line the median value of the variable; the whiskers extend to the most extreme value of that variable 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.

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 percentage points, though it is imprecisely estimated. 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 2.3: Jail Sentences on 2012 Voting

	Dependent variable:	
	jail	vote2012
	(1)	(2)
Court Jail Average (Yr)	0.999***	
	(0.051)	
Jail		−0.044
		(0.034)
Most Severe Charge	0.021***	0.016***
	(0.003)	(0.002)
Constant	−0.121***	0.053***
	(0.035)	(0.020)
Year dummies	Yes	Yes
Observations	113,403	113,403
Adjusted R^2	0.005	0.017
F Statistic	89.009*** (df = 6; 113396)	
Note:		*p<0.1; **p<0.05; ***p<0.01

Next, I split the sample to explore whether the deterrent effect of jail differs by race.[16]

Table F.1 presents 2SLS estimates of the effect of jail on voting for black and white defendants separately, and Figure 2.2 plots them. The estimates are strikingly different. The

[16]Race, unlike the few other personal characteristics available from court records (age, gender, physical characteristics and markings), 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.

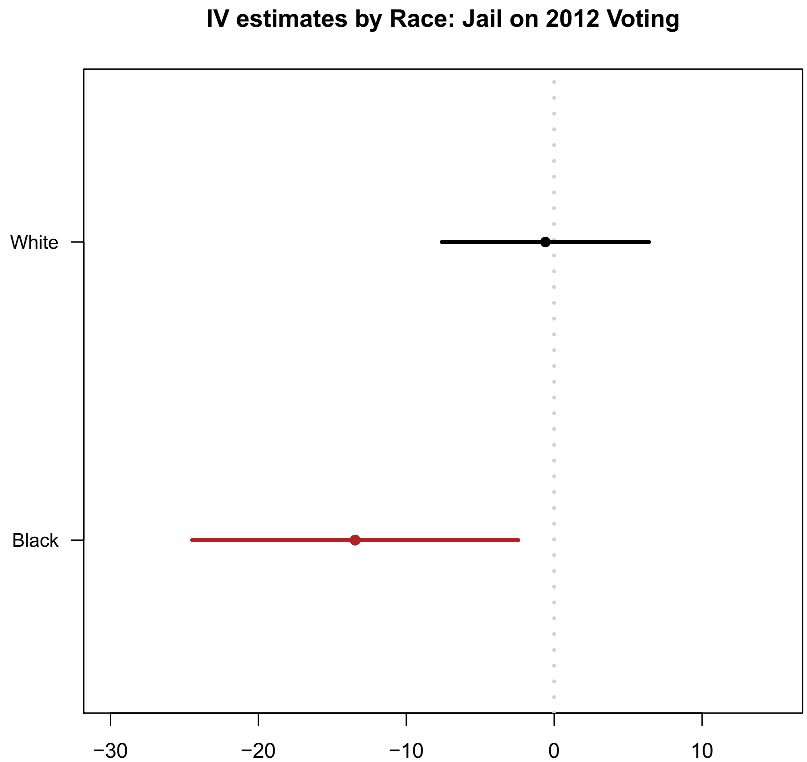

Figure 2.2: Jail's effect on voter turnout (2SLS estimates), by race of defendant. A coefficient of -.13 indicates a turnout decrease of 13 percentage points (among compliers).

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 presents a slightly different model including 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 ($p < 0.01$). 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. I explore this possibility further in section 2.4.3.

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 Appendix E, 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.

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 conceivably have received jail sentences or not depending on their courtroom assignment.

This local effect is interesting from a policy standpoint. The defendants 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. [17] I discuss the generalizability of these results further in Section 2.4.5.

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

[17] One 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.

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.[18] It is possible that individuals still believe in the value of voting (contrary to the theory of Weaver and Lerman (2014)), but that they simply find it too difficult to vote when they are dealing with other problems (Verba, Schlozman, and Brady 1995). The two mechanisms (jail socialization and resource constraints) are slightly different, and I cannot distinguish between them without further data on either resources or defendants' views of government. Nonetheless, 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.

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.[19] 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 2013). But Drucker and Barreras (2005)'s survey of adults with a history of criminal

[18]For state-by-state level data on such consequences, see the American Bar Association's project at http://www.abacollateralconsequences.org/

[19]Simply 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.

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 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. Only defendants who received exceptionally long sentences or those who were sentenced very near the election (or those who were re-arrested) would have been in jail at the time of the election itself.[20] Dropping all cases filed in 2012 yields similar results and rules out this possibility for nearly all defendants.

A related mechanism would be re-arrest: if people who are sentenced to jail in their first case 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 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). 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).[21]

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[20]Technically, 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.

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

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, 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. 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.

However, Harris County's court system appears to have been relatively consistent over the past decade.[22] 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 2.4 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.

[22]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 2.4: IV estimates: Jail sentences on voting, 2000-2012

	Dependent variable:			
	vote2012			
	All defendants		Black defendants	
	(1)	(2)	(3)	(4)
jail	−0.034	−0.024	−0.077**	−0.046
	(0.023)	(0.030)	(0.037)	(0.047)
Constant	0.170***	0.163***	0.284***	0.264***
	(0.016)	(0.021)	(0.025)	(0.032)
Year dummies	Yes	Yes	Yes	Yes
First Stage F-Statistic	5950.73	2453.71	135.41	88.59
2009-2012 data included	Yes	No	Yes	No
Observations	347,896	238,868	93,249	62,953
Adjusted R^2	0.011	0.007	0.023	0.015
Note:				*p<0.1; **p<0.05; ***p<0.01

2.4.3 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.[23] 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

[23] This is not the only possible mechanism that could produce the racial differences I find. Black defendants sentenced to jail could be treated differently in jail than white inmates, or could perceive their sentence differently. Here, I present evidence consistent with one possible mechanism, but do not rule out these other possibilities.

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

Table 2.5: Differences in pre-arrest voter turnout by race

	Dependent variable:	
	Turnout 2008	Turnout 2008
Black	0.084***	0.090***
	(0.002)	(0.002)
Male		−0.042***
		(0.002)
Over 30		0.101***
		(0.002)
Charge severity		0.013***
		(0.002)
Constant	0.085***	0.006
	(0.001)	(0.012)
Observations	113,415	113,274
R^2	0.014	0.042
Adjusted R^2	0.014	0.042
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 2.5 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

[24] In fact, a 2012 lawsuit filed by LULAC (the League of United Latin American Citizens) claimed that Harris county was disproportionately purging minority voters from the voting rolls. So this file may provide an even more conservative measure of past voting for black voters than for white ones.

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 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 deterred 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 are so much higher among black voters than white voters. 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. 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 deterrent effect, 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 (whether they were charged with a class A misdemeanor).

This apparent difference in who gets arrested (black voters, but not white voters) could have serious implications for political representation. We knew that black people were disproportionately likely to be arrested, so even an overall effect of jail on voting for all defendants could have meant that courts were deterring black potential voters more often than white potential voters. But having more regular voters in the pool of black defendants than among white defendants, such that there is a deterrent effect only among black defendants, means that jail sentences will only affect black voter turnout.

2.4.4 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 my main 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. 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 2.6. 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.

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 rely on the assumption 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).

What kinds of other treatments could courtrooms provide? Jail time seems like the

Table 2.6: Placebo IV estimates: Jail on pre-arrest voting

	Dependent variable:		
	vote2012		
	All Defendants	Black Defendants	White Defendants
	(1)	(2)	(3)
jail	−0.009	0.060	0.0001
	(0.044)	(0.086)	(0.043)
Constant	0.104***	0.129***	0.075***
	(0.025)	(0.047)	(0.025)
Year dummies	Yes	Yes	Yes
First Stage F-Statistic	519.9	124.93	398.1
Observations	48,570	14,041	32,440
Adjusted R^2	0.003	−0.028	−0.0001
Note:			*p<0.1; **p<0.05; ***p<0.01

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 convicted, assessed fines, or put on probation. And people's actual time in the courtroom could matter: in theory, defendants' interactions with judges and prosecutors could deter them from voting.[25]

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 simple 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 out-

[25]In practice, 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.

comes. 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 there 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.[26] 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). As sparse as "enrichment" programs such as work opportunities or educational programs may be in prisons, they are essentially nonexistent in jails. 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 jail inmates (Noonan et al. 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).[27] 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;

[26] This is not surprising, as conviction is a necessary prerequisite for jail sentencing.

[27] Some jail inmates are now sent to other Texas counties, or to Louisiana, to reduce crowding.

(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 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, 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, with an F-statistic of less than 10), 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 2.7 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.[28] Figure 2.3 presents first differences based on the reduced form. Even if we can't be 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.

2.4.5 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 are there, both in Harris County and nationwide, that could face demobilization from jail sentencing?

Generalizing LATE I begin by characterizing black compliers, using the few pre-treatment characteristics available from court records. In an analysis in the SI, 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. Compliers are somewhat more likely to be male, are younger than the average defendant, and are less likely to have been charged with a class A (more serious) misdemeanor.

Then, I reweight the complier population to resemble the entire population of black defendants (Aronow and Carnegie 2013). With some distributional assumptions, 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. Using this approach, I estimate an ATE of -.19 for black defendants in Harris County (slightly larger than the complier-specific LATE

[28] 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 2.7: Reduced-form: Courtroom assignment on voting

	Dependent variable:	
	vote2012	
	All Defendants	Black Defendants
	(1)	(2)
courtroom instrument	−0.045	
	(0.034)	
courtroom instrument		−0.134**
		(0.057)
Constant	0.142***	0.263***
	(0.020)	(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<0.1; **p<0.05; ***p<0.01

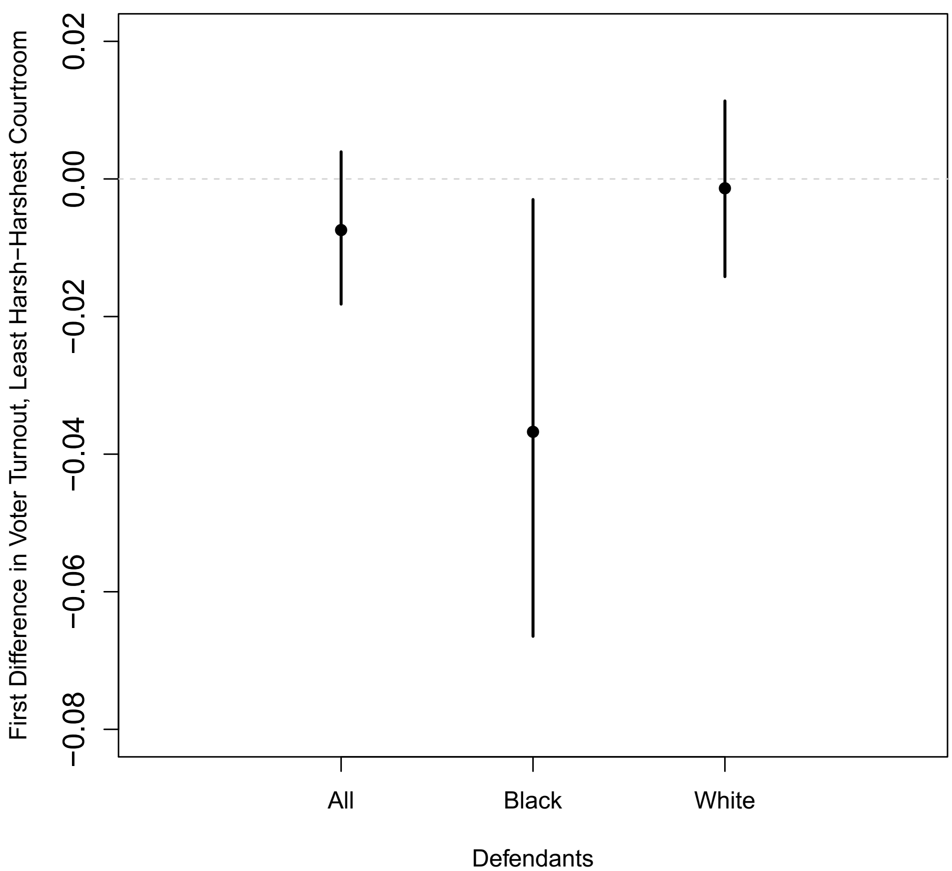

Figure 2.3: Simulated first differences based on the reduced form: these show the predicted change in voter turnout for defendants if they were to be moved from the courtroom with the lowest to the highest incarceration tendency.

estimated in Section 2.4.2).

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, my 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. If the covariate-reweighted ATE holds, the number of demobilized voters rises to about 3,100. These are significant numbers 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. 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.

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 jail admissions data from the Bureau of Justice Statistics. The assumptions made are discussed further in the SI. Briefly, I begin with the 11.6 million people who are estimated to have been admitted to local jails in 2012, and adjust that number to reflect the possibility of individual arrestees being double-counted, the proportion of jail inmates who are black, the proportion of jail inmates that have not been convicted of misdemeanors (either because they are awaiting trial or because they are serving felony sentences in local jails), and the proportion of misdemeanants that are first-time offenders. This yields an estimate of about 300,000 black first-time misdemeanor defendants sentenced to time in local jails each year, or about 1.2 million between the 2008 and 2012 elections.

An alternative approach is to look at Harris County's first-time misdemeanor jail rate for black residents, and extend that rate to the entire US. The 16,000 black first-time misdemeanor defendants sentenced to jail between the 2008 and 2012 elections represented about 1.8% of Harris County's black population. If we assume black residents across the US experience misdemeanor jail sentences at the same rate, this would mean just over 765,000 people were sentenced to jail based on a first misdemeanor case nationally, fewer than estimated from the BJS data above.

Thus, estimates of the affected population range from 765,000 to 1.2 million. If they faced the same rates of demobilization estimated in the main analysis (a drop of 16 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. If we instead use the covariate-reweighted ATE estimated above, then the estimated number of demobilized voters rises, to between 145,000 and 228,000.[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.

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

2.5 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 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 voters simply drop out of the electorate for a decade or more 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, so even an equally-sized effect of jail on voting would result in a larger fraction of black voters being deterred. But that the effect is actually larger for black defendants (in addition to their being more likely to face such jail terms) means that the effect of jail on voting 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 low 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; Walker 2014)? Future research will exploit the same court-assignment design to examine jail's effect on the voter turnout of people that are socially or geographically close to defendants.

3 | Locking Up The Vote: Household spillover effects of incarceration

3.1 Introduction

Millions of Americans have had the experience of seeing a family member or close friend incarcerated. Wildeman (2009) estimates that by 1990, the risk of parental incarceration had risen to 4% of white children and over 25% of black children. Seven percent of women responding to the nationally-representative National Sexual Health Survey reported that they had a primary male partner who had been to prison or jail (Comfort et al. 2005).

Incarceration can shape a range of life outcomes for the families and social networks of people sentenced to jail or prison time. Children of incarcerated parents do worse in school, face higher rates of mental health problems and mortality, and are more likely to be incarcerated themselves (Johnson 2009; Wakefield and Wildeman 2013; Lee, Fang, and Luo 2013). Adults whose romantic partners are incarcerated face emotional stress as well as financial strain, as they try to pay for visits and phone calls to their loved one on a single income (Grinstead et al. 2001; Comfort 2008).

What about political participation? Does the incarceration of a household member, whether it's a family member, romantic partner or perhaps a roommate/friend, change people's political behavior? Survey evidence on the political participation of people with incarcerated loved ones has been mixed, depending on the sample used, the type of relationship examined, the kind of carceral experience measured, and the political outcomes collected.

In this paper, I measure the effect of "proximal contact" to incarceration using administrative data rather than survey responses, and focus on a fairly common experience: having a member of one's household spend a short time in jail due to a misdemeanor conviction. Using a dataset from one large county court system, I geolocate misdemeanor defendants and then rely on the state's voter file to find registered voters who lived at the same address as a defendant. This allows for a broader sample, as well as more reliable measure of voter turnout and incarceration than would be available from surveys.

I find evidence that voters whose household member was incarcerated before the 2012 general election were less likely to vote in that election than voters whose housemate faced charges but was not sentenced to jail. This is true even when I adjust for covariates such as voting in prior elections, and when I use genetic matching to construct demographically-comparable groups of “treated" and “untreated" voters. I also take advantage of the random assignment of defendants to courtrooms, using courtroom assignments as instrumental variables to find the causal effect of jail on household members voting, and find similar estimates.

These results underscore the political importance of incarceration in an era of record penal populations. This paper carefully measures just one part of a much larger effect, the net effect of criminal justice contact (from police encounters to arrest to probation or incarceration) on the families and friends of those targeted. This paper draws a narrow definition of proximal contact, and then estimates the marginal effect of seeing a household member jailed among people who have already had a lot of contact with the criminal justice system (seeing their housemate arrested and criminally charged). That I find substantial effects of jail on household members' voting, even when focusing on people that have already experienced some proximal contact, suggests that the larger cumulative effect of criminal justice contact is larger still.

3.2 Incarceration and Political Participation

Interactions with the criminal justice system, especially time spent behind bars, have been shown to reduce political participation (and to change views of government) among people who directly experience them (Fairdosi 2009; Weaver and Lerman 2010, 2014; White 2015). But what about people who have "proximal contact" (Walker 2014) with the system through family, friends, or neighbors? Do they become less likely to undertake political action, either because of the practical costs that incarceration imposes on households or because of alienation or distrust in government (Weaver and Lerman 2014; Lee, Porter, and Comfort 2013; Burch 2013)? Or do they instead become politically activated, as people who are somewhat shielded from the actual experience of incarceration but still see it as a threat to their community (Walker 2014)?

Existing work describes a number of mechanisms by which proximal criminal justice contact could reduce political participation, as well as ideas of when these mechanisms may or may not operate. Lee, Porter, and Comfort (2013) divides these mechanisms into indirect (transmitted through the incarcerated loved one) and direct effects of familial incarceration. Indirect mechanisms could include political socialization (your family member tells you about the criminal legal system and changes your view of government or your sense of efficacy) as well as modeled behavior (your family member becomes less likely to vote or follow politics and you follow their lead). Direct mechanisms include the stresses incarceration places on a household (time spent dealing with criminal cases or visiting jail or prison, loss of a wage earner, emotional tension), or the "secondary prisonization" that happens when families adapt to the structures of the prison system and become less accustomed to other state structures (Lee, Porter, and Comfort 2013; Comfort 2008). People may also withdraw from political and civic life due to shame and the stigma that surrounds incarceration, or may change their views of government due to having seen the state incarcerate their loved one (Weaver and Lerman 2014).

However, Walker (2014) hypothesizes that this sort of proximal contact might not always lead to withdrawal. Focusing not only on immediate family but anyone who reports that a friend or family member has had criminal justice contact,[1] Walker (2014) notes that some

[1] The survey question asks "And what about someone you know, such as a close friend or family member? Do you know someone who has been arrested, charged, or questioned by the police, even if they were not guilty, excluding minor traffic stops such as speeding?"

people who do not bear the direct costs of this contact might not be politically demobilized. Instead, drawing on the literature on mobilization in response to threat, Walker (2014) proposes that members of groups that are disproportionately targeted by the criminal justice system, such as young black men, might be moved to push back on this system by becoming even more politically involved.

The argument in Walker (2014) does not focus on voting so much as on other forms of political turnout (protest, contacting officials, etc.), but the paper nonetheless reports a null effect on voting: seeing a friend or family member face criminal justice contact does not decrease voter turnout at all.[2] Is this null finding a survey artifact, driven by a strange sample or by the characteristics of respondents that are willing to admit to having proximal contact? Or is it because this survey uses a looser definition of proximity than earlier studies of family members have done, as well as a lower bar for criminal justice contact?

The disparate results of prior papers suggest that more work is needed to accurately measure various kinds of proximal contact and test their effects on participation. I further this goal by using a large sample not dependent on survey response, and by carefully defining a well-measured treatment and a distance-based measure of proximal contact. I also use a treatment (exposure to short jail terms from misdemeanor crimes) that presents in many ways a hard test of the hypothesis that proximal contact can reduce participation, as I discuss further in Section 3.4.

[2]Similarly, Burch (2013)'s analysis of neighborhoods with incarcerated residents show mixed results, with some specifications finding that neighborhoods where someone was incarcerated before an election had lower voter turnout and others finding no effect (p. 90).

3.3 Using Administrative Data to Measure Contact

3.3.1 Setup

I begin with public records from the Harris County (Texas) criminal courts at law, which hear all misdemeanor cases in the county (including those from the city of Houston).[3] These records contain information on nearly all misdemeanor cases filed in recent years, including the charges faced, the date of the case, name and other identifying information about the defendant, and case disposition and sentencing.[4] I focus on first-time misdemeanor cases filed between 2009 and 2012 (before the 2012 election), in which the defendant has a valid address within Harris County on file. I then use the recorded addresses of these defendants to precisely geocode their residences.[5]

Next, I find household members of these defendants within the Texas state voter file, which contains the addresses of all registered voters. I geocode all voters within Harris County and then find any registered voters who live within five meters of one of the geocoded misdemeanor defendants. To avoid including all residents of large apartment complexes or housing projects as "household members" of a given defendant, I omit all addresses with more than 10 registered voters at that address. I also ensure that no actual misdemeanor defendants are included in the sample.[6] This yields a sample of registered voters who lived with a person charged with a misdemeanor between January 2009 and the 2012 general election. Appendix A discusses the evidence that these voters genuinely have some relationship with the defendants to which they've been matched; many appear to be close family members.

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[3] Records were requested from the District Clerk's office and are publicly searchable at the District Clerk's website: http://www.hcdistrictclerk.com

[4] A very small number of cases may not appear in this dataset due to having been sealed. This appears to be extremely uncommon (Mueller-Smith 2014).

[5] I geocode using ArcMap 10. See the Supplemental Appendix for additional geocoding details.

[6] Some of the defendants are themselves registered voters and so appear in the voter file. I find and remove them based on last name and birthdate matches at each address.

This dataset allows me to compare voters whose housemates were sentenced to jail time to those whose household members faced misdemeanor charges but were not ultimately jailed. Thus I can observe the marginal effect of housemates' jail time, not the total effect of having a household member arrested, charged, and ultimately incarcerated. Everyone in this sample experienced a household member facing criminal charges.

3.3.2 Descriptive Statistics

Defendants Of the 182,562 people that faced misdemeanor charges for the first time in 2012, 90,606 of them were successfully geocoded to addresses in Harris County.[7] Of these, 36,442 lived in a household with at least one registered voter.

The defendants in this sample were charged with misdemeanor crimes, which in Texas carry up to one year's penal sentence.[8] In practice, 66% of them were ultimately convicted of a crime, and 45% were sentenced to some jail time. But most sentences were short: the median length of sentence was 10 days, and only 39% of jailed defendants were sentenced to a month or more in jail. Certainly this is a long enough time for proximal contact to seriously affect housemates' lives, but it is unlikely that some of the longer-term life disruptions described by prison ethnographers would happen over this time frame (Comfort 2008; Wacquant 2000).

Voters 87,591 registered voters in Harris County lived at the same address as someone who faced misdemeanor charges in 2012. Table 3.1 compares these voters' characteristics to the entire universe of registered voters in the county. Compared to all registered voters, those living with defendants are younger and have been registered to vote for less time. They are also less likely to have voted in prior elections, even before their household member was arrested. Still, turnout rates for this group are far higher than turnout found among people

[7] Defendants that could not be geocoded were either recorded as homeless, did not have any address on file, or had malformed or extremely vague addresses on file. Many others had valid addresses outside of the county.

[8] Common case types for these courtrooms include driving while intoxicated, theft, possession of small amounts of marijuana, and certain types of (non-aggravated) assault.

with direct criminal justice contact in prior studies (Burch 2010; Haselswerdt 2009; White 2015). This could allow for much larger demobilizing effects among family members than have been seen for incarcerated people themselves.

Registered voters living with defendants are also less likely to have voted in the 2012 election than the general public. In the next section, I focus on the set of voters living with defendants and ask whether seeing a housemate go to jail makes one even less likely to vote.

	Proximal Contact Sample	All Voters
Voter Turnout 2012	0.46	0.55
Prior Voter Turnout (2008)	0.42	0.53
Mean Age (Years)	42.09	46.10
Proportion Male	0.47	0.46
Mean Time Registered (Years)	10.10	11.48

Table 3.1: Comparing the sample used in this paper to the full set of registered voters in Harris County.

3.3.3 Regression Analysis

In this section, I ask whether voters whose household member spends time in jail become less likely to vote in the next election. First, I run a simple regression model predicting registered voters' 2012 voter turnout with a dummy variable for whether their housemate was sent to jail for any length of time.[9] In the subsequent columns of Table 3.2, additional covariates are included in the analysis. Column 2 includes the voter's prior turnout in the two preceding congressional elections; Column 3 adds in some demographic information about their household member, and Column 4 includes the age and gender of the voters themselves.[10]

[9] I focus on defendants' first misdemeanors and do not follow up, so it is possible that some small subset of voters whose housemate wasn't jailed the first time could still have seen their housemate go to jail in a subsequent case. This should make the estimates presented here conservative.

[10] This is an individual-level analysis at the level of the voter, but all results are robust to clustering standard errors at the level of the criminal defendant, to account for the fact that several voters could live with the same defendant.

Across these specifications, I estimate a substantively large, statistically-significant decrease in voter turnout associated with having a household member incarcerated (relative to having them face criminal charges, but not go to jail). These results are robust to including geographic fixed effects at the level of the zip code, state house district, or state senate district, as reported in Appendix B. They are also robust to including various census-tract level covariates from the 2010 Census, such as population, percent Black or Latino, and average household size (table in Appendix B).

We might wonder whether this is a short-term effect: do people avoid voting six months after a housemate is jailed, but return to voting two or three years later? Table 3.3 presents estimates from year-specific regression models: the effect of having a housemate jailed in 2009, 2010, 2011, or 2012 on 2012 voting. The estimates do not appear to drop off substantially over the four year period, suggesting that there is a lasting deterrent effect. Future work will extend these estimates further into the past to see whether the effect persists beyond one presidential election cycle.

3.3.4 Matching

We may worry that households whose members are jailed differ (across a number of dimensions) from those whose members are not jailed, and that the OLS estimates above could be extrapolating beyond the range of the data. In this section, I use genetic matching to prune the set of "untreated" voters (those whose household members avoided jail) to those that most closely resemble the "treated" households on a range of individual, household, and neighborhood-level variables. I use genetic matching (Sekhon 2011) to match on voters' age, gender, prior vote history (2008 and 2010) and housemate race and gender, performing one-to-one matching with replacement. After constructing this more-balanced sample (see plots in the supplemental appendix for evidence of improved covariate balance), I non-parametrically estimate the effect of having a household member go to jail. These estimates look extremely similar to the OLS estimates presented in the prior section: seeing a household member jailed reduces voter turnout by about 2.9 percentage points.

Table 3.2: Basic OLS estimates, including prior vote, defendant and voter characteristics

	Dependent variable:			
	vote2012			
	(1)	(2)	(3)	(4)
Household Member Jailed	-0.054^{***}	-0.025^{***}	-0.026^{***}	-0.026^{***}
	(0.003)	(0.003)	(0.003)	(0.003)
2008 Turnout		0.491^{***}	0.483^{***}	0.481^{***}
		(0.003)	(0.003)	(0.003)
HH Member Black			0.071^{***}	
			(0.003)	
HH Member Male			0.006^{**}	
			(0.003)	
HH Member Age (Years)			0.00000	
			(0.00000)	
Voter Male				-0.042^{***}
				(0.003)
Voter Age (Years)				0.001^{***}
				(0.0001)
Constant	0.482^{***}	0.262^{***}	0.235^{***}	0.257^{***}
	(0.002)	(0.002)	(0.005)	(0.004)
Observations	87,591	87,591	87,459	86,241
R^2	0.003	0.238	0.242	0.242
Adjusted R^2	0.003	0.238	0.242	0.242
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 3.3: OLS estimates by year of household member's criminal case

	Dependent variable:			
	vote2012			
	2009	2010	2011	2012
	(1)	(2)	(3)	(4)
Household Member Jailed	-0.025^{***}	-0.030^{***}	-0.025^{***}	-0.032^{***}
	(0.006)	(0.007)	(0.007)	(0.007)
2008 Turnout	0.473^{***}	0.487^{***}	0.485^{***}	0.477^{***}
	(0.007)	(0.007)	(0.007)	(0.007)
Voter Male	-0.045^{***}	-0.044^{***}	-0.053^{***}	-0.032^{***}
	(0.006)	(0.007)	(0.007)	(0.007)
Voter Age (Years)	0.001^{***}	0.0005^{**}	0.001^{***}	0.001^{***}
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Constant	0.236^{***}	0.267^{***}	0.280^{***}	0.254^{***}
	(0.010)	(0.010)	(0.010)	(0.010)
Observations	18,782	17,759	16,951	16,205
R^2	0.240	0.247	0.245	0.238
Adjusted R^2	0.240	0.246	0.245	0.238
Note:				*p<0.1; **p<0.05; ***p<0.01

3.3.5 IV: Random Courtroom Assignment

We might still worry that these estimates do not have a causal interpretation, because lower voter turnout among people with proximal contact could be caused by some unobserved variable and not the spillover effects of jail sentences. In this section, I present estimates from an instrumental-variables (IV) analysis intended to address these concerns. In this design, I take advantage of the random assignment process used to distribute misdemeanor cases to the 15 misdemeanor courtrooms within Harris County. These courtrooms differ substantially in how frequently they sentence defendants to jail, so being sent to a "harsh" courtroom rather than a more "lenient" one at random can make a defendant much more likely to be receive a jail sentence. This yields a quasi-experiment: some people get sent to jail essentially because they had the bad luck to be sent to a harsher courtroom, while others walk free because of a lucky draw.[11]

I use the fact that defendants are randomly assigned to these harsher or more lenient courtrooms to instrument for whether they receive a jail sentence at all.[12] Table K.1 in Appendix C presents first-stage results, demonstrating that courtroom assignment really does affect jail sentencing. As an illustrative example, in 2012 the 15 different courtrooms sentenced people to jail at extremely different rates, ranging from 47% to 61%. This was not driven by differences in defendant characteristics, as defendants were randomly assigned to courtrooms, yielding very similar caseloads across the different courtrooms.[13]

When I use courtroom assignment as an instrument for housemates being jailed, I find estimates of jail on voter turnout that are substantively similar to the main estimates in Section 3.3.3, though somewhat noisier. Figure 3.1 presents IV estimates of housemate jail

[11]See Mueller-Smith (2014) and White (2015) for other designs taking advantage of random courtroom assignment in Harris County in particular; see Kling (2006) and Green and Winik (2010) for similar IV designs in other contexts.

[12]Because I am interested in the behavior of registered voters who live with defendants, it is more accurate to say that I use housemates' courtroom assignment to instrument for whether a voter sees their housemate sent to jail.

[13]For balance tests and more evidence of genuinely random assignment in this data, see Mueller-Smith (2014) and White (2015).

on 2012 voter turnout from several specifications (described in further detail in Appendix C). In all cases, the estimates are negative and similar in size to (or somewhat larger than) the OLS estimates. This lends further support to the causal interpretation of these estimates.

3.4 Interpretation

What can these results tell us about the net effect of criminal justice contact on friends and family members? In this section, I first discuss how common the examined experience (watching a household member go to jail over misdemeanor crimes) is, and how many voters may be deterred by having this experience, both in the examined county and across the United States. Then, I discuss what these results can and cannot tell us about the broader set of experiences that make up proximal contact.

Within the area of this study (Harris County, Texas), I identified over 87,000 registered voters who saw a household member face misdemeanor charges between 2009 and 2012, with over 38,000 watching that person go to jail. This is likely an undercount of the number of people who actually saw loved ones arrested, since it does not account for people who are not registered, who do not live with their partner or family member, or who live with a defendant who did not have a valid address on record with the court. Still, applying the estimated three-percentage-point decrease in turnout to the families in the sample who saw members jailed suggests that over a thousand voters stayed home from the polls in this county due to brief misdemeanor jail sentences.

It is harder to know how many people could be proximally affected by misdemeanor jail sentences nationwide. There is little national data on misdemeanor charges or jail sentencing, so I present a back-of-the-envelope calculation based on jail admissions data from the Bureau of Justice Statistics. The assumptions made are discussed further in the SI. Briefly, I begin with the 11.6 million people estimated to have been admitted to local jails in 2012, and adjust that number to reflect the possibility of individual arrestees being double-counted, the proportion of jail inmates that have not been convicted of misdemeanors (either because

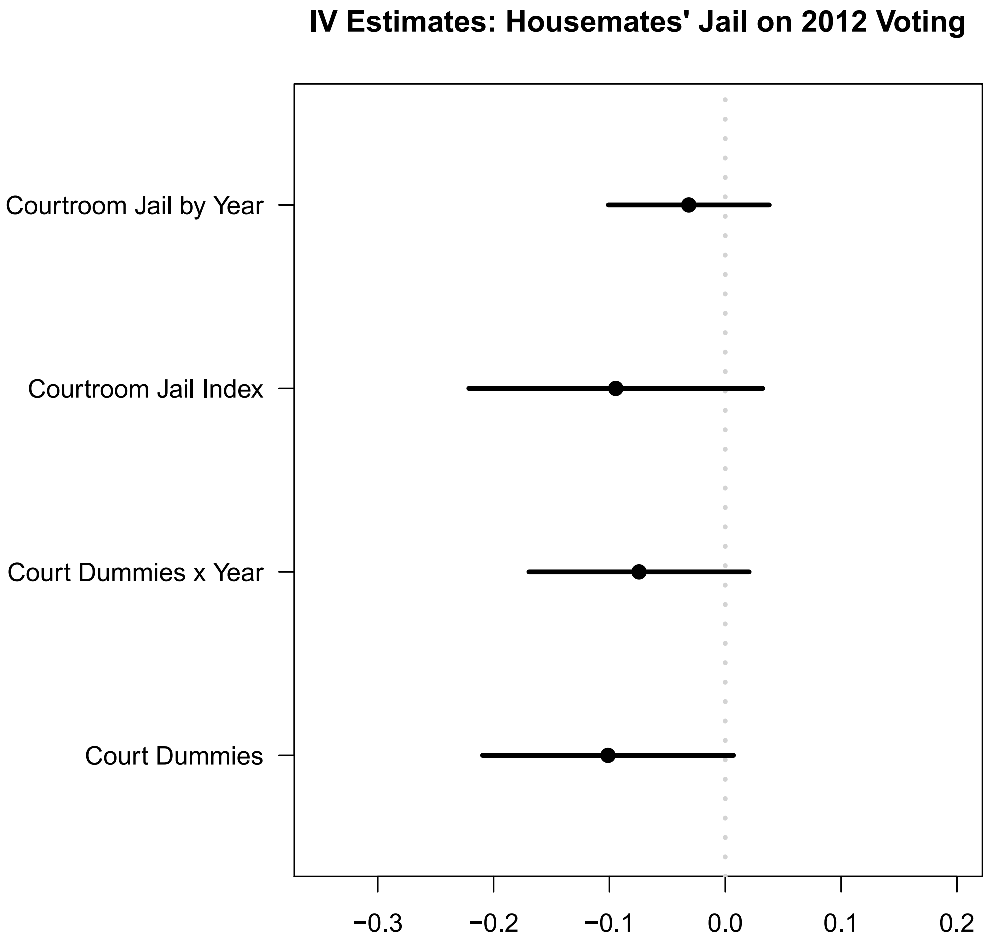


Figure 3.1: Using random courtroom assignment as an instrument for whether a household member goes to jail.

they are awaiting trial or because they are serving felony sentences in local jails), and the proportion of misdemeanants that are first-time offenders. This yields an estimate of over 800,000 first-time misdemeanor defendants sentenced to time in local jails each year. If close to half of these defendants live with a registered voter (as seen in Harris County) and these voters are demobilized at the same rates estimated in Section 3.3.3, this could translate into over 10,000 voters across the US staying home from the polls after seeing a loved one jailed for a misdemeanor crime in a given year. Table 3.3 suggests that this demobilization can persist for multiple years, so the true effect may be several times that size.

These estimates are not meant to capture the entire set of spillover effects that could occur due to criminal justice contact. Instead, they capture one very narrow part of the effect. These estimates do not include people whose loved ones faced felony (not misdemeanor) convictions, or who don't live with their loved one. They do not address the question of cumulative effects, as people continue not voting over time or see more people in their lives jailed. They do not include longer-term effects driven by people who weren't registered voters, but might have registered in the future if they hadn't been deterred by proximal contact.[14]

Most importantly, these estimates capture the additional effect of seeing a household member jailed *after* already having seen them face charges. This is the incremental, additional effect of the jail term among people who have already borne the costs of having a household member arrested and charged with a misdemeanor. That I still find an effect of jail among people who have experienced other forms of criminal justice contact underscores the continuing importance of incarceration in people's political lives. Even short jail sentences loom large in the lives of those close to the incarcerated.

[14] This consideration may be especially important for the children of incarcerated parents, who face early political socialization around the criminal legal system.

3.5 Conclusion

With close to 25% of the American public carrying the stigma of a criminal record, millions of Americans know someone who has been arrested, convicted of a crime, or incarcerated. These experiences are particularly pervasive for African-Americans and for low-income people. If even a fraction of the family members or close friends of people with criminal justice contact are deterred from voting, this could translate into massive distortions in the makeup of the American electorate. This could lead to policy feedbacks in which the most-heavily-targeted communities withdraw from political life, and incarceration decisions continue to be made without their political input (Bruch, Ferree, and Soss 2010; Campbell 2002).

This paper has measured just a sliver of possible effects from proximal contact, focusing on the immediate housemates of people jailed for misdemeanor crimes and estimating the additional effect of jail after having already experienced arrest. Even with such a narrow focus, I find substantial demobilizing effects: people become several percentage points less likely to vote in the next presidential election if their household member is sentenced to jail. These results are robust to a variety of specifications, and are similar to estimates derived from a quasi-experimental approach based on random courtroom assignment.

It is worth noting that this demobilization is occurring among people who have not themselves committed a crime.[15] Such "collateral consequences" raise normative questions about the representativeness of American democracy.

That said, voter turnout may be only one part of the proximal contact story. As Walker (2014) and others point out, voting is only one piece of a range of political activities available to the family and household members of people trapped in the criminal justice system. The question, then, is whether voting is only the tip of the iceberg, indicating a broader

[15] I have not directly measured whether the housemates of people who are jailed become themselves more likely to be arrested, convicted, or jailed, but such a mechanism is unlikely to drive the entire estimated effect; if such a mechanism were in play, we should not see an effect for voters whose housemates faced charges in 2012 (as this leaves very little time for housemates to see the case play out and then go get arrested).

withdrawal from civic and political life: lower willingness to protest, to contact government officials, or even to request needed services (Lerman and Weaver 2013). Or is voting instead one rare case amidst a swell of political activity by people with proximal contact? Future work will seek to collect the other outcome measures necessary to estimate the full extent of proximal contact's political effects.

A | Appendix to Chapter 1: Testing Assumptions/ Robustness Tests

Due to the limitations of the Catalist database and the ACS, I do not have reliable voter turnout data for the years prior to 2006, which makes it difficult to test the assumptions of the difference-in-differences setup. However, in this section I present several tests of the assumptions based on the available data. I verify that pre-treatment trends in turnout do not predict treatment, I run a placebo test to demonstrate that my approach does not find treatment effects where none should exist, and I use synthetic matching to address concerns that control units may not be similar enough to treated units.

Checking pre-treatment trends

First, we might worry that places that already had steeper growth in Latino turnout might have also received the SC treatment for some reason, such that the effect I observe is not actually driven by immigration enforcement. To test for this possibility, I use the best available data from 2002 and 2006 to check whether the pre-treatment turnout trends predict treatment. I construct 2002 voter turnout data slightly differently than the 2006 and 2010 data; I use CVAP estimates from the 2000 Census because the ACS did not produce estimates of Latino CVAP prior to 2006 (and then interpolate using 2000 Census and 2006 ACS data to produce 2002 estimates).[1] Further, Catalist began collecting voter files to construct their database in 2006, so it is possible that their turnout data for prior years is incomplete due to people voting and then being removed from the voter rolls before 2006.

[1] These estimates are available for about half of the jurisdictions in the main dataset.

Both numerator and denominator are biased by an unknown amount, so it is not clear in which direction the turnout estimates will be biased.

Table A.1 presents the results of a regression of the treatment variable onto the 2002-2006 change in Latino turnout in each state cluster.[2] There is no evidence that pre-2006 time trends, at least for the limited period for which there is data, predict treatment.

Table A.1: Predicting treatment with prior Latino turnout trends (including all jurisdictions)

2006 - 2002 Latino turnout (percentage points)	−0.005
	(0.004)
Constant	0.131**
	(0.051)
Observations	49
R^2	0.025
Adjusted R^2	0.004
Note:	*p<0.1; **p<0.05; ***p<0.01

Next, I use another dataset to verify the parallel-trends assumption. I use Latino citizen voter turnout rates from the Current Population Survey for elections from 1996 to 2006, and check whether these turnout rates predict treatment (enrollment in the Secure Communities Program). This analysis is shown in Table A.2.

I calculate Latino citizen turnout rates for each cluster as follows: I restrict the dataset to jurisdictions that are included in my dataset for the above analyses (dropping places in each state that voluntarily enrolled in SC). Then, for each "cluster" (roughly a state, but with self-selected counties dropped), I calculate the percentage of Latino citizens of voting

[2]For the purposes of this test, I focus on full states, rather than on "state" clusters that omit jurisdictions that selected into the SC program. I think this is more realistic, as treatment was determined at the state level. However, the results do not change substantively if I omit the jurisdictions that voluntarily enrolled in SC, as in the dataset used for the main analysis; there is still no significant relationship between 2002-2006 change in turnout and treatment at the state level. Similarly, no significant relationship emerges if I weight the regression by the number of units in the state pre-collapse, or by the cluster's 2002 Latino population. Finally, no significant relationship emerges if I run the same analysis at the county level rather than the state.

age that report having turned out in the most recent election, using the survey weights provided with the survey. The November CPS supplement asks about the general election that has just taken place, so for some years it is the midterm congressional election, and in others it is the presidential election.

Some clusters contained very few respondents, so the turnout estimates were quite noisy. In Column (1) of Table A.2, I have dropped all clusters with fewer than 30 respondents; Column (2) contains all clusters. In both cases, there is no evidence that previous years' turnout rates predicted treatment, which supports the parallel trends assumption. Figure A plots the Latino turnout trends of states with and without treated units.

Placebo test: 2002-2006

Having constructed Latino turnout estimates from 2002 for some of the jurisdictions from the main dataset, I can also run a placebo test to check whether there is evidence of a "treatment effect" before the treatment actually took place. Table A.3 replicates the main analysis in the paper, the models from columns 4 and 5 of table 1.4, for the turnout change from 2002-2006 instead of 2006-2010. As discussed above, this data covers a limited number of places and is likely an undercount of voters, but is the best data available. I do not find a comparable treatment effect for 2006-2010.

Synthetic control

Next, I address concerns about the comparability of treatment and control units, and the possibility of extreme counterfactuals, by using synthetic matching (Abadie, Diamond, and Hainmueller 2010). I use this approach to construct a "synthetic control" for each of the treated clusters that is a weighted average of other clusters in the dataset.[3] I use the available pre-treatment data – the change in Latino voter turnout in each cluster from 2002-2006 – to create matches that should have similar time trends in voter turnout. This process

[3] I perform this matching using the "Synth" package for R (Abadie, Diamond, and Hainmueller 2011).

Table A.2: Predicting treatment with prior turnout from CPS

	SC treatment	
	Better data	All states
	(1)	(2)
Latino Citizen Turnout, 1996	25.702	6.761*
	(20.701)	(3.725)
Latino Citizen Turnout, 1998	−10.784	0.046
	(10.212)	(3.448)
Latino Citizen Turnout, 2000	21.766*	8.069
	(12.640)	(5.634)
Latino Citizen Turnout, 2002	3.167	−2.645
	(7.831)	(5.451)
Latino Citizen Turnout, 2004	2.956	−2.796
	(10.900)	(3.683)
Latino Citizen Turnout, 2006	−35.302	−4.941
	(25.346)	(3.553)
Constant	−13.881*	−6.122**
	(7.193)	(3.123)
Observations	32	50
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table A.3: Placebo test: main analysis replicated on 2002-2006 treatment change

	Turnout change, 2006-2010	
	(1)	(2)
Treatment (Involuntary SC enrollment)	-0.038	-0.043
	(0.045)	(0.038)
Senate election 2006	0.063^{**}	0.064^{**}
	(0.026)	(0.027)
Senate election 2002	-0.007	-0.005
	(0.027)	(0.027)
Governor election 2006	0.043^{*}	0.036
	(0.024)	(0.024)
Constant	0.084^{**}	0.089^{**}
	(0.038)	(0.039)
Observations	1548	1548
Note:	*p<0.1; **p<0.05; ***p<0.01	

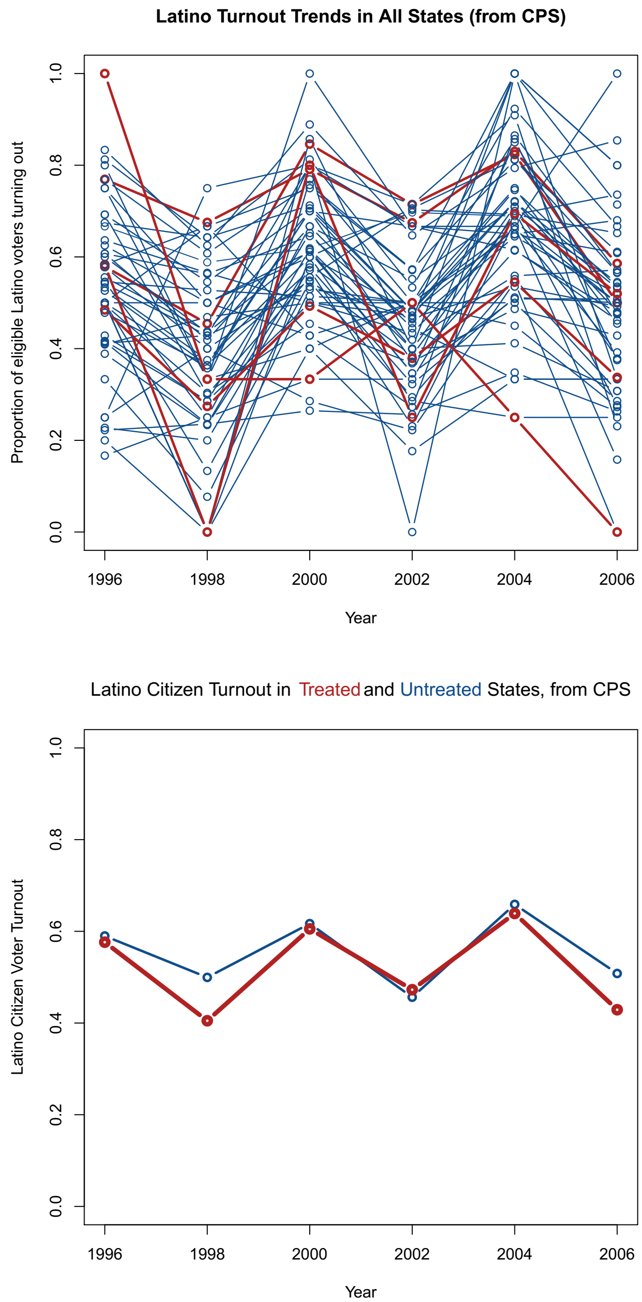


Figure A.1: Latino turnout trends in the Current Population Survey. Treated states represented with thicker lines in both plots.

would be improved by the inclusion of more historical turnout data, but even with limited data it serves as a check on the difference-in-differences results.

I draw from the untreated clusters (that is, states without full pre-election SC enrollment, with any voluntarily-enrolled jurisdictions dropped) to construct matches for each of the treated clusters. For each cluster, I then compare the change in Latino turnout from 2006 to 2010 between the treated and synthetic control unit. The difference between these changes is taken as the treatment effect of Secure Communities enrollment. I take the mean of all treated clusters' estimates to find an overall estimate of 1.4 percentage points. This is slightly lower than the 2-3 percentage points estimated in the main analysis in Table 1.4, but is in the same direction and is of comparable magnitude. As shown in Table A.4, a mean weighted by the 2006 Latino population of each cluster yields a point estimate of 2.9 percentage points, somewhat larger than the main estimate.[4]

names	ddests
Delaware	0.0415
Florida	0.0041
Virginia	0.0262
Texas	-0.0114
West Virginia	0.0084
Mean	0.0138
Population-weighted Mean	0.0290
Unit-weighted Mean	0.0111

Table A.4: Difference-in-difference estimates, compared to synthetic versions of each cluster

The resulting weights for each synthetic match are available on request, and will be included in the online supplemental information. I have not attempted to quantify the uncertainty around the estimate produced via synthetic matching, as it is not immediately clear how to do so with multiple treated units. The results are fairly similar to the OLS estimates presented in Section 1.4, and so I rely on the better-understood OLS standard errors, as do other papers using this approach as a check (Hall 2013).

[4]It may seem that Texas, the only cluster with a negative point estimate, should be weighted more heavily. But recall that the population used is the Latino population in the cluster after having dropped places that voluntarily selected into the program. Texas' major population centers were enrolled into the SC program quite early.

B | Appendix to Chapter 1: Analysis of Record Submissions

One mechanism discussed above was direct experience with deportation: citizens might observe people they know being deported, and change their political behavior in response. This is unlikely to explain my results, as I focus on places that enrolled in the program only a few months before the 2010 election. However, I use available ICE data to ensure that program implementation in those few months does not explain the turnout results presented here.

Relatively few people would have been deported due to the Secure Communities program at the time of the 2010 election, but there is some variation in the number of people whose fingerprints were submitted to ICE to check their immigration status. In this section, I explore whether places with different numbers of fingerprint submissions had different political responses.

To examine whether program implementation affected changes in turnout, I split the treated units into those with high (above-median) and low (below-median) numbers of fingerprint submissions to ICE, and estimate the SC treatment effect in each subset. ICE provided data on submissions from the time of program activation until August 2012, so I adjusted them to reflect the amount of time the program had actually been in effect by the time of the 2010 election. I assumed that submissions were uniform across the time period reported, and simply multiplied the total number of submissions by the fraction of activated time that fell before the 2010 election.[1] I divided the treated portion of the sample into units

[1] This may not be an accurate assumption, but there was no more precise data available on the timing of fingerprint submissions. Still, I use this assumption only to divide the sample into above- and below-median

that had sent more than 74 (the sample median) records to ICE prior to the 2010 election, and those that had submitted fewer than that. These record submissions represent the upper bound of people who might have faced deportation due to the Secure Communities program in that jurisdiction– not everyone whose fingerprints were submitted would actually have been deported, and very few people were likely deported before the 2010 election.

Table B.1: Treatment effects by number of fingerprint submissions (Robust clustered SE's)

	Dependent variable:	
	Turnout change, 2006-2010	
	High submissions	Low submissions
	(1)	(2)
Treatment (Involuntary SC enrollment)	0.017***	0.031***
	(0.005)	(0.005)
Senate election 2010	0.004	0.004
	(0.007)	(0.007)
Senate election 2006	−0.031***	−0.031***
	(0.005)	(0.005)
Governor election 2006	−0.003	−0.003
	(0.004)	(0.005)
Governor election 2006	−0.010	−0.010*
	(0.006)	(0.006)
Constant	0.028***	0.029***
	(0.009)	(0.010)
Observations	2,397	2,398
R^2	0.051	0.054
Adjusted R^2	0.049	0.052
Note:		*p<0.1; **p<0.05; ***p<0.01

jurisdictions on submissions, so even a rough measure should provide a reasonable division. Further, this analysis is used only to ensure that Secure Communities enrollment is not driving the main results, and this assumption should, if anything, overestimate the number of people who had direct experience with the program prior to the election.

Table B.1 shows the results of this analysis. They support the assertion that personal experiences with deportation do not drive the turnout effects reported in the main paper. If individual people were turning out to vote because someone they knew personally was in danger of deportation, we would expect more record submissions to be associated with more votes and thus a bigger turnout effect. This is decidedly not the case; as seen in Table 3, higher-submission communities do not show a larger treatment effect than low-submission communities.

It should be noted that this is an observational analysis, and we might think that places with many submissions are different from places with few submissions in many other ways that could affect turnout and the way the SC program was implemented and perceived. One such concern is population, but the same pattern of results appears when the analysis is performed with population-adjusted counts of record submissions (submissions per 1,000 residents, or per 1,000 Latino citizens).

C | Appendix to Chapter 1: Additional CCES Analysis

Table C.1: Respondent-reported campaign/activist contact, 2006 (Latinos)

	(1)	(2)	(3)
SC Treatment	−0.059	−0.054	−0.120
	(0.108)	(0.110)	(0.114)
Registered Voter		0.422***	0.428***
		(0.072)	(0.074)
Gender: Female		−0.008	0.009
		(0.058)	(0.056)
Age		0.008***	0.008***
		(0.002)	(0.002)
Party ID:Republican			0.121*
			(0.067)
Party ID: Independent			0.028
			(0.056)
Party ID: Other			0.074
			(0.148)
Senate Election 2006			0.053
			(0.063)
Governor Election 2006			0.064
			(0.058)
Constant	0.706***	−0.029	−0.154
	(0.027)	(0.115)	(0.134)
Observations	337	336	336
R^2	0.001	0.128	0.143
Adjusted R^2	−0.002	0.117	0.119
Note:			*p<0.1; **p<0.05; ***p<0.01

Table C.2: Respondent-reported campaign/activist contact, 2010 (non-Latinos)

	(1)	(2)	(3)
SC Treatment	0.023	0.008	0.006
	(0.034)	(0.022)	(0.024)
Registered Voter		0.297***	0.245***
		(0.008)	(0.008)
Gender: Female		0.009***	0.008***
		(0.0002)	(0.0002)
Age		−0.046***	−0.035***
		(0.007)	(0.007)
Party ID:Republican			0.031***
			(0.009)
Party ID: Independent			−0.001
			(0.008)
Party ID: Other			0.032*
			(0.019)
Party ID: Not Sure			−0.221***
			(0.012)
Senate Election 2010			0.052***
			(0.015)
Governor Election 2010			0.060***
			(0.015)
Constant	0.626***	−0.031***	−0.051***
	(0.009)	(0.010)	(0.019)
Observations	23,059	23,059	23,059
R^2	0.0001	0.152	0.171
Adjusted R^2	0.0001	0.151	0.171
Note:			*p<0.1; **p<0.05; ***p<0.01

D | Appendix to Chapter 2: 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 checks to make sure the data looks as if cases were indeed assigned to courtrooms without regard to defendant or case characteristics.

Figure 2.1 in the paper demonstrates that the 15 courtrooms appear to have fairly similar caseloads on defendant and case characteristics, such as race, gender, and charge severity. Next, I test for that similarity more formally in several ways.

I begin by regressing several key pre-treatment characteristics onto courtroom assignment dummies.[1] 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 D.1 then presents F-statistics from these models. For pre-assignment characteristics like age or sex, the F-statistics are relatively small (some represent technical rejection of the null hypothesis, but represent very small substantive differences on these characteristics). 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 2.1, 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

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

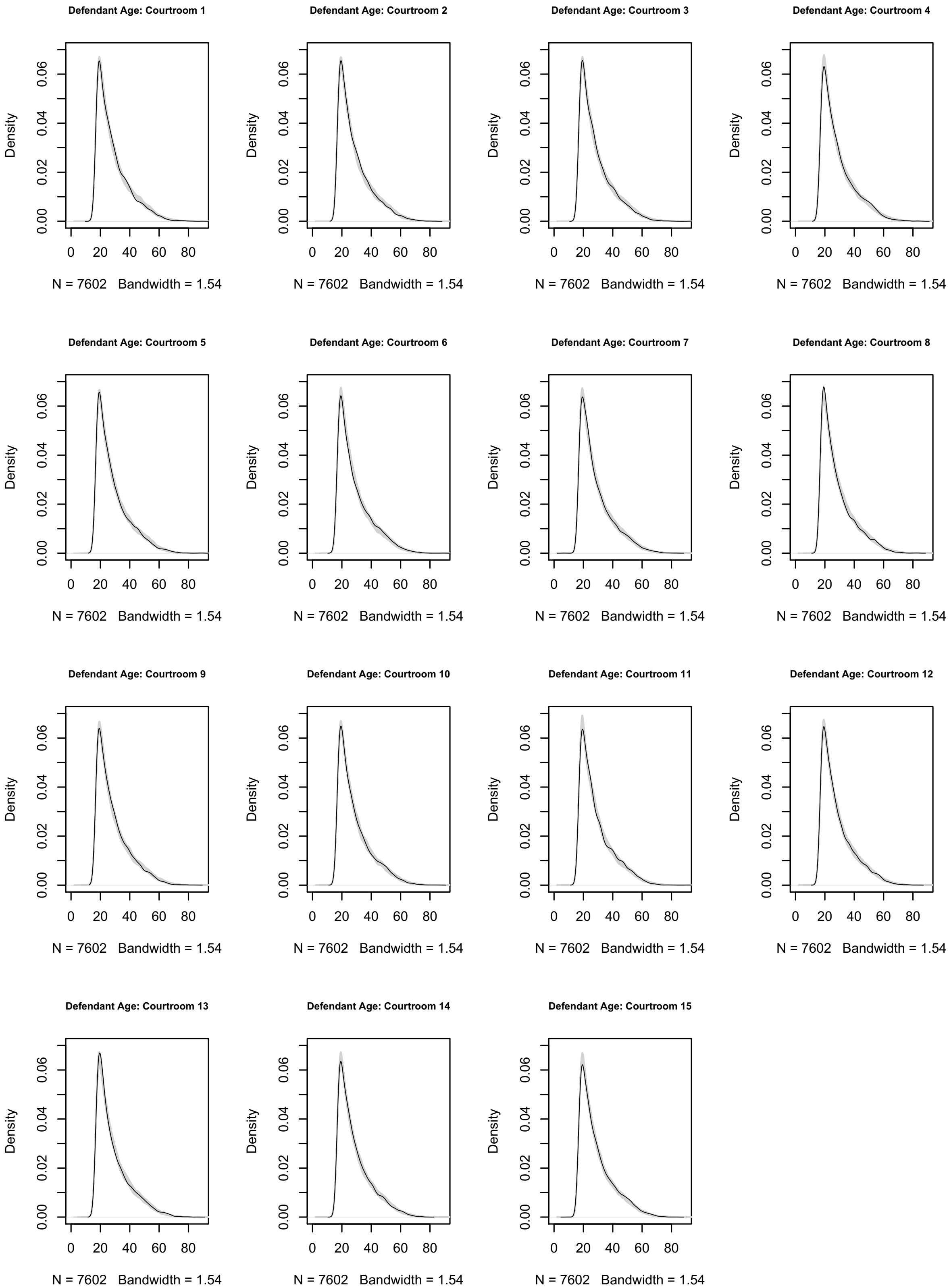
harshness.

Table D.1: Testing Court Caseload Differences

Variable	F-Statistic
Male	1.27
Black	1.4
Age	1.39
Conviction	7.77
Fine	23.54
Probation	11.01
Jail	7.53
Jail Time	14.12

Next, I do some permutation tests for the main continuous pre-treatment variable that is available in these court records: age.[2] We might worry that courtrooms' caseloads would have the same mean defendant age, but perhaps have different distributions. In Figure D, 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 data 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.

[2]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 = 7602 Bandwidth = 1.54

E | Appendix to Chapter 2: Table of Courtroom Caseloads

Table E.1: Defendant Characteristics by Courtroom, 2008-2012

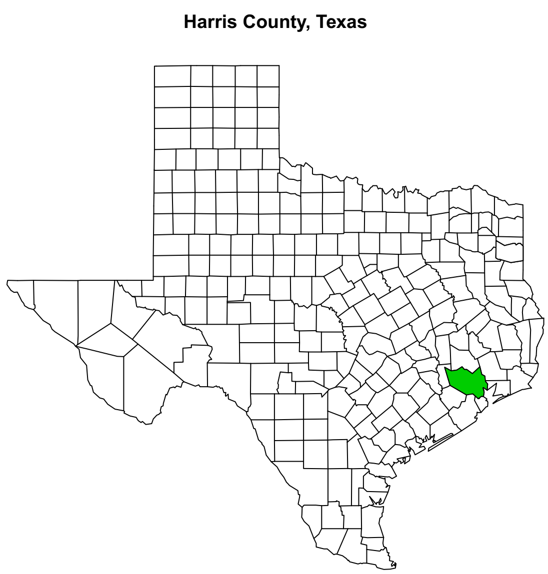
Court	Total	Pct. Male	Pct. Black	Pct.>30	Pct. Jailed	Pct.Voted2012
1	7,606	0.697	0.268	0.338	0.517	0.131
2	7,558	0.694	0.277	0.342	0.587	0.121
3	7,453	0.697	0.285	0.340	0.513	0.125
4	7,604	0.701	0.277	0.348	0.533	0.128
5	7,570	0.707	0.280	0.340	0.537	0.128
6	7,545	0.697	0.282	0.355	0.502	0.123
7	7,442	0.702	0.274	0.343	0.497	0.125
8	7,591	0.691	0.273	0.333	0.551	0.132
9	7,674	0.691	0.283	0.342	0.528	0.131
10	7,615	0.698	0.275	0.344	0.545	0.129
11	7,691	0.687	0.277	0.348	0.530	0.119
12	7,512	0.694	0.286	0.341	0.527	0.127
13	7,513	0.691	0.268	0.340	0.534	0.125
14	7,565	0.693	0.284	0.346	0.555	0.129
15	7,476	0.692	0.280	0.353	0.528	0.130

F | Appendix to Chapter 2: Regression Table from Figure 2.2

Table F.1: IV estimates: Jail sentences on voting, by race

	Dependent variable:	
	vote2012	
	Black Defendants	White Defendants
	(1)	(2)
jail	-0.134^{**}	-0.006
	(0.056)	(0.036)
Constant	0.263^{***}	0.091^{***}
	(0.031)	(0.022)
Year dummies	Yes	Yes
First Stage F-Statistic	52.99	64.55
Observations	31,524	77,779
Adjusted R^2	0.034	0.003
Note:		*p<0.1; **p<0.05; ***p<0.01

G | Appendix to Chapter 2: Map of Harris County



H | Appendix to Chapter 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 appendix, 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.[1] 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.[2] 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 Anglo defendants. When running the analysis this way, I find evidence of substantial demobilization among Hispanic defendants. The IV estimates in column 2 of Table H.1 indicate that jail caused an almost 11-percentage-point drop in turnout for Hispanic defendants. Column 4 suggests a small, but insignificant

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

[2] A 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.

positive effect for Anglo 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 Anglo defendants' jail effects.[3]

Table H.1: 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		-0.107^{**}		-0.013
		(0.042)		(0.044)
Constant	0.000	0.118^{***}	-0.000	0.119^{***}
	(0.058)	(0.028)	(0.039)	(0.025)
Year dummies	Yes	Yes	Yes	Yes
Observations	29,582	29,582	48,205	48,205
F Statistic	29.824^{***}		56.270^{***}	
Note:			*p<0.1; **p<0.05; ***p<0.01	

[3]See the SI for this table.

I | Appendix to Chapter 3: Checking for Household Relationships

This project assumes that voters and criminal defendants living at the same geographic coordinates (according to public records) know each other, and will often have some sort of familial or romantic relationship. I check this by drawing a random sample of observations from my dataset and verifying that many of the households do appear to include such relationships.

I first run this check for households of 2012 defendants. I randomly sample 100 rows from the nearly 18,000-person sample. Then, for each row, I investigate the relationship between the registered voter and the criminal defendant who live at this address. First, I check whether they share a last name; this is a fairly conservative measure of family relationships, as many people may be related but not share a surname (and the probability of coincidences in which people report the same residential address and share a last name but are not related seems low). 47% of households contain voters and defendants that share a last name. This is quite high, considering that many romantic partners in this sample may be unmarried (Western 2006).

Next, for households that do not share a last name, I look for other evidence of connections in public records of marriages and births. By looking up defendants' and voters' names in the Texas state birth index from 1907-1993 (searchable through Familysearch.org), I find evidence of parent-child relationships, shared children, or shared parents (sibling relationships) for an additional 10 % of observations. A number of other observations appear to be related (age gaps suggest parent-child or parent-grandchild relationships, and naming similarities suggest a familial link), but could not be verified using public records and are not counted here.

Thus, I find strong evidence of close familial relationships for 57 % of observations. This is quite high, as it does not capture people cohabiting without children in common, and may also not capture parent-child relationships for people born outside Texas, not to mention close friends or other relatives. To the extent that some of the remaining observations introduce error to the analyses by inaccurately classifying strangers or neighbors as household members, this should make the estimates in the paper conservative ones.

J Appendix to Chapter 3: Including Fixed Effects and Census Data

Table J.1: OLS estimates with geographic fixed effects

	Dependent variable:		
	vote2012		
	(1)	(2)	(3)
Household Member Jailed	-0.021^{***}	-0.023^{***}	-0.023^{***}
	(0.003)	(0.003)	(0.003)
2008 Turnout	0.471^{***}	0.473^{***}	0.473^{***}
	(0.003)	(0.003)	(0.003)
Voter Male	-0.043^{***}	-0.043^{***}	-0.043^{***}
	(0.003)	(0.003)	(0.003)
Voter Age (Years)	0.001^{***}	0.001^{***}	0.001^{***}
	(0.0001)	(0.0001)	(0.0001)
Constant	0.212^{***}	0.252^{***}	0.252^{***}
	(0.065)	(0.009)	(0.009)
Zip Code Fixed Effects	*X*		
State House District Fixed Effects		*X*	
State Senate District Fixed Effects			*X*
Observations	86,241	86,241	86,241
R^2	0.248	0.245	0.245
Adjusted R^2	0.247	0.245	0.245
Note:		*p<0.1; **p<0.05; ***p<0.01	

Table J.2: OLS estimates including census tract characteristics

	Dependent variable:
	vote2012
Household Member Jailed	-0.021^{***}
	(0.003)
2008 Turnout	0.471^{***}
	(0.003)
Voter Male	-0.043^{***}
	(0.003)
Voter Age (Years)	0.001^{***}
	(0.0001)
Tract Population	0.00000^{**}
	(0.00000)
Tract Percent Latino	-0.116^{***}
	(0.009)
Tract Percent Black	0.016^{**}
	(0.007)
Tract Average Household Size	0.001
	(0.004)
Constant	0.293^{***}
	(0.012)
Observations	86,241
R^2	0.246
Adjusted R^2	0.246
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

K | Appendix to Chapter 3: Instrumental Variables

Table K.1 shows first-stage results for several different ways of constructing the instrument. Column 1 shows the regression of the treatment of interest (household member jail) onto all but one of the individual dummy variables for courtroom assignment (as in Green and Winik (2010)). Column 2 takes the same approach, but interacts the courtroom dummies with the filing year of the cases, to account for possible non-monotonicity (some courtrooms may be more harsh than others one year, but not the next)(Mueller-Smith 2014). Note that courtroom-year interaction terms are omitted from the table for parsimony; full table available on request. Column 3 presents the first stage using the courtroom's jail-sentencing rate as an instrument (that is, I calculated the proportion of all first-time misdemeanor defendants that were sentenced to jail by that courtroom, regardless of whether those defendants lived with any registered voters or not), and Column 4 does the same thing but calculates jail sentencing rates within-year (again due to non-monotonicity concerns)(Di Tella and Schargrodsky 2013). In all cases, the F-statistic is above 10. In the supplemental appendix, I will also construct leave-one-out courtroom sentencing means to use as instruments.

Table K.1: First Stage: Predicting Jail with Courtroom Assignment

	Dependent variable:			
	anyjail			
	(1)	(2)	(3)	(4)
crt_2	0.099***	−28.093**		
	(0.009)	(13.197)		
crt_3	0.029***	18.525		
	(0.009)	(13.223)		
crt_4	0.027***	21.122		
	(0.009)	(13.117)		
crt_5	0.034***	−15.025		
	(0.009)	(13.395)		
crt_6	0.001	−10.496		
	(0.009)	(13.280)		
crt_7	0.015	−11.364		
	(0.009)	(13.253)		
crt_8	0.043***	−2.340		
	(0.009)	(13.022)		
crt_9	0.038***	−0.200		
	(0.009)	(13.447)		
crt_10	0.050***	−35.367***		
	(0.009)	(13.169)		
crt_11	0.042***	33.143**		
	(0.009)	(13.289)		
crt_12	0.024***	35.237***		
	(0.009)	(13.158)		
crt_13	0.055***	−12.049		
	(0.009)	(13.160)		
crt_14	0.068***	−59.935***		
	(0.009)	(13.207)		
crt_15	0.011	−17.943		
	(0.009)	(13.239)		
fyear		−0.004		
		(0.005)		
crtjailavgd			1.104***	
			(0.080)	
crtjailavg1				1.140***
				(0.046)
Constant	0.401***	7.779	−0.133***	−0.152***
	(0.007)	(9.325)	(0.041)	(0.024)
Year-Courtroom Interactions		X		
Observations	87,591	87,591	87,591	87,591
R^2	0.003	0.004	0.002	0.007
F Statistic	16.114*** (df = 14; 87576	11.446***	190.255***	617.622***
Note:				*p<0.1; **p<0.05; ***p<0.01

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