

When Threat Mobilizes: Immigration Enforcement and Latino Voter Turnout

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Abstract

Immigration enforcement, and deportation in particular, has been shown to have social and psychological effects on the non-deported as well, but its political effects have gone largely unexamined. I use the staggered implementation of Secure Communities, an information-sharing program between the federal government and local law enforcement, to estimate the effect of stricter immigration enforcement on Latino voter turnout. A difference-in-differences analysis indicates that enrollment in Secure Communities led to an increase in county-level Latino voter turnout of two to three percentage points. This relatively large effect may be due to greater Latino activism in the wake of program implementation, or to individuals responding to particular police interactions. These results extend the existing literature on mobilization in response to threat, demonstrate that policies can have far-reaching and unexpected political implications, and suggest that the current immigration debate may have major consequences for the future makeup of the American electorate.

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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 they 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 may observe the immigration-enforcement process second-hand although they themselves are not targeted for removal.² 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 (Lopez, Gonzalez-Barrera and Motel, 2011). Another survey found that of Latino registered voters in five battleground states, 30% knew someone who had been deported or detained, and 55% knew someone who was undocumented. Eleven percent reported that a family member was undocumented (Latino Decisions / America's Voice, 2012). This suggests that a large number of potential voters are being impacted by enforcement policies. 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 deportation could affect their political behavior. Further, activist mobilization in the wake of deportations could mobilize voters who are not personally aware of them. However, there is little research linking this experience to voter turnout.³

²Latino citizens are not the only ones who could be exposed to 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).

³There 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

Much has been written about changes in U.S. immigration enforcement tactics. Some research has even discussed the social and economic effects of these policy changes on the families and neighbors of those targeted. But very little has been said about the political impact of punitive immigration policies. If they shape the voter behavior of people who are not normally considered targets of these policies – and are not eligible for deportation – there are major implications for democratic representation, and for our understanding of policy and policy feedbacks.

This paper seeks to measure the impact 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 led to large increases in deportations and removals in counties where it was implemented, also 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.

This result contributes to our understanding of how groups can be mobilized in response to threat, demonstrating that political responses can arise even when voters themselves are not the targets of threatening policy. Immigration enforcement is shown not only to have unintended consequences, but to have “second-hand” effects on people who were not actually targeted by the policy. This concept may be applicable to a range of policy arenas, such as policing.

This finding also adds to our knowledge about immigration enforcement specifically,

shaping political behavior even in the absence of political rhetoric about one party’s hostility toward Latinos, simply because of actual government actions.

by demonstrating that this enforcement does have political implications. As discussed in Section 2, it was not obvious *a priori* that there would be such an effect, or that it would necessarily run in the direction of increasing turnout. 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. This paper suggests that the partisan dynamics of immigration enforcement are not straightforward, and raises questions of how immigration policy debates and a growing Latino citizen population will reshape the electoral landscape in the years to come.

The rest of the paper proceeds as follows: Section 2 describes recent changes in immigration policy and existing research on Latino turnout and policy feedbacks that may help to predict the effects of these changes. Section 3 discusses the data and methodological approach, Section 4 presents results, Section 5 explores possible mechanisms underlying the results, and Section 6 concludes.

2 Background

2.1 Spillover Effects from Immigration Enforcement

There are several reasons to expect political “spillovers” from immigration policies such as Secure Communities, especially in recent elections. As discussed in the introduction, changes in immigration policy over the past few decades have led to the removal of many people and exposed many U.S. citizens to the immigration system through the removal of friends or family members. This exposure to punitive action by the government seems likely to shape the way people approach governmental interactions in the future, as has been seen after experiences with the GI Bill or Social Security (Mettler and Soss, 2004;

Campbell, 2002). Though most work on such “policy feedbacks” has focused on the people directly targeted or benefited by public policy, several recent studies explore the spillover effects of another punitive policy, incarceration, on the friends and neighbors of the incarcerated (Bowers and Preuhs, 2009; Burch, 2011). A similar mechanism is posited in the case of immigration.

More than a 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 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, N.d.). These people are more likely to have established family and community ties in the U.S. than recent migrants, and their removal seems 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 may represent repeated deportations of the same individuals, but it still appears that many people are being deported from an increasingly broad geographic area.⁴

⁴If this is the case, then it could be that fewer families/social groups than expected are observing deportations, which could diminish the effect I seek to measure. But see the discussion in section 1 of survey evidence on this— it is fairly common for Latino citizens to know people who have been or are at risk of being

This is a strikingly large policy shift, and deserves further academic consideration.

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. 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 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. But other things are likely to happen in the 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 can provide a site for this publicity, as has happened in the wake of other immigration crackdowns (Hagan, Rodriguez and Castro, 2011). It is worth considering this entire “treatment” when discussing mechanisms by which the program could affect turnout.

deported.

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, foreshadow this study's finding. 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 racially-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 mirrors this paper's main finding.

Two 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 deportations 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.

There are parallel findings for other identity groups: Cho, Gimpel and Wu (2006) finds that Arab-American voter registration seems to increase in response to threats such as the PATRIOT Act. In that case, socioeconomic status seems to matter, as people who are least likely to be directly threatened by government actions – well-educated, English-speaking Arab-American citizens – are most likely to be aware of such policies. The authors suggest that threats “provide the motivation to participate from those who have the ability to participate, but heretofore have chosen not to do so.”

The studies just described suggest that immigration enforcement could increase voter turnout. Several strands of literature suggest mechanisms by which this increase could happen. First, implementation of harsher immigration enforcement measures could activate people’s group memberships. In some communities, people 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)).⁵

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 protest against the policy, or 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

⁵This could also 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.

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. 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.

The psychological responses just discussed pertain to individuals. But these individual responses could be converted into higher turnout in several ways. Individuals who are unhappy about the implementation of Secure Communities might feel that voting is a means of changing policy or making their voices heard, and decide on their own to turn out to the polls. Or people may turn out more in the wake of Secure Communities implementation because they are already politically mobilized (Verba, Schlozman and Brady, 1995). If they have turned out to protest or been part of any activist response to the program, they may be easier to reach when activist groups seek to turn out Latino voters. Or, the individuals angered by the program could themselves become activists: they may feel more capable of turning out others, and be more willing to talk to their neighbors about issues and encourage them to vote. This may be especially effective if they reach out to co-ethnic eligible voters (Shaw, de la Garza and Lee, 2000; Ramirez, 2007; Barreto and Nuno, 2009). Section 5 attempts to distinguish between these possible mechanisms (individual choices or activist mobilization).

I expect to observe a net increase in voter turnout due to immigration enforcement, but there are also reasons to think that immigration enforcement could depress voter turnout, particularly from the "policy feedbacks" literature. Research on other punitive interactions with government finds diminished turnout due to feelings of alienation and lack of political efficacy (Soss, 1999; Bruch, Ferree and Soss, 2010). Deportations have been linked to family disruption for citizens or legal residents with undocumented relatives (Hagan, Rodriguez and Castro, 2011; Capps et al., 2007).⁶ Immigration en-

⁶Also, there is often a major economic *cost* associated with having a family member face removal: people

forcement has reshaped many of life’s social rhythms in targeted neighborhoods, and so it seems plausible that it could also disrupt political behavior.⁷ Although these responses may be occurring on a small scale, this paper finds a net increase in turnout after Secure Communities implementation.

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 could be biased because places that volunteered to take part in the program might differ in unobservable ways from other places.⁸

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

may spend money on bail or lawyers, but then they also face a major loss of income if a working family member is deported (Hagan, Rodriguez and Castro, 2011). This change in family resources (both of money and of time/attention) could also affect the likelihood of turning out (Verba, Schlozman and Brady, 1995).

⁷One other mechanism that could reduce turnout would be reduced voter fraud: that is, that noncitizens who had been illegally voting would be deported or fear deportation and thus that voter turnout would decrease. This seems highly unlikely: the cost of voting illegally is quite high if caught, and the payoff of casting a single vote is quite small and unlikely to affect any political outcomes. The number of people convicted of such crimes is vanishingly small, even after a major federal effort to find and prosecute voter fraud (Minnite, 2007). I do not anticipate that this mechanism will play a role in any effect detected.

⁸As 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.

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.⁹ Other jurisdictions in states without such MOAs, who also took no action to enroll in Secure Communities, were left un-enrolled. 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 3.¹⁰

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

⁹I 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.

¹⁰I 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 estimated turnout. This is a very small proportion of all units in the analysis, and represents places with extremely small Latino populations.

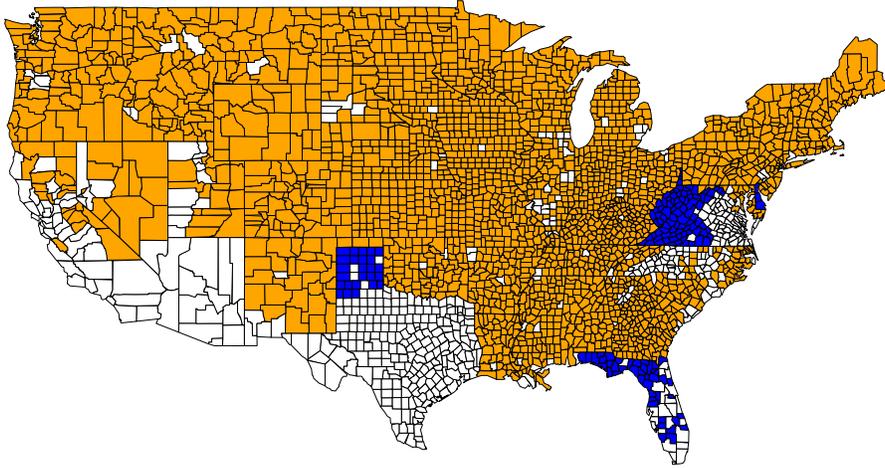


Figure 1: Jurisdictions considered treated (blue), untreated (yellow), and omitted (white) for the main analysis.

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 3 shows treated, untreated, and omitted jurisdictions.

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

Data Sources Information on the timing of Secure Communities implementation in over 3,000 jurisdictions is drawn from ICE records.¹¹ 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, 2013). 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

¹¹For 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.

of Latino eligible voters from the American Community Survey. Using CVAP 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. This is important because the effect could operate through previously-unregistered people being mobilized to register and vote.

4 Results

I first present observational results from the entire country, without dropping jurisdictions that may have selected into Secure Communities. Table 3 shows the results of an OLS 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 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.¹² Columns 3/4 and 5 of Table 4 present the fixed effects from hierarchical linear models with varying intercepts and slopes, and just varying intercepts, respectively.

Figure 1 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.¹³ 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

¹²One 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.

¹³This 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).

encouraging one to vote in the classic turnout experiment reported in Gerber and Green (2000).

4.1 Stability of results

These results are consistent under various model specifications and data subsets.¹⁴ The coefficient estimates from the model presented in column 4 of Table 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 is more plausible. 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.¹⁵ 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

¹⁴Regression tables available upon request.

¹⁵States 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.

**Estimated change in Latino voter turnout (2006–2010)
due to Secure Communities implementation**

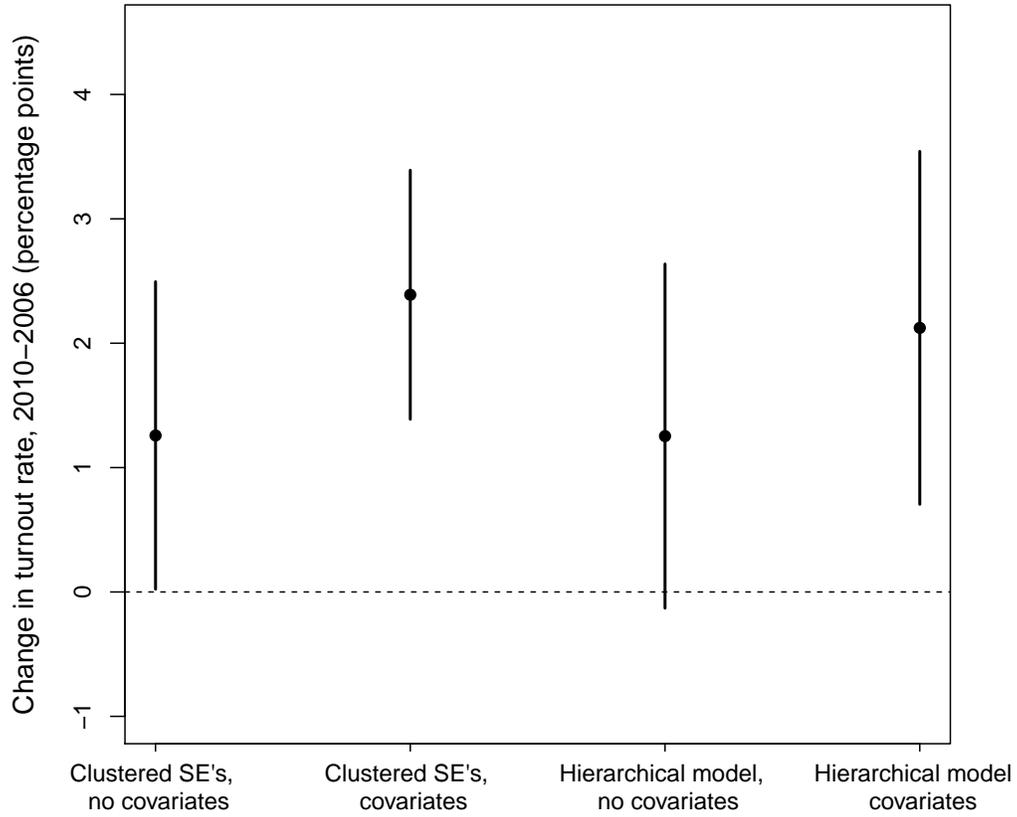


Figure 2: Estimates of SC treatment effect. “Covariates” indicates the inclusion of indicator variables for whether the cluster had senatorial or gubernatorial races on the ballot in 2006 or 2010. Lines represent 95% confidence intervals.

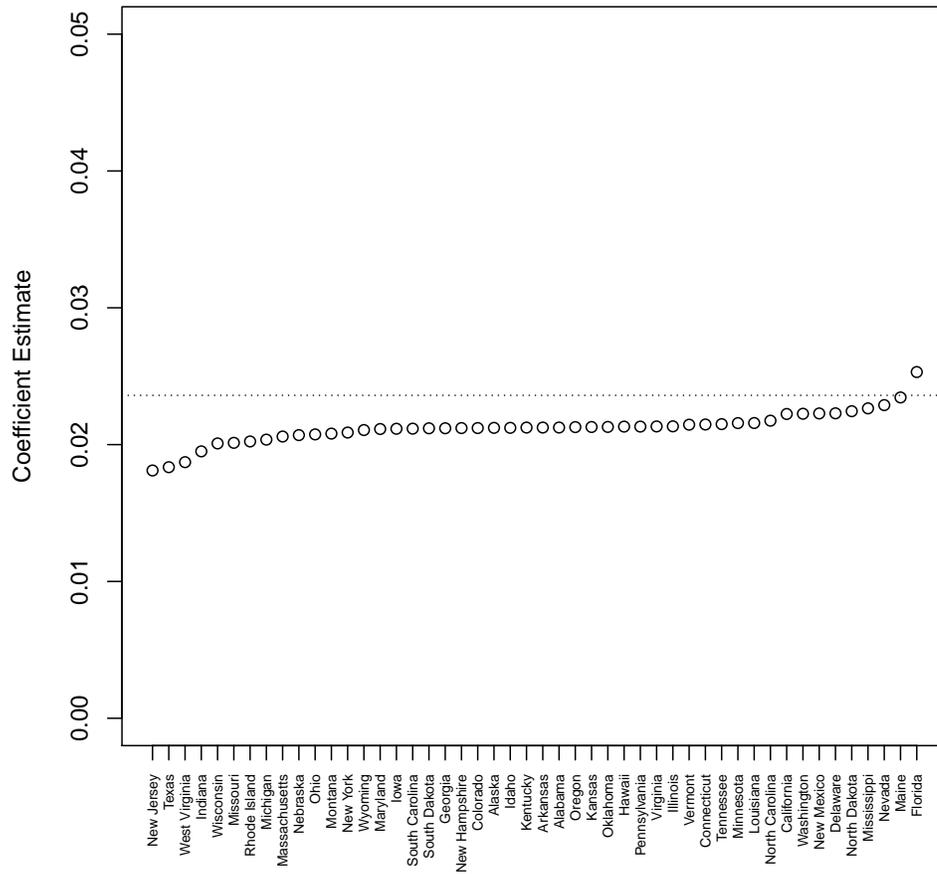
thousands of individual jurisdictions.¹⁶ Regressing the cluster-level change in Latino turnout from 2006 to 2010 onto the SC treatment variable and the set of election-timing covariates, as in the main analysis (weighted by the number of jurisdictions in each cluster, as in Green and Vavreck (2007)), 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 strange events. Figure 4.1 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.

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.

¹⁶Arizona 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.

Estimated Effects, One State Dropped



5 Possible Mechanisms

5.1 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. Different jurisdictions received different amounts of this type of “treatment” (actual detention of potential deportees). Relatively few people would have been deported due to the Secure Communities program at the time of the 2010 election, because most treated units in my sample had only been enrolled for several months, 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. I divided the treated portion of the sample into units 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. ¹⁷

¹⁷ICE does provide comparable data on removals, but I am reluctant to use this data in the same way, as it is very unlikely that removals are uniformly distributed across time. We don’t get to observe how many

Table 5 shows the results of this analysis. They are consistent with a story about mobilization rather than one about individual turnout choices. 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. But if it were the case that local activists heard about the implementation of the Secure Communities program from national or state organizations and then redoubled their efforts to get out the vote, we wouldn't necessarily expect such a pattern. This "mobilization" mechanism is explored in the next section.

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). But it is possible that the places that implement the Secure Communities program more zealously are also places where the dynamics of Latino turnout work differently for other reasons. These results do not definitively establish the mechanism by which immigration enforcement shapes turnout, but provide some suggestive evidence.

5.2 Survey Data on the "Mobilization" Mechanism

The finding that the treatment effect doesn't seem to depend on implementation (see 5.1) suggests that voters are not deciding to vote based on their personal experiences with the Secure Communities program. Instead, it appears that they might be mo-

people entered deportation proceedings by the time of the election, which would be the clearest measure. This number is likely to be quite small, given that many of the treated jurisdictions in the dataset entered the program only a few months before the election.

bilized to vote by activists who are aware of Secure Communities' implementation. I next test this theory using 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 6 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 2010.¹⁸ 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 7 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 effect (neither substantively nor statistically significant) for non-Latino CCES respondents in 2010. Both these results should give us confidence

¹⁸For 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.

that the results presented in table 6 are not simply a coincidence, but are due to specific mobilization of Latinos in the wake of Secure Communities implementation.¹⁹

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. This opens 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? Finally, when does this result (of increased turnout) hold, and when does it disappear or even reverse? As noted in Section 2, other punitive or paternalistic policies are associated with diminished turnout, even for those who experience them secondhand. Why should the realm of immigration enforcement differ?

The case of immigration enforcement is different from 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 incarceration, though this is debatable. Deportation also differs from incarceration

¹⁹See appendix for regression tables.

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.

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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: 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 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 3: Observational approach: Comparing all jurisdictions enrolled in SC to all unenrolled jurisdictions

	<i>Dependent variable:</i>
	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
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01

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

Note:

*p<0.1; **p<0.05; ***p<0.01

Table 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 5: Treatment effects by number of fingerprint submissions (Robust clustered SE's)

	<i>Dependent variable:</i>	
	Turnout change, 2006-2010	
	High submissions	Low submissions
	(1)	(2)
Treatment (Involuntary SC enrollment)	0.017*** (0.005)	0.031*** (0.005)
Senate election 2010	0.004 (0.007)	0.004 (0.007)
Senate election 2006	-0.031*** (0.005)	-0.031*** (0.005)
Governor election 2006	-0.003 (0.004)	-0.003 (0.005)
Governor election 2006	-0.010 (0.006)	-0.010* (0.006)
Constant	0.028*** (0.009)	0.029*** (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

Table 6: Reported campaign/activist contact, CCES 2010 (Latinos)

	(1)	(2)	(3)
SC Treatment	0.158** (0.071)	0.130* (0.074)	0.129* (0.072)
Registered Voter		0.247*** (0.019)	0.192*** (0.019)
Gender: Female		0.010*** (0.001)	0.009*** (0.001)
Age		-0.051** (0.024)	-0.043* (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*** (0.018)	-0.138*** (0.027)	-0.052 (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 7: Reported general election turnout, CCES 2010 (Latinos)

	(1)	(2)	(3)
SC Treatment	0.141** (0.070)	0.116 (0.073)	0.059 (0.066)
Registered Voter		0.093** (0.045)	0.063 (0.041)
Gender: Female		0.011*** (0.001)	0.010*** (0.001)
Age		-0.115*** (0.023)	-0.097*** (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*** (0.017)	0.069 (0.054)	0.130* (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

Appendix A: 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).²⁰ 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. 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 8 presents the results of a regression of the treatment variable onto the 2002-2006 change in Latino turnout in each state cluster.²¹ There is no evidence that

²⁰These estimates are available for about half of the jurisdictions in the main dataset.

²¹For 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

pre-2006 time trends, at least for the limited period for which there is data, predict treatment.

Table 8: 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
<i>Note:</i>	*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 9.

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 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.

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.

Some clusters contained very few respondents, so the turnout estimates were quite noisy. In Column (1) of Table 9, 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 6 plots the Latino turnout trends of states with and without treated units.

Table 9: Predicting treatment with prior turnout from CPS

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

Note: *p<0.1; **p<0.05; ***p<0.01

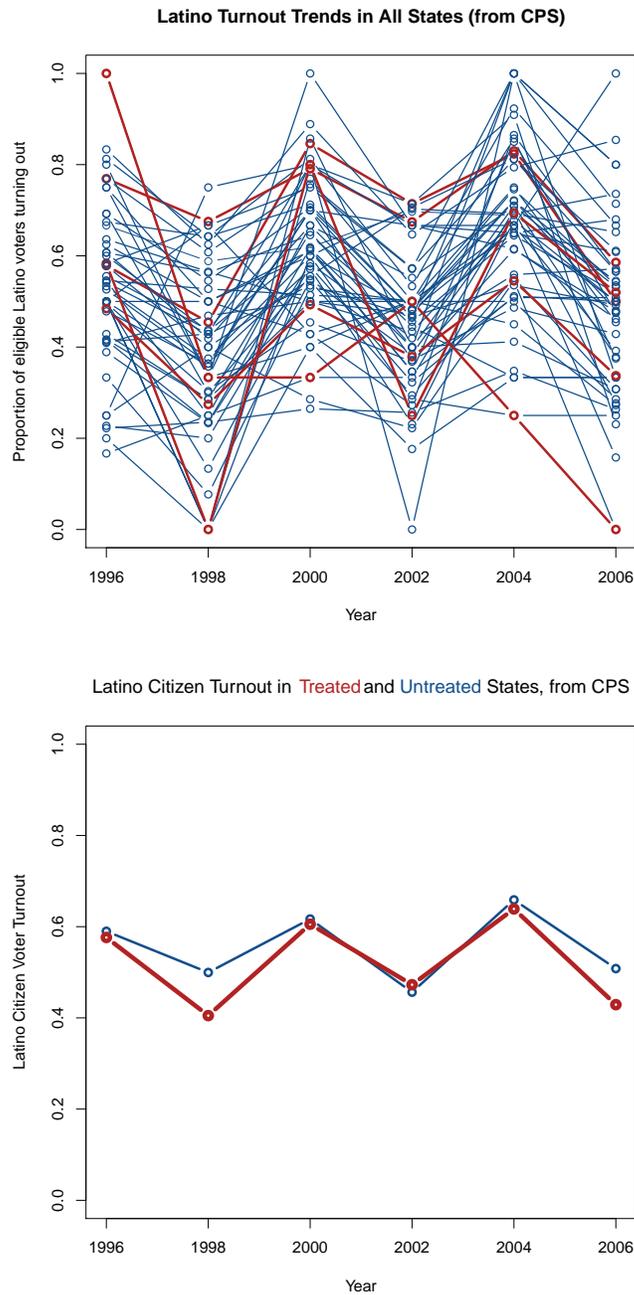


Figure 3: Latino turnout trends in the Current Population Survey. Treated states represented with thicker lines in both plots.

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 10 replicates the main analysis in the paper, the models from columns 4 and 5 of table 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.

Table 10: Placebo test: main analysis replicated on 2002-2006 treatment change

	Turnout change, 2006-2010	
	(1)	(2)
Treatment (Involuntary SC enrollment)	-0.038 (0.045)	-0.043 (0.038)
Senate election 2006	0.063** (0.026)	0.064** (0.027)
Senate election 2002	-0.007 (0.027)	-0.005 (0.027)
Governor election 2006	0.043* (0.024)	0.036 (0.024)
Constant	0.084** (0.038)	0.089** (0.039)
Observations	1548	1548

Note: *p<0.1; **p<0.05; ***p<0.01

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.²² 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 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 4, but is in the same direction and is of comparable magnitude. As shown in Table 11, 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.²³

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 11: Difference-in-difference estimates, compared to synthetic versions of each cluster

The resulting weights for each synthetic match are available on request, and will

²²I perform this matching using the "Synth" package for R (Abadie, Diamond and Hainmueller, 2011).

²³It 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.

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 4, and so I rely on the better-understood OLS standard errors, as do other papers using this approach as a check (Hall, 2013).

Appendix B: Additional CCES Analysis

Table 12: Respondent-reported campaign/activist contact, 2006 (Latinos)

	(1)	(2)	(3)
SC Treatment	-0.059 (0.108)	-0.054 (0.110)	-0.120 (0.114)
Registered Voter		0.422*** (0.072)	0.428*** (0.074)
Gender: Female		-0.008 (0.058)	0.009 (0.056)
Age		0.008*** (0.002)	0.008*** (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*** (0.027)	-0.029 (0.115)	-0.154 (0.134)
Observations	337	336	336
R ²	0.001	0.128	0.143
Adjusted R ²	-0.002	0.117	0.119
<i>Note:</i>	*p<0.1; **p<0.05; ***p<0.01		

Table 13: Respondent-reported campaign/activist contact, 2010 (non-Latinos)

	(1)	(2)	(3)
SC Treatment	0.023 (0.034)	0.008 (0.022)	0.006 (0.024)
Registered Voter		0.297*** (0.008)	0.245*** (0.008)
Gender: Female		0.009*** (0.0002)	0.008*** (0.0002)
Age		-0.046*** (0.007)	-0.035*** (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*** (0.009)	-0.031*** (0.010)	-0.051*** (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