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Understanding minority incorporation: evidence from state and local politics

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UNDERSTANDING MINORITY INCORPORATION:

EVIDENCE FROM STATE & LOCAL POLITICS

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DEDICATION

I would like to dedicate this work to my partner in life and ever-soothing voice of reason, Ike, and the two best things in my world, Simone & Sobolev.

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ABSTRACT

This dissertation seeks to identify why some local governments succeed at incorporating minority populations while others fail. I do this by looking at three distinct areas of political life: elections, policy implementation, and legislative responsiveness. In the first paper I investigate when and how party information affects minority electability. With nonpartisan ballots are used in more than three-quarters of local elections, studies tend to overlook the importance of party in election outcomes. However, after coding newspaper articles about mayoral elections across the U.S., I show that party information is often a central feature of partisan and nonpartisan contests alike. The importance of this finding should not be understated as the data reveals that an increase in voter access to party information substantially weakens the effect of an African American candidate's race on their electability.

The second paper uses the case of Secure Communities to argue that partisanship is not sufficient for explaining variation in local approaches to immigration policy. Using a novel dataset that combines county-level deportation rates, policing budgets, and data

on contracts between local prison facilities, private corporations, and federal agencies, I find that local compliance is explained by resource capacity first and political orientations second. Given the opposing positions of Republicans and Democrats on immigration enforcement programs, this result demonstrates that even when dealing with a particularly partisan issue there are other forces that can moderate the extent to which partisan influence matters.

The final paper tests whether legislators are responsive to minority-based interests using the case of E-Verify – an employment verification system that nearly half of all state legislatures have implemented. Assessing both state-level variation in E-Verify adoption and the roll call behavior of individual legislators, I show that legislative bodies and their members are responsive to sub-constituencies with the strongest preferences on E-Verify: agribusiness and the foreign-born community. However, responsiveness only occurs if that group is a constituency that the legislator would normally cater to. In other words, Republicans are willing to break with their party position and vote against E-Verify, but only if they represent districts with large agribusiness interests. Likewise, Democrats are responsive to their foreign-born constituents, but not farm owners. The implication of this is that the interests of minorities in Republican districts may suffer when they are not aligned with aggregate opinion or another sub-constituency that holds substantial influence over Republican lawmakers.

PREFACE

As the United States becomes increasingly multiracial and multiethnic, how are local communities – states, counties, and cities – responding to and dealing with this change? When I began my dissertation I had this broad question in mind, believing that local governments could play a key role in fostering the political incorporation of minorities by helping them to achieve access to socioeconomic and political opportunities and, in turn, develop feelings of social inclusion and belonging. However, I found that scholarship was deeply divided when it came to explaining why some localities succeed at creating inclusive environments while others establish a reputation as exclusionary and discriminatory. To identify why existing studies lacked consensus, I pursued a dissertation project that looked into three distinct areas of political life: elections, policy implementation, and legislative responsiveness. It was my hope that by broadening my inquiry into various areas of political life I could resolve some of the ongoing debates and push our knowledge of minority incorporation forward. While there is certainly room for further advancements, the three papers in this dissertation point out key omissions in prevailing studies and, subsequently, provide us with a strong foundation for how future research should move forward.

In the first paper, for example, I investigate what contexts are most beneficial for the electoral success of African American mayoral candidates. As far as the political incorporation literature is concerned, minority electability has received ample attention. In addition to consistently accounting for candidate attributes and the powerful affect that the racial composition of a jurisdiction has on minority success, scholarship on this topic has relied on institutional factors such as single-member vs. at-large districts and partisan vs. nonpartisan ballots to explain minority electability. Since the former has no bearing in the case of mayoral elections, I began exploring the literature surrounding the partisan-nonpartisan elections debate. Interestingly, I found that the evidence supporting each side of this disagreement was along methodological lines: experiments indicated that minority candidates are more electable when voters know their party affiliation while observational studies concluded that minorities fare just as well in nonpartisan elections. Upon closer examination, the reason for this tension became clear: observational studies assumed that because an election was nonpartisan, party played no role in the contest. I show that in fact party information is often abundant in both partisan and nonpartisan contexts and that as its availability to voters increases, the effect of a candidate's race on vote choice is substantially weakened. The implication of this is that minority candidates have tools that can be deployed to overcome racial prejudice and that local elections - even officially "nonpartisan" elections - are not as candidate-centered as many scholars have assumed.

Whereas the first paper shows that omitting party information from models of minority electability fundamentally changes our understanding of how a candidate's race influences their electoral success, the second paper uses the case of Secure Communities to argue that studies of policy implementation rely too much on party-based explanations to explain local compliance with federal immigration enforcement efforts and neglect the practical considerations that contribute to local decision-making. Since the early 2000s, the pursuit of immigration-related policies at the local level has become more commonplace transforming the relationship between minorities and their local governments. Unsurprisingly, immediate inquiries into the adoption of immigration enforcement efforts found that the partisan-leanings of local governments and their citizens were highly determinant of whether or not a community *elected* to pursue a tougher stance on immigration through a program like 287(g) or a more inclusive agenda by passing legislation like the Dream Act. However, scholarship on federal mandates such as Secure Communities and the Priority Enforcement Program was rash to assume that partisanship would also dominate whether counties *complied* with federal mandates. In this paper, I show that when a mandate incurs costs on local governments and their agencies, levels of compliance are explained by resource capacity first and political orientations second. In other words, without the necessary financial and physical resources to implement a policy, local actors are pressed to limit or modify their compliance regardless of normative or political preferences. Given the staunch difference that previous scholarship has found in the types of immigration policies Republican and Democratic governments support, this result demonstrates that even when dealing with a particularly partisan issue we cannot overlook how logistical considerations moderate the extent to which partisan influences matter.

The final paper uses a minority-related policy area – E-Verify – to analyze how legislators respond to constituency preferences. Previously, studies assumed that as an issue's salience diminishes, constituency pressures have a weaker effect on legislative responsiveness and partisan loyalties become central to understanding legislative behavior. I use the case of E-Verify to demonstrate that if we shift our focus from the aggregate to those sub-constituencies for which a policy is most relevant, we find that individual legislators and legislative bodies as a whole are responsive to their constituents. The potential for sub-constituencies to affect legislative behavior has been considered at the national-level, but state-level analyses have virtually ignored this relationship. Instead, their focus has been limited to inquiring whether state policy reflects public opinion in the aggregate. Since E-Verify yields weak preferences collectively, but is especially salient to the agribusiness and foreign-born communities, using it as a test of legislative responsiveness to particular sub-constituencies works well. However, E-Verify is also a strong test case for illuminating our understanding of legislative responsiveness to minority-related issues more specifically. I find that Democrats are highly responsive to the preferences of their foreign-born constituents, but that only the presence of large farm owning populations sways Republicans to turn against E-Verify. This implies that minorities may need to form alliances with groups that traditionally have influence over Republican behavior in order to find responsive representation across the parties.

In addition to deepening our understanding of minority political incorporation, it is my hope that these cases also develop our broader theoretical knowledge about how local governments operate. In the case of the third paper on legislative responsiveness, the more general theoretical implications are clear: constituencies are capable of influencing legislative behavior and supplanting party loyalties even when dealing with policy issues of lower salience. Since the federalist system arguably places state governments in a better position than national government to tailor their policies to the preferences of their various constituencies, exploring the relationship between legislative responsiveness and sub-constituencies is a natural and overdue line of inquiry. Likewise, that partisan information is abundant in nonpartisan as well as partisan elections has implications for candidates of all races and genders. Accordingly, measures of party information should be adopted into any model trying to explain local election outcomes or voting behavior. In the case of the second paper, it may not be novel to assume that discrepancies in the implementation of federal mandates are linked to a local government's resource capacity, but a prudent (and thus far untaken) path for future research would be to consider if capacity also moderates the association between ideological preferences and compliance with various areas of federal policy.

TABLE OF CONTENTS

LIST OF TABLES

LIST OF FIGURES

CHAPTER ONE

Electing Black Mayors:

Does Party Information Make a Difference?

As a diverse society, who we elect is a vital indicator of our progress towards inclusion. Political leaders not only represent the "face" of our society, but also reflect how power and influence are distributed. Thus, it is not surprising that scholars have paid a great deal of attention to how voters respond to minority candidates in an attempt to identify the steps we can take to increase descriptive representation. In the context of U.S. local elections, most of this attention is focused on whether particular electoral systems create environments that are more or less advantageous to minority electability. Scholars initially exhibited a great deal of skepticism about the potential of minority candidates to win nonpartisan elections precisely because the absence of party would encourage voters to give greater consideration to physical attributes like race (Adrian 1952; Jennings and Zeigler 1966; Karnig 1976; Lieske and Hillard 1984). Since then, party and race have been the focus of a vast number of studies; yet, we still have surprisingly little knowledge or consensus about how these two attributes interrelate to affect vote choice.

On the one hand, experimental evidence appears to confirm our early suspicions that minority candidates are more electable when voters know their party affiliation (Burnett and Kogan 2014; Kam 2007). On the other hand, observational studies find that minorities fare just as well in nonpartisan elections (Abrajano, Alvarez, and Nagler 2005; Stein et al. 2005) and that their electoral prospects may actually improve when party is left off the ballot (Marschall and Ruhil 2006; Meier et al. 2005; Sonenshein 1986). Thus, existing literature leaves us with a baffling puzzle: why do methodological choices (experimental versus observational data) produce incongruous explanations of the relationship between race, party, and vote choice? I propose that the problem resides in how observational studies measure partisanship: voters are thought to be informed of the candidates' parties in partisan contests, but are assumed to be unaware of party affiliation in nonpartisan elections. Yet, this simple dichotomy rarely exists. Even in officially nonpartisan contests voters are regularly knowledgeable of candidate party affiliation. To correctly identify the relationship between a candidate's race and an individual's vote choice, models of local voting behavior should reflect this reality.

I show how this is possible by considering the *amount* of party information available to voters in partisan and nonpartisan mayoral elections across the United States. Equipped with this continuous measure of party information, I argue that party moderates the influence of race on vote choice: as the level of party information available to voters increases, the effect of race on vote choice is weakened. I find that it is not the type of electoral system – partisan or nonpartisan – that affects minority electability, but rather the availability of party information. This means that the real and experimental worlds produce findings that are more alike than otherwise thought: party cues influence how voters react to minority candidates. The implication of these findings is that minority candidates have tools that can be deployed to overcome racial prejudice and that local elections - even officially "nonpartisan" elections - are not as candidate-centered as many analysts have assumed.

Race, Party, & Voting Behavior

A general consensus across the political science literature is that individuals rely on shortcuts, such as party (Rahn 1993; Schaffner, Streb and Wright 2001), incumbency (Krebs 1998), and candidate attributes (Matsubayashi and Ueda 2011; Squire and Smith 1988) to offset the high cost of obtaining, processing, and evaluating information about the candidate's policy positions, political experience, and future objectives (Downs 1957; Popkin 1994; Lupia and McCubbins 1998). While a number of studies have reinforced the strong effect that cues about partisanship and incumbency have on voter decision-making, there is less clarity when it comes to explaining the influence of a candidate's attributes on voters (Krebs 1998; Wolman, Page, and Reavley 1990).

Nonpartisan elections – which account for nearly three-quarters of all local elections in America – are one arena where scholars predict that candidate attributes will be especially important to voter decision-making. Indeed, political scientists projected early on that racial cues would be especially salient to voter decision-making in nonpartisan elections and, subsequently, warned of the damage that this new form of ballot could have on minority electability (Adrian 1952; Freeman 1958; Jennings and Zeigler 1966; Karnig 1976; Lieske and Hillard 1984; Mueller 1970; Pomper 1966). Experimental studies of minority candidates and vote choice substantiate this assertion. In the absence of party labels, white respondents are less likely to vote for or positively evaluate minority candidates across various levels of office (president, governor, and city councilman) regardless of their personal characteristics, issue positions, or job experience (McConnaughy et al. 2010; McDermott 1998; Terkildsen 1993).

Other experiments, however, find that this effect diminishes when respondents are informed of candidate partisanship. For example, respondents in Kam's (2007) study were given information on three judicial candidates – two white and one Latino – to test whether ethnicity affected voter preferences. Half of her respondents were provided with information about the party of the governor that endorsed each candidate. When given that cue, the support for the Latino candidate increased by 10 percentage points. Similarly, Burnett and Kogan (2014) relate partisan cues to the salience of a candidate's ethnicity by showing participants a series of quotations from either white or Latino candidates. Some respondents were given information about the candidates' parties and others were not. They find that quote misattribution decreased by 44 percentage points when information about candidate party affiliation was also provided. The authors acknowledge that their results fail to speak explicitly to whether or not ethnic categorization disadvantages minority candidates, but they nevertheless caution that nonpartisan elections are liable to impair minority electability.

Based on experimental studies alone we might conclude that any negative effects a minority candidate's race has on their ability to capture votes is offset by party cues. This explanation, however, is belied by observational studies that show nonpartisan elections either pose no disadvantage for minority candidates (Abrajano, Alvarez, and Nagler 2001; Stein et al. 2005) or, in some cases, actually increase the odds of a minority being elected (Marschall and Ruhil 2006; Sonenshein 1986). Yet, whereas the experiments control for respondent knowledge of partisanship, the observational models neglect the possibility that party influences vote choice in nonpartisan elections. At the same time, their discussion of particular elections suggests that an unmeasured level of party information may actually be instrumental to explaining election outcomes. For example, Abrajano, Alvarez, and Nagler (2001) comment in their study of the 2001 Los Angeles city election that voters were well informed about the partisanship and ideological leanings of the contenders *despite* the elections being officially nonpartisan. Similarly, Stein and colleagues (2005) note that of surveyed voters, 86% were able to correctly identify the party affiliations of both candidates in Houston's 2001 "nonpartisan" mayoral election.

I argue that, by failing to take into account this unmeasured level of party information, prevailing studies on minority candidates and vote choice have missed a pivotal moderating variable thereby obfuscating the true effect of racial cues on minority electability. In the next section, I develop a theoretical framework that explains how knowledge of candidate partisanship is likely to moderate inclinations toward race-based voting when minorities are on the ballot.

Minority Electability & Cue Dominance

The theory of minority electability presented here builds on the idea that voters utilize shortcuts in order to simplify decision-making during the voting process, but contends that particular heuristics have a stronger impact on vote choice than others. In particular, I argue that when a minority candidate is also a co-partisan, voters are more likely to vote according to party rather than racial considerations.

For most voters, a cue like race indirectly impacts vote choice by triggering a particular image of what that candidate will be like in terms of their ideology, competence, and character. White voters in particular depict African-Americans as lacking the necessary qualities of a strong political leader (Best and Williams 1990; Broockman et al. 2014), describing them as less hardworking (Gilens 1999; Sniderman and Piazza 1993), less competent (Sigelman et al. 1995), and less intelligent (Bobo et al. 2012). Black candidates are also more likely to be associated with the Democratic Party, depicted as ideologically liberal, and thought to be especially sensitive to minority-related issues when compared to their white counterparts (Berinsky et al. 2011; Jacobsmeier 2014; Sigelman et al. 1995). Absent other information, the stereotypes triggered by a black candidate's race can certainly cost them votes (McDermott 1998). In fact, many African-American candidates pursue campaign agendas that consciously avoid emphasizing their race or racializing issues in order to lessen the use of racial stereotypes (Kaufmann 2004). However, media coverage of minority politicians, which tends to disproportionately focus on race and ethnicity, makes it difficult to prevent unfavorable – and typically automatic – stereotyping (Niven 2002; Zilber and Niven 2000).

Despite the automaticity in which this process of categorization occurs, the effect of stereotyping on impression building is constrained when individuals receive additional information – what Kunda and Thagard call "individuating information" – that is pertinent to the judgment at hand (Kunda and Thagard 1996). This information does not necessarily eliminate the activation of stereotypes, but it can *neutralize* the importance of the stereotype by redirecting the observer's attention. Individuating information is thought to make stereotypes impotent because individuals view stereotype-based judgments as less valid than those that are rationalized from individuating information (Crawford et al. 2011; Hilton and Fein 1989; Locksley et al. 1980; Nisbett, Zukier, and Lemley 1981). In the context of a biracial election, I propose that party cues are precisely the type of individuating information that can counteract the negative effects that racial stereotypes have on vote choice.

Whereas race incites particular impressions about a candidate that may or may not be substantiated with further information, party provides voters with the ability to make competent inferences about candidate issue positions and ideology (Conover and Feldman 1989; Rahn 1993). Furthermore and perhaps more importantly, party information activates partisan-rooted loyalties that typically make evaluating and weighing other pieces of information unnecessary for voters (Cohen 2003; Popkin 1991; Rahn 1993; Zaller 1992). As Beck (1997) explains, party labels "organize and simplify" electoral contests that are otherwise laden with "strident rhetoric" that most voters find confusing.

But how does party supplant racial cues in *nonpartisan* contests? Previous studies of judicial elections indicate that party identification is a key indicator of vote choice across partisan and nonpartisan ballot formats (Bonneau and Cann 2013; Rock and Baum 2010). If this is true, then it seems likely that partisanship also has a role to play in mayoral elections regardless of whether or not party labels are on the ballot. To confirm this, I conducted a content analysis of local newspaper coverage of biracial (white-black) mayoral elections in large U.S. cities from 1991 to 2015.[1] Using the News Bank Database, I collected every article about the relevant mayoral contest for the two months preceding each election. I then recorded the total number of election-related articles and coded the number of articles that mentioned the party and race of the top vote-getting black and white candidates. Although using news coverage of the elections is an imperfect substitute for the information that was available to voters, previous media studies show that newspaper articles are typically an accurate reflection of the issues discussed in local campaigns (Barrett and Barrington 2005; Erbring, Goldenberg and Miller 1980) and that newspaper readers receive approximately the same campaign information as television viewers (Mutz 1995). Moreover, newspaper articles are the only archived source of campaign coverage that is consistently available across cities and time.

In total, I found newspaper coverage for 128 of the 159 biracial elections during this period, resulting in 7,574 articles coded. Table 1.1 displays key summary statistics for the number of election-related articles and the regularity with which the party of the top vote-getting black candidate was mentioned. Clearly, it is not uncommon for party cues to be available in nonpartisan contests. In some cases nearly 82 percent of articles about a nonpartisan election referenced the black candidate’s party affiliation. This indicates that a simple dichotomous indicator of partisan elections would inadequately reflect the potential role of party in nonpartisan elections.

[1] Large cities are defined as those that had a population of 100,000 or more between 1991 and 2015. More discussion about the cities sampled can be found in the section on study design and a complete listing in the paper’s appendix.

Table 1.1. Content and Frequency of Coverage of the "Top" Black Candidate

	Mean	St. Dev.	Min	Max
Articles per election	59.17	37.18	9	209
# articles mentioning candidate party in all elections	22.16	28.61	0	164
# "nonpartisan" election articles mentioning candidate party	13.07	13.29	0	69
% articles mentioning candidate party in all elections	33.22	25.46	0	96.64
% "nonpartisan" election articles mentioning candidate party	23.61	19.56	0	81.81
% partisan election articles mentioning candidate party	46.79	26.79	2.32	96.64

Figure 1.1 provides a visual comparison of the frequency of party cues in partisan and nonpartisan contests. More often than not there are some references to candidate party, but there is also considerable variation in terms of the regularity with which voters might be exposed to party information. So, how do we determine *when* voters are informed of candidate party? Unlike partisan elections, where we know with certainty that even the most disengaged voter will see the candidates' parties on the ballot, it is difficult to identify the number of party mentions that are necessary before we can state with confidence that voters are knowledgeable of candidate partisanship in nonpartisan elections. Indeed, it is rare that every voter – or even most – read every article about an upcoming mayoral election. Thus, it would be hasty to assume that because references to the candidates' parties exist in news coverage, voters were also knowledgeable of that information.

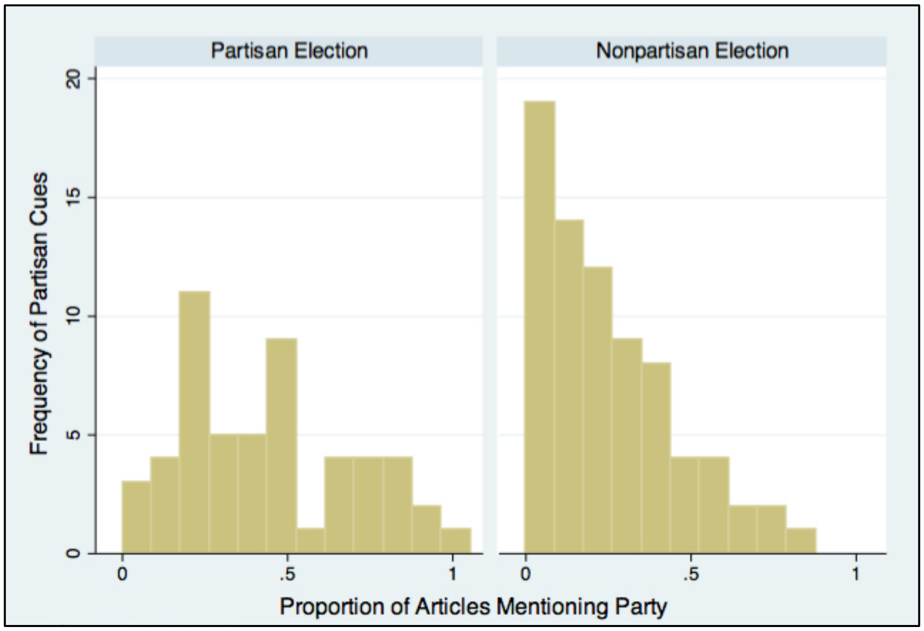

Figure 1.1. Partisan Cues in Partisan and Nonpartisan Elections.

Like the partisan/nonpartisan distinction, simplifying elections into those that mention party and those that do not is an imprecise approach to measuring the influence of party cues. A better alternative is to consider the amount of party information on a continuous scale. In this case, we would expect that as the percentage of articles mentioning candidate partisanship increases, the *likelihood* that a particular voter would be informed about the party affiliations of the candidates also increases.[2] Likewise, we would expect that fewer voters received partisan cues if party affiliation was mentioned rarely in newspaper coverage. In this way the continuous scale allows us to test whether

[2] It is possible that using the percentage of articles inflates just how "available" party information was for voters. For example, if there are only 9 articles about a particular election, but 5 of them mention the black candidate's party, then the model would assume that the possibility of a voter being aware of party is relatively high. However, there are very few actual opportunities for voters to obtain this information. To account for this, I test the models using an alternate measure of party information – the *number* of articles that mention party. As discussed later on, the results, which are located in Table 6, are consistent with measuring information as a percentage.

the proportion of votes received by the black candidate changes when their partisanship is more or less likely to be known by voters (all else being equal). Specifically, I posit the following hypothesis:

> Hypothesis: *Even in technically "nonpartisan" contests, as the level of party information in the media increases, race will become less salient leading to an increase in a) the black candidate's vote share and b) the probability of a black candidate winning the election.*

In addition to Kam's (2013) experiment mentioned previously, this hypothesis is in line with prior research showing that partisan cues are capable of reducing the effect of another candidate attribute, gender (Burnett and Tiede 2015; Matland and King 2002; but see: Sanbonmatsu and Dolan 2015). Yet, thus far the evidence suggesting that party supplants cues like race and gender is limited to experiments in which respondent information about the candidates is carefully controlled and, consequently, lacks external validity. I improve upon this work to show that party moderates the relationship between race and vote choice in *actual* elections.

In sum, this theory of minority electability suggests that negative racial stereotypes are more likely to hurt black electability when party information is absent or more difficult for voters to obtain. Showing that party information varies in its availability and its influence on voters in local elections is especially challenging. In the next section, I explain how I addressed this challenge by combining the content analysis discussed above with election data from mayoral elections across the United States.

Study Design

The observational study uses original data from 128 biracial mayoral elections in 39 American cities. Table 1.2 provides an overview of the number of elections observed for each city and the type of election they employ. The appendix features a more detailed list of the year of each election, the election stage, and the number of candidates competing by city. Although these cities are unrepresentative of *all* urban contexts, they offer a strong and representative sample of larger, diverse cities – in other words, those cities where we would expect to see a minority on the ballot. The elections span from 1991-2015 and are limited to those contests where at least one black and one white candidate were running. In cases where multiple black and/or white candidates were on the ballot, the analysis focuses on the vote share of the top vote-getting black and white candidates.[3] General elections make up the bulk of the contests in the dataset, but I also include primaries and runoff elections.[4] Since including primaries and runoffs means that there

[3] There are 19 elections with 2 black candidates and 3 elections with 3 black candidates. To be sure that elections with multiple African-Americans contending are not biasing the results, I employ additional analyses that exclude these elections from the dataset. The results, available in the appendix, reflect that regardless of the number of black candidates running in an election, party cues continue to be a key component to understanding black electability.

[4] This includes both partisan and nonpartisan primaries. Because it is possible that the inclusion of partisan primaries will bias the results (since partisanship is constant and known across candidates, making race an arguably more salient factor), I rerun the main

can be more than one contest for a city-year observation, the regression models include a dummy variable that accounts for contests that occurred within the same election sequence. For example, because Albany, New York's 2005 general election results and newspaper coverage may very well be influenced by their 2005 primary, there is a dummy variable denoting that the general election is linked to its primary.[5]

Table 1.2. Election Type and Number of Elections Observed by City

City	State	Election Type	Number of Elections	City	State	Election Type	Number of Elections
Albany	NY	P	5	**Memphis**	TN	NP	2
Athens	GA	NP	3	**Milwaukee**	WI	NP	3
Atlanta	GA	NP	3	**Mobile**	AL	NP	3
Augusta	GA	NP	7	**Montgomery**	AL	NP	2
Aurora	IL	NP	3	**New Orleans**	LA	P	8
Baltimore	MD	P	3	**New York**	NY	P	3
Boston	MA	NP	1	**Oakland**	CA	NP	1
Buffalo	NY	P	5	**Orlando**	FL	NP	2
Charlotte	NC	P	2	**Philadelphia**	PA	P	8
Chicago	IL	NP	3	**Pittsburg**	PA	P	2
Cincinnati	OH	NP	3	**Sacramento**	CA	NP	2
Cleveland	OH	NP	2	**San Francisco**	CA	NP	2
Columbus	OH	P	5	**Seattle**	WA	NP	1
Denver	CO	NP	6	**St. Louis**	MO	P	7
Detroit	MI	NP	2	**St. Petersburg**	FL	NP	1
Houston	TX	NP	10	**Syracuse**	NY	P	2
Indianapolis	IN	P	1	**Tampa**	FL	NP	2
Jacksonville	FL	NP	5	**Washington**	DC	P	2
Los Angeles	CA	NP	2	**Wichita**	KS	NP	2
Macon	GA	NP	2				

regression models excluding them. These results, available in the appendix, show no meaningful differences from the models in the main text.

[5] In the appendix, I rerun the regression models without these 27 "linked" observations. The results are consistent with the models in the main text.

Information on the number of candidates running, incumbency, gender, race/ethnicity, and partisanship was acquired either through official election results found on the city clerk or county website or through the candidates' personal websites. When such information was not available from these sources, I made use of Project Vote Smart and Our Campaign's catalog of political candidates as well as news stories about the election that featured candidate biographical information.

To test my theory of minority electability, that an increase in party information improves black electability, I use the information about party cues gleaned from the analysis of news articles to generate the central explanatory variable: the percentage of articles that reference the black candidate's party. The effect of party information is tested on two primary outcomes: the black candidate's overall vote share and an electoral victory by the black candidate. The first outcome specifically tests the principal hypothesis' claim about the relationship between party information and black vote share. However, because the data includes a mixture of election types – primary, general, and runoff elections – and variation in terms of the number of candidates running (with the majority of contests featuring two, but others as many as 6), the second dependent variable – a black election victory – is necessary to capture situations where black candidates win the election despite obtaining a relatively small percentage of the vote.

The statistical model includes several independent variables that are typically related to vote choice and that are featured prominently in existing models of local voting behavior: whether the contest was partisan or nonpartisan, incumbency, if the election had 3 or more candidates,[6] the amount of racial information made available to voters via newspaper coverage, and the type of election (primary, general, or runoff). For the majority of cities there is only one election to analyze while for others I have up to seven. Consequently, the statistical models include as many potential confounding city-level demographics as possible: the population size, the percentage of black and white residents, the percent of college educated residents, the median household income, and the percentage unemployed. Data for each variable was matched to the year of the election or for the closest year that data was available. For example, the Houston mayoral election for 2009 uses U.S. Census population estimates from 2010 while the 1993 election is matched with population estimates from the 1990 Census. Finally, from Einstein and Kogan (2015), I include the percentage of residents that voted for the 2008 presidential candidate from the same party as the black mayoral candidate to account for how aligned the city's general political preferences are with the black candidate.

The study design used here makes a dramatic improvement over existing observational studies. By combining observational data with a content analysis the study gains considerable generalizability without losing the contextual detail that the existing studies, which focus on one or a few elections, excel at. Additionally, by limiting the dataset to elections that feature a black candidate on the ballot, I avoid conflating factors that influence black electability with those that increase the odds of a black candidate running. This makes a considerable improvement over Marschall and Ruhil's (2006) study of black mayoralties, which covers an impressive 309 cities but includes elections

[6] Candidates had to obtain at least 5% of the vote in order to be included in the dataset as a viable candidate.

with no black candidates on the ballot. Recent research (Juenke 2014; Juenke and Shah 2015; Shah 2010; Shah 2014) illustrates that failing to account for *when* minorities are on the ballot is particularly problematic for correctly specifying models that will predict the likelihood of a minority candidate winning.

Results

Before testing for information effects, Model 3.1 of Table 1.3 investigates the bivariate relationship between a simple dichotomous indicator of partisan/nonpartisan elections and black vote share. This allows us to compare the current study to previous work on minority electability in partisan/nonpartisan contests. Although the coefficient for nonpartisan elections in Model 3.1 is negative, suggesting that minority candidates perform best when party labels are on the ballot, this relationship is not statistically significant in the conventional sense. In other words, consistent with prevailing studies, Model 3.1 implies that there is no meaningful difference in how black candidates fare when party labels are on or off the ballots. But is this simplified depiction of party influence an adequate reflection of the role party plays in voter decision-making?

Table 1.3. The Effect of Partisan Cues on Black Vote Share

	3.1	3.2	3.3	3.4
% Party mentions		35.21^{***}(6.458)	27.02^{**}(9.23)	25.08^{***}(6.243)
% Race mentions				-2.382(7.179)
Nonpartisan elections	-4.475(3.441)	1.659(3.452)	-4.481(5.535)	5.003(3.684)

% Party mentions x nonpartisan elections			17.85(13.00)	
Incumbent				26.58***(3.942)
3(+) Candidates				-8.948**(2.867)
Election Type				
General				2.701(3.793)
Runoff				8.766(8.476)
Population (logged)				0.266(1.811)
% Black				0.108(0.114)
% White				0.0174(0.154)
Share of pres. vote for candidate's party				0.105(0.0789)
Bachelor's degree				0.0560(0.156)
Median household income (logged)				-0.477(7.349)
% Unemployed				-0.219(0.403)
Constant	39.34***(2.73)	24.01***(3.857)	27.76***(5.08)	14.29(85.52)
N	128	128	128	126
R^2	0.044	0.219	0.230	0.604
AIC	8.74	8.54	8.54	8.06

Note: All Models use OLS regression with a dummy variable for contests that occurred in the same election sequence and robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Turning to information effects, Model 3.2 adds to our bivariate model the key explanatory variable: the percentage of election-related articles that mention the black candidate's party affiliation. The results of this model provide us with two important findings. First, the official context of the election – partisan or nonpartisan – is inconsequential to black vote share. Second, the availability of party cues considerably improves black electoral prospects. On average, the black candidate receives 27% of the vote when no party information is available to voters. This vote share increases to more than 44% when half of election-related articles mention party affiliation.

Models 3.3 and 3.4 provide us with even more confidence that party information has important implications for black electability that are not captured by a simple partisan/nonpartisan distinction. Whereas Model 3.1 leads us to believe that black candidates are not systematically disadvantaged by nonpartisan contests, Model 3.3 reveals the "hidden" information that gives this statement credibility. By adding an interaction between the level of party information available to voters and the partisan/nonpartisan status of the election to our previous model, we discover that yes, black candidates can perform well in nonpartisan elections, but only because so many of those contests provide voters with partisan cues. Although this relationship just misses conventional thresholds of statistical significance (with a P-value of 0.118 and 95% confidence interval spanning from -5.12 to 44.79), Figure 1.2 illustrates that the effect of party information on vote choice – though positive in both partisan and nonpartisan contests – is especially potent in officially "nonpartisan" elections.

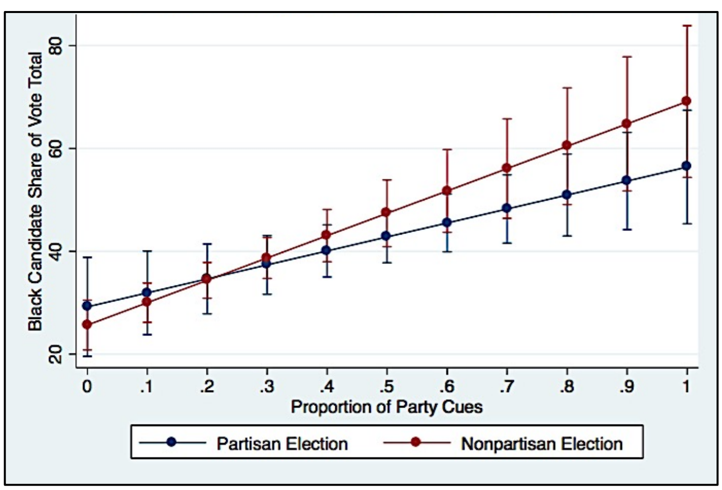

Figure 1.2. Effect of Party Information of Black Candidate Vote Share in Partisan and Nonpartisan Elections (with 95% confidence intervals).

Importantly, Model 3.4 reveals that the relationship between party information and black vote share endures even when controlling for factors typically shown to affect vote choice in biracial elections: namely, incumbency (Hajnal 2001; Stein et al. 2005) and a larger field of candidates (Bullock 1984; Hajnal and Trounstine 2014). Figure 1.3 shows us the marginal effect that additional party cues have on voter support for the black candidate while holding all other variables from Model 3.4 at their means. A black candidate's vote share increases by more than 10 percentage points moving from an election with no party information to an election where half of all articles contain party cues. Another 11 percentage points are gained when all of the articles discuss party, implying that voters are more compelled to support African-American candidates if they are informed about their party affiliation.

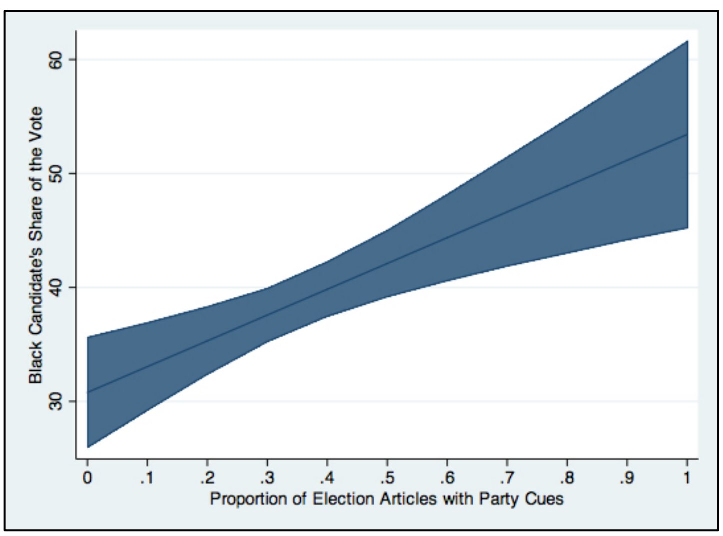

Figure 1.3. Marginal Effect of Partisan Cues on Black Candidate's Vote Share (with 95% confidence interval).

Table 1.4 confirms that in addition to partisan cues boosting a black candidate's vote share, this information also produces more black mayoral victories. Relying only on the conventional binary distinction of partisan/nonpartisan elections, as shown in Model 4.1, implies that black candidates have a 40 percent chance of winning *any* nonpartisan election – only slightly less than their 43 percent chance of winning a partisan contest. By adding a continuous measure of party information to Model 4.2, we find that black electability is actually highly dependent on voter access to party information. The probability of a black candidate winning, illustrated in Figure 1.4, more than doubles from 19 percent to 54 percent when moving from an election with no party information to an election where 50 percent of articles mention the black candidate's party.

Table 1.4. The Effect of Partisan Cues on a Black Election Victory

	4.1	4.2	4.3
% Party mentions		3.480^{***}(0.916)	3.722^{**}(1.316)
% Race mentions			0.444(1.568)
Nonpartisan elections	-0.145(0.366)	0.457(0.433)	1.410(0.756)
Incumbent			4.207^{***}(1.242)
3(+) Candidates			-0.001(0.626)
Election Type			
General			0.399(0.680)
Runoff			0.511(1.308)
Population (logged)			0.199(0.349)
% Black			0.0102(0.0234)
% White			0.0180(0.0325)

Share of pres. vote for candidate's party			0.0315(0.0221)
Bachelor's degree			0.0251(0.0343)
Median household income (logged)			0.259(1.421)
% Unemployed			-0.0998(0.0883)
Constant	-0.322(0.294)	-1.875**(0.543)	-12.16(15.79)
N	128	128	126
R^2	0.003	0.103	0.320
AIC	1.39	1.29	1.16

Note: Models are logistic regressions with a dummy variable for contests that occurred in the same election sequence and robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

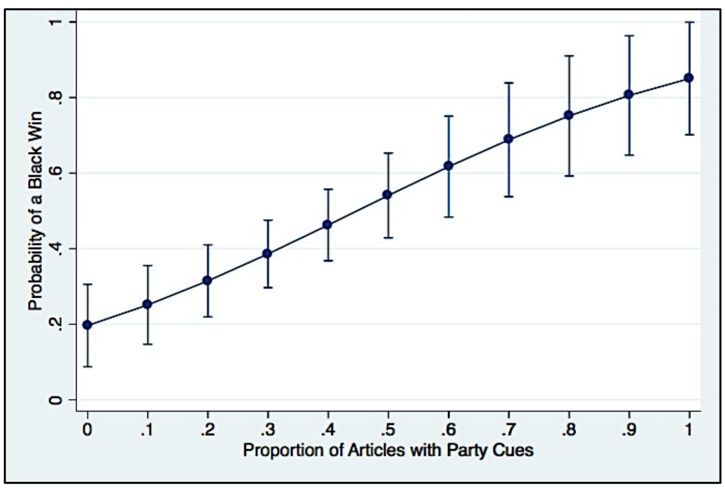

Figure 1.4. Effect of Partisan Cues on the Probability of a Black Candidate Winning (with 95% confidence interval).

Again, this relationship holds even when controlling for the variables added to Model 4.3. Holding all other variables from Model 4.3 at their means, Figure 1.5 compares how partisan cues improve the probability of a black candidate winning in partisan and nonpartisan elections. The probability of a black candidate winning a mayoral election increases from 0.2 in a "nonpartisan" election with no cues to 0.61 in a "nonpartisan" election where half of the articles contain information about candidate party. Unsurprisingly, the confidence intervals for partisan and nonpartisan elections overlap considerably at the highest levels of party information – those elections where we would expect the majority of voters to be aware of candidate party affiliation regardless of whether or not party is on the ballot. However, Figure 1.5 illustrates that, in general, party information's sharpest effect is on vote choice in nonpartisan contests.

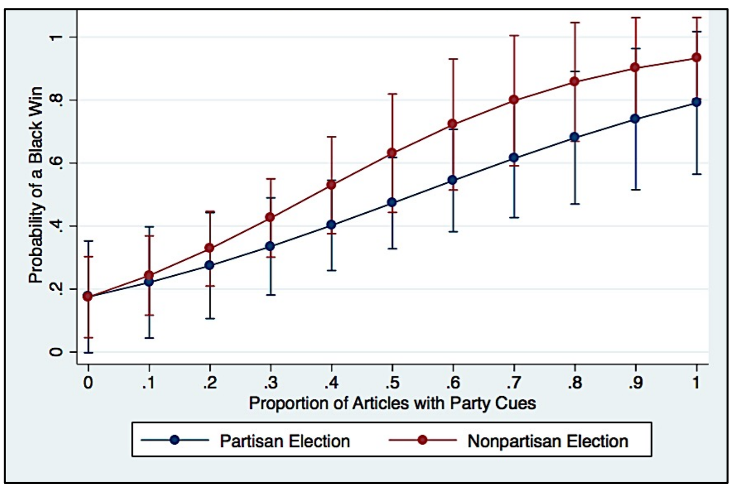

Figure 1.5. Effect of Party Information on the Probability of a Black Candidate Winning Partisan or Nonpartisan Election (with 95% confidence intervals).

What remains puzzling about the findings from both Table 1.3 and Table 1.4 is why party information also has a positive effect on black electability in partisan elections – where party affiliation is plainly marked on voter ballots. Although the theory of minority electability – that voter access to party information offsets negative biases stemming from racial cues – was meant to highlight the influential role that unmeasured levels of party information have in nonpartisan elections, it might be the case that additional information about candidate partisanship chips away at the relative importance voters place on racial cues in officially partisan elections as well. Put simply, black candidates benefit from elections where party affiliation becomes an increasingly central aspect of the contest regardless of whether that election is officially partisan or nonpartisan. This suggests that releasing more information about their partisanship to voters could be a well thought-out campaign strategy employed by the candidate to increase their vote share. Indeed, Spiliotes and Vavreck (2002) report that candidates are particularly tactical when deciding whether to make partisanship a main component of their campaign rhetoric. In this scenario, the relationship between partisan information and vote share/election outcome may be more endogenous than causal. I examine this possibility in the following section.

Robustness Checks

In this section I perform a series of robustness checks on our main models (3.2, 3.4, 4.2, and 4.3) to address the potential limitations of a causal relationship between party information and minority electability. First, as suggested above, it is possible that levels of party information vary in response to expectations about how such information will affect voter preferences. If this is the case, then to accurately assess the relationship between party information and vote share we need to identify a set of instrumental variables that are highly correlated with former, but have no direct effect on the latter. These instruments will be used in a two-stage least squares regression model (TSLS) to estimate the effect of our potentially endogenous variable, party information. I use two variables to do this: whether or not the candidate's main opponent identifies with a different party and whether or not the city's news outlets have a pronounced partisan bias.

The first instrument, *opposition party*, is coded 1 if the white candidate's party is different from the black candidate's party and 0 if they both affiliate with the same party. According to our theory of minority electability, the use of party information helps to offset negative biases stemming from racial cues, but this information is considerably less valuable to voters if both candidates are from the same party. The application of this idea to nonpartisan elections is clear: candidates should be more prone to using party cues when their opposition is from another party and less so when they share partisanship. But how does this help us to understand the utility of party information as a campaign strategy in partisan elections, which presumably produce candidates from the same party in primaries and opposite parties in general elections? The interesting thing about mayoral elections is that we actually do find cases where officially partisan elections yield different-party candidates in primaries and same-party candidates in general elections. In some cases this results from cities that offer open primaries. In other cases, it is not uncommon for the runner-up in the primary election to continue their run into the general election if the city is overwhelmingly Democratic or Republican. In this dataset, 32 percent of the "partisan" general elections feature candidates from the same party.

While it seems probable that a candidate would seek to share party information only in contexts where they believe it will increase their odds of winning an election, their campaign strategy cannot control for how the media reports it. Traditional models of vote choice depicted the media as a conduit for elite discourse (Bennett 1990; Zaller 1992), but we now know that the media both report the campaign as carried out by the candidates and offer their own point of view (Box-Steffensmeier et al. 2009). Recall that one of the major setbacks faced by minority candidates is the inordinate amount of attention that the media draws to their race or ethnicity even when they attempt to run a race-free campaign (Kaufmann 2004). This means that while media references to a candidate's partisanship may be the consequence of the candidates bringing party into their campaigns, we need to also accept the possibility that the media itself has a stake in offering this information to its readers. Under such conditions we should find the highest levels of party information in cities that have particularly partisan news outlets. To account for this, I use Gentzkow and Shapiro's (2010) index of media slant, which measures the partisan slant of newspapers in major cities across America. Gentzkow and Shapiro developed this index by measuring the frequency with which newspapers used phrases regularly employed by Republicans or Democrats as captured in the *Congressional Record*. From this index, I created our second instrumental variable, partisan news, which is coded 1 if the news outlet in the city is especially prone to using partisan language and 0 if its content is "unbiased," which in this case means that it rarely frames its stories with a Republican or Democratic angle.

Transforming Gentzkow and Shapiro's original index into a binary scale is done for two reasons. First, given that we want to account for the possibility that cities with partisan news outlets are more likely to report the party affiliation of the candidates *regardless* of their partisan bias, categorizing a city's news source as partisan (or not) makes more sense than if we were to measure to what degree the news reports stories with a Democratic or Republican spin. For example, a Republican-leaning news source may be more likely to reveal a Democratic candidate's party affiliation to persuade voters to cast their ballots *against* them even if the said Democrat attempts to minimize public knowledge of their party. The second reason for the transformation is to eliminate any concern of collinearity between the political preferences of a city (captured by presidential vote share in our model) and the political leanings of the newspaper.

Table 1.5 reports the first stage of the TSLS regressions, which estimate how well our instrumental variables predict party information, and the results of a series of specification tests done to ensure that the instruments chosen are a viable substitute for party information. The regression model employed uses a limited information maximum likelihood estimator (LIML), which is more robust to the presence of weak instruments (Hahn et al. 2004). As shown in both models 5.1 and 5.2, the instruments are highly significant predictors of the percentage of party information and, aside from nonpartisan elections and education level, are the only variables that explain variation in the level of party information. While the statistical significance of these instruments is a good sign, it is not enough to conclude that they are sufficient instruments. As advised by Sovey and Green (2010), I further test the tenability of these variables as instruments and provide the results at the bottom of the Table 1.5.

Following Stock and Yogo (2005), I performed a test of weak instruments, which looks at the ratio of the bias of the estimator to the bias of the OLS estimator. If the null hypothesis – that the instruments are weak – holds, then we would conclude that the instrumented model's estimates are biased. According to Staiger and Stock (2002) a key criterion for measuring the reliability of an instrumental estimator is that it has an F-statistic of 10 or greater. The two reported in models 5.1 and 5.2 are 22.96 and 20.55, respectively. Using these values we can easily reject the null hypothesis that the chosen instruments are weak. In other words, our models tolerate a bias of no more than 5% in the TSLS estimator. Additionally, the significant p-value indicates that the instruments (opposite party and partisan news) have meaningful explanatory power for the percentage of party mentions even *after* controlling for the other covariates (e.g., nonpartisan elections in Model 5.1 and all of the additional covariates in Model 5.2).

The Anderson-Rubin test (1950) (A-R test) further confirms that the instruments chosen are significant: the null hypothesis, that the coefficients of the endogenous regressors in the equation are jointly equal to 0, is rejected. The A-R test is especially useful to test the robustness of potentially weak instruments since the power of the test is tied to the strength of the instruments. In other words, as instruments become weaker, the power of the test declines and the null is less likely to be rejected.

Table 1.5. First-Stage Regressions for Estimating Party Information

	5.1 % Party Mentions	5.2 % Party Mentions
Opposite Party	0.172^{***} (0.036)	0.154^{**}(0.060)
Partisan News	0.135^{***} (0.037)	0.209^{***}(0.039)
Nonpartisan elections	-0.182^{**}(0.036)	-0.131^{*}(0.053)
Racial Cues		-0.047 (0.106)
Incumbent		-0.012(0.052)
3(+) Candidates		0.064(0.048)
Election Type		
General		-0.085(0.059)
Runoff		-0.106(0.104)

Population		0.000(0.000)
% Black		-0.002(0.002)
% White		0.004(0.002)
Share of pres. vote for candidate's party		-0.000(0.001)
Bachelor's degree		-0.009**(0.003)
Median household income (logged)		-0.158(0.134)
% Unemployed		0.007(0.006)
Constant	0.296***(0.033)	2.027(1.472)
N	128	126
F statistic	22.96 (*p-val*: 0.0000)	20.55 (*p-val*: 0.0000)
A-R statistic	30.36 (*p-val*: 0.0000)	11.16 (*p-val*: 0.0038)
Kleibergen-Paap rk LM statistic	27.53 (*p-val*: 0.0000)	25.57 (*p-val*: 0.0000)
Basmann F-test	0.005 (*p-val*: 0.9411)	0.039 (*p-val*: 0.8434)

Note: Models 5.1 and 5.2 are the first stage of a Two Stage Least Squares (TSLS) regression and use a limited information maximum likelihood estimator. Both models include a dummy variable for contests that occurred in the same election sequence and report robust standard errors.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

The Kleibergen-Paap (2006) rk LM statistic tests for underidentification. The significant p-value tells us that the chosen instruments adequately identify the equation. This test is a generalization of the Anderson (1951) or Cragg and Donaldson (1993) correlation tests to the non-*i.i.d.* case and, thus, has the advantage of its results being robust to heteroskedacity, autocorrelation, and clustering. The fourth statistic, Basmann's (1960) F-test, checks for overidentification in two ways: whether the instruments are uncorrelated with the error term and whether any of the excluded exogenous variables should be included in the structural equation. In this case, a significant test statistic would mean that our model employs a poor instrument or fails to treat one of the exogenous variables as an instrument. As reported, both F-statistics are non-significant, suggesting that our instruments are a good fit and that the models are correctly specified.

The chosen instruments appear to satisfy various tests of robustness. The next step is to test whether controlling for the possibility of endogeneity changes the relationship between party information and black vote share. Re-estimation using the instrumental variables in Table 1.6 shows that party information continues to affect a black candidate's vote share in a meaningful way. Comparing Model 3.2 to Model 6.1 we find that the coefficients for both party information and nonpartisan elections increase substantially. While nonpartisan elections appear to have a stronger influence on the black candidate's vote share, the relative impact of the two variables on our outcome remains unchanged. The difference between Model 3.4 and Model 6.2 is much more subtle. Again, we find a slight increase in the coefficients for party information, but no significant change in terms of its marginal effect on vote share or election outcome. The main difference we find is in Models 6.3 and 6.4, which report that nonpartisan elections now appear to significantly affect the probability of a black candidate winning an election. Yet, party information continues to play a strong role in predicting black electability even with this newfound relationship. Overall, the TSLS estimations provide confidence that our original models sufficiently tested and correctly reported the relationship between party information and black electability.

Table 1.6. Second-Stage Results for the Effects of Party Information on Black Electability

	6.1: Vote Share	6.2: Vote Share	6.3: Election Win	6.4: Election Win

% Party mentions	68.016*** (14.547)	30.33** (10.163)	4.155***(0.560)	4.663***(0.741)
% Race mentions		-1.777(7.028)		-0.114(0.798)
Nonpartisan elections	7.372(4.230)	4.719(4.026)	0.652*(0.255)	0.877*(0.387)
Incumbent		26.05***(3.868)		1.749**(0.604)
3(+) Candidates		-8.277**(2.868)		0.304(0.315)
Election Type				
General		2.419(3.825)		-0.0460(0.373)
Runoff		8.329(8.456)		-0.202(0.680)
Population		-0.121(.063)		-0.372**(0.013)
% Black		0.0286(0.105)		-0.0126(0.0115)
% White		-0.120(0.147)		-0.030(0.0163)
Share of pres. vote for candidate's party		0.108(0.0756)		0.013(0.010)
Bachelor's degree		0.0605(0.173)		0.029(0.016)
Median household income (logged)		1.008(7.645)		0.498(0.719)
% Unemployed		-0.290(0.427)		-0.084(0.45)
Constant	9.726 (6.740)	10.84(80.07)	-1.977*** (0.308)	-6.693(7.495)
N	128	126	128	126
R^2	0.168	0.527		

Note: Models 6.1, 6.2, 6.3, and 6.4 are the second stage of a Two Stage Least Squares (TSLS) regression model. Models 6.1 and 6.2 use TSLS regression with a limited information maximum likelihood estimator while Models 6.3 and 6.4 use TSLS probit regression. All models include a dummy variable for contests that occurred in the same election sequence and report robust standard errors.
$^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

It is also possible that the model suffers from another form of endogeneity: omitted variable bias. Perhaps, for example, cities with higher levels of turnout or more competitive elections have more partisan cues. To address the possibility of an omitted variable influencing the results, I created a smaller data set of 19 cities that have multiple observations. In Table 1.7, I use city fixed effects to check the robustness of the results. Given the small number of observations for this dataset, I limit its testing to the black candidate's vote share. Using this smaller data set yields the same pattern of results: party information, as tested in Model 7.1, explains nearly 21 percent of the variation in an African-American candidate's vote share. When we combine this with our other covariates in Model 7.2 more than 56% of the variation within cities and 58% of variation across cities is explained. Since a city's election type (partisan or nonpartisan) is unchanging for the years covered by the dataset, Model 7.3 limits our observations even further by considering the effect of party information in a "nonpartisan" setting exclusively. Model 7.3 looks remarkably similar to Model 7.2, but loses significance in the statistical sense with a reported p-value of 0.108 for the percentage of party mentions.

Table 1.7. Fixed Effects Models for Black Vote Share

	7.1	7.2	7.3 (*NP Elections Only*)
% Party mentions	29.82^{*}(12.68)	27.14^{**}(9.323)	20.79 (12.57)
% Race mentions		-6.268 (10.40)	-10.16 (14.72)
Incumbent		23.66^{***} (3.752)	29.03^{***} (5.250)
3(+) Candidates		-11.31^{**} (3.839)	-4.961 (5.338)
Election Type			
General		-2.728 (4.199)	24.86(13.77)

Runoff		5.105 (7.137)	36.87*(14.83)
Constant	30.18***(4.902)	30.27*** (6.173)	8.292(14.32)
N	*93*	*91*	*49*
City Groups	*19*	*19*	*11*
Within R^2	*0.07*	*0.555*	*0.619*
Between R^2	*0.209*	*0.584*	*0.369*
Overall R^2	*0.119*	*0.567*	*0.545*
AIC	*8.198*	*7.606*	*7.477*

Note: Models 7.1, 7.2, and 7.3 use Ordinary Least Squares (OLS) regression with a dummy variable for contests that occurred in the same election sequence and city fixed effects.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Third, some elections may have a high proportion of articles that mention candidate party, but very few actual articles about the election. For example, Table 1.1 tells us that some contests had as few as 9 articles about the election. If 5 of those mention the black candidate's party, then a whopping 56% of articles have party cues, but there are still relatively few opportunities for voters to obtain information about candidate party. To account for this possibility I reran the models above using the total number of articles with partisan cues (as well as the total number of articles with racial cues) as the key independent variable. Party information does lose some of its explanatory capacity in models 8.3 and 8.6 (the number of party mentions has a p-value of 0.061 in Model 8.3 and a p-value of 0.057 in Model 8.6), which include other potential predictors of black support. Despite this, the overall results shown in Table 1.8 confirm that even when measured as a count, partisan cues continue to influence black electability.

Table 1.8. Effect of Number of Party Mentions on Black Vote Share & Electoral Victory

	8.1: Vote Share	8.2: Vote Share	8.3: Vote Share	8.4: Victory	8.5: Victory	8.6: Victory
# Party mentions	0.163** (0.0527)	0.156** (0.0548)	0.144^ (0.076)	0.016* (0.006)	0.018* (0.007)	0.0367^ (0.018)
# Race mentions			-0.187 (0.117)			-0.041^ (0.026)
Nonpartisan elections		-1.201 (3.492)	4.072 (3.660)		0.221 (0.428)	1.297^ (0.702)
Incumbent			27.01*** (4.482)			3.91** (1.236)
3(+) Candidates			-11.81*** (3.002)			-0.483 (0.619)
Election Type						
General			3.287 (3.598)			0.579 (0.747)
Runoff			9.357 (8.388)			0.685 (1.302)
Population (logged)			2.052 (2.158)			0.341 (0.388)
% Black			0.185 (0.128)			0.021 (0.024)
% White			0.157 (0.160)			0.028 (0.032)
Share of pres. vote			0.082 (0.077)			0.024 (0.019)
Bachelor's degree			-0.0215 (0.168)			0.0164 (0.034)
Median household income (logged)			-4.169 (8.125)			-0.355 (1.554)
% Unemployed			-0.0329 (0.441)			-0.065 (0.084)
Constant	33.00*** (2.364)	33.87*** (3.334)	31.84 (94.47)	-0.784** (0.252)	-0.946* (0.381)	-6.385 (17.727)
N	128	128	126	128	128	126
R^2	0.09	0.091	0.546	0.030	0.038	0.294
AIC	8.684	8.720	8.180	1.362	1.365	1.197

Note: Models 8.1, 8.2, and 8.3 use Ordinary Least Squares (OLS) regression and Models 8.4, 8.5 and 8.6 use logistic regression. All models have a dummy variable for contests that occurred in the same election sequence and robust standard errors in parentheses. $^{\wedge} p < 0.1$, $^{*} p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$.

Conclusions

This paper sought to resolve why observational and experimental studies of minority electability tend to produce diverging conclusions about the relationship between candidate race, partisanship, and vote choice. Combining a novel dataset of mayoral elections with a content analysis of election news coverage, I demonstrated that when we properly account for partisan information, the real and experimental worlds produce similar conclusions: party cues improve black electability. This has important methodological and substantive implications.

Methodologically, the findings confirm that it takes a continuous measure of party information to truly capture how party affects minority electability. Indeed, as noted in the discussion of the models from Table 1.3 and Table 1.4, a black candidate gains more than 10 percentage points of the vote share when moving from a nonpartisan election with no party information to one where half of all articles reference party. Furthermore, that candidate has a nearly 60 percent chance of winning their election. Yet, had we relied on the conventional binary classification of partisan/nonpartisan elections we would have concluded that a black candidate has only a 40 percent chance of being elected in *any* nonpartisan election**.** Clearly, relying on a partisan/nonpartisan distinction misinforms us about the relationship between party, race, and vote choice and, consequently, encumbers our theoretical understanding of minority electability.

Substantively, this means that the official status of the election – partisan or nonpartisan – rarely reflects the actual role that party is playing in the contest. Indeed, aside from incumbency and the number of candidates running, voter access to party information is the most consistent factor for explaining voter support for black candidates. Again this highlights the importance of how we measure partisanship: when we take the amount of party information available into account, it reveals that black candidates have a tool they can use to overcome racial prejudice in partisan and nonpartisan elections alike.

There is unmistakably an important role for party information to play in future models of voting behavior. That said, the cue-based theory of minority electability presented here still requires some development. First, there is a very real chance of the relationship between party information and vote choice being endogenous. While the two-stage least squares regressions reported in Tables 1.6 and 1.7 does its best to account for this possibility, it is difficult to identify strong instrumental variables that completely dispel concerns about endogeneity. However, there is evidence to indicate that this theory also holds in an experimental setting, which would confirm the causal influence of party information on minority electability (Jaeger, forthcoming).

Second, it would be informative to determine *whose* vote choice is affected by party information. Do black candidates make comparable gains in their support from Republicans, Democrats, and Independents when their party affiliation is revealed? Considering that the vast majority of African-Americans do identify as Democrats, it is feasible that revealing a Republican identity could lead to a profound boost in Republican support, but also cost them votes amongst liberals. Likewise, does the availability of party information have similar consequences for voters of all racial and ethnic backgrounds? Possibilities such as this raise questions about whether party information is *always* beneficial to black electability.

Another important line of inquiry would be to consider other minority candidates: do Latino and Asian candidates experience the same boost in voter support from party information? Both Latinos and Asians are less likely to be associated with a particular party affiliation, so it could be that partisan cues are *even more* consequential for how voters evaluate their candidacy. Future work should pursue these questions through a combination of additional observational data and experiments. Combining these approaches would assist in fully developing and testing the causal claims behind the theory of minority electability.

Given what we know about the primacy of party affiliation for voting behavior, research concerning local elections should be better at incorporating appropriate measures of party into its models. It is not simply a matter of distinguishing which elections put party labels on the ballot and which do not. Rather, this paper demonstrates that party can play a fundamental role in determining who wins elections even when it has no "official" role.

CHAPTER TWO

Securing Communities or Profits?

The Effect of Federal-Local Partnerships on Immigration Enforcement

Courts and legal scholars have consistently placed the power to regulate immigration within the realm of federal authority. Yet, the National Conference of State Legislatures (2015) reports that since 2010, state and local governments have enacted 1,655 laws and resolutions related to immigration. Although these initiatives were not met without contestation, rulings by the Supreme Court suggest that immigration policy is no longer exclusively within the federal domain (Chacon 2012; Chin and Miller 2011). Furthermore, the federal government has reformed its own approach to immigration by actively pursuing partnerships with local actors to expand immigration enforcement from the border into the country's interior. Although subnational assistance in immigration enforcement was clearly recognized in the 1996 amendments to the Immigration and Nationality Act, the government's more recent push for states to use 287(g) authority and their execution of programs such as Secure Communities and the newly fashioned Priority Enforcement Program, sends a strong message: immigration control now operates under a decentralized agenda. Scholarly inquiries attempting to make sense of the many and varied local policy initiatives popping up across the country have become common, but to date there exists no underlying theory to explain when and why local actors cooperate with federal initiatives. Since all signs point to a continued role for local actors in the execution of federal immigration enforcement, this paper uses the case of the

Secure Communities program to develop a better understanding of federal-local partnerships in terms of program implementation and outcomes.

Secure Communities, also known as S-Comm, is an interior deportation program that depends on local law enforcement to assist federal agents in apprehending and detaining unlawful residents. Unlike 287(g), where local actors volunteered to partner with Immigration and Customs Enforcement (ICE) to enforce federal immigration laws, counties were "activated" into S-Comm via a federal directive. Secure Communities was seen as a successor to the 287(g) program and a model for how the government plans on moving immigration enforcement forward. Indeed, its replacement, the Priority Enforcement Program, has the same policy objectives and relies on a similar level of local cooperation in order to achieve its purpose. In this way, Secure Communities represents not only an appropriate case study to examine how immigration enforcement has been carried out, but also a realistic example of what implementation programs will look like.

In this paper, I show that because Secure Communities depends on the cooperation of local law enforcement agencies to achieve its mandate, any conclusion about the program's outcomes depends on an understanding of the *partnership* between federal and local actors. Drawing on existing theories of intergovernmental policy implementation, I reveal that ICE uses financial incentives to simultaneously minimize their own logistical barriers to implementation and motivate local cooperation. Moreover, unlike existing studies on local responses to immigration, I find no evidence to suggest that Republican-leaning counties produce more deportations. Instead, resource capacity appears to strongly moderate the relationship between political orientations and deportation outcomes.

The Secure Communities Partnership

In the face of opposition from governors, law enforcement officials, and immigrant rights advocates, the Obama administration oversaw a rapid rollout of the Secure Communities program – expanding it from 14 jurisdictions in 2008 to all 3,181 jurisdictions by 2013. The program relies on local cooperation at two specific phases. In the pre-custody phase, law enforcement personnel are expected to run the fingerprints of apprehended individuals through the FBI criminal database. That information is then automatically forwarded to ICE to be checked against the Department of Homeland Security's biometric database, IDENT, which links digital fingerprints, photographs, iris scans, and facial images to biographic information meant for verifying identities. If the database produces a "match," indicating the individual is eligible for removal, ICE issues a detainer request asking the law enforcement agency (LEA) to hold the individual until ICE can assume custody.

Once ICE assumes custody, immigrants are transferred to detention centers. Since 2005, the number of immigrants in federal detention waiting for a deportation decision has doubled since 2005 to more than 400,000 individuals a year (TRAC 2015). Given that there are less than 8,000 detention beds in facilities owned and operated by ICE, the agency faces a considerable logistical dilemma. To overcome this predicament, ICE contracts local jails and private prison companies to assist in housing detainees. Although post-custody detainment was not a planned aspect of the federal-local partnership, it has become essential to S-Comm's implementation and, consequently, fundamental to explaining deportation outcomes.

Political Culture, Resources, and Implementation

Federal-local partnerships are now considered a common approach to policy implementation in the U.S. (O'Toole 2000); however, the strategic value of this method for achieving successful policy implementation is continually called into question because of principal-agent problems. Local compliance with federal initiatives can be erratic, producing inconsistent and often undesirable outcomes (Hill and Hupe 2002; Shapiro 2005). The resulting disconnect between policy objectives and policy outcomes has become known as "the implementation gap."

One explanation for the implementation gap is that local actors bring their own normative standards and political cultures into the implementation process. The consequence is that policies are interpreted and enacted to fit preexisting agendas and political preferences resulting in uneven levels of compliance (Cho et al. 2005; Keiser and Soss 1998; Mazmanian and Sabatier 1981; McLaughlin and Talbert 2001; Meier and O'Toole 2006; Spillane et al. 2002; Weissert 1994). Others have shown, however, that without the necessary financial and physical resources to implement a policy, local actors are pressed to limit or modify their compliance regardless of normative or political preferences (Barrilleaux, Feiock, and Crew 1992; Hasenfeld and Brock 1991; Lipsky 1984; Scholz and Wei 1986). Since the early 1990s, an increase in federal mandates and a simultaneous decline in federal aid have made it even more difficult for the federal government to obtain consistent and comprehensive cooperation across local governments (Ray and Conlan 1996). In effect, states and local agencies have become "reluctant partners" in the task of intergovernmental implementation (Stoker 1991).

Chand and Shreckhise (2014) clearly situate local compliance with the Secure Communities program in the political culture camp, arguing that Republican jurisdictions should be more apt to comply with the S-Comm mandate and engage in more deportation efforts than their Democratic counterparts precisely because it is inline with their Party's ideological commitment to pursue a tough immigration enforcement program. Given the strong correlation between Republican governments and the adoption of other restrictive immigration efforts at the local level (Chavez and Provine 2009; Creek and Yoder 2012; Ramakrishnan and Wong 2010; Zingher 2014), this line of thought makes sense. Additionally, compliance with Secure Communities should not place any additional fiscal burden on local governments and agencies – in theory.

In the following section, I show, however, that Secure Communities does impose financial hardship on counties. With this clear, I argue that a more nuanced relationship between political orientations, resources, and policy compliance exists. The resource-based framework that I propose does not disregard a role for political orientations, but insists that financial capabilities and incentives are foundational to understanding when and where deportations are most likely to occur. This develops existing theory on intergovernmental policy implementation, which has been described as "unsatisfactory" (Weissert 2001) and "lacking rigour" (Kettl 2000), in two ways. First, it demonstrates that we need not rely on political culture or resources alone to explain policy compliance. As the case of Secure Communities illustrates resources are a prerequisite to policy implementation while the influence of political culture comes later. Second, whereas extant studies have highlighted the importance of local resource capacity, I submit that because intergovernmental policy implementation is a *partnered* effort it depends on the resource capacity of both local *and* national actors.

A Resource-based Framework

Secure Communities was intended to be a "simple and common sense way" to improve federal removal efforts in the country's interior without creating an additional burden on local agencies (Department of Homeland Security 2015). Despite this, reports across the country suggest participating in Secure Communities takes a toll on local resources. In Washington State the program has increased average jail time by 161 percent, driving up detention costs by about $3 million (Beckett 2013). In Los Angeles County, California, where immigrants are detained 21 days more than the average inmate, the county pays more than $26 million a year (Greene 2012). And, in New York City, ICE detainers led to non-citizens staying, on average, approximately 73 more days in jail than other offenders (Shahani 2010). The Salt Lake City Police Chief, Chris Burbank, comments that when local officers engage in immigration enforcement, it "diverts resources away from [the department's] central responsibilities during a time of budget cuts and staffing shortages" (U.S. Congress 2010). Lieutenant Michael Barry of Martin County, Florida, further explains how the demands of fulfilling the Secure Communities directive places a strain on law enforcement:

> Sending I.A.Q's [Immigration Alien Queries], waiting for responses, making phone calls to different immigration officials for clarification on detainees status, gathering additional information for immigration such as photos, booking sheets, fingerprints, and palm prints takes away from the deputies regular duties.

These officers make clear that even if an agency with *wants* to cooperate, their limited resources may make them *incapable* of doing so. Thus, we should see a marked difference in deportation rates depending on the existing budgets of local law enforcement:

> ***Resource Constraints Hypothesis****: jurisdictions with larger operational budgets for law enforcement will have more deportations than those with smaller budgets*.

Yet, the policing resources of local governments are only half of the equation. As Hall and O'Toole (2004) note, unlike traditional studies of policy implementation, which look to its hierarchical nature to inform theories, many of today's policies are applied in a "horizontal" environment, where "national programs are implemented by a variety of different kinds of actors both within and without government" (190). McGuire (2006) notes that while scholars of policy implementation have been quick to note the structural differences that "collaborative" policy implementation brings, research has been slow to assess the effects of partnered efforts on policy outcomes.

Just as law enforcement agencies often lack the financial resources to hold individuals until ICE assumes custody, ICE agents, as "aerial-level" bureaucrats, also face resource limitations. Specifically, once ICE agents assume custody they need a place to house detainees waiting on a deportation decision. Although Congress set a detention-bed mandate that requires ICE to "maintain a level of not less than 34,000 detention beds" at any given time (Office of Management and Budget 2014, 520), ICE owns and operates less than 8,000 beds. Consequently, ICE has entered into Intergovernmental Service Agreements (IGSAs) and private contracts with local jails and privately owned correctional facilities to house detainees. As shown in Figure 2.1, the usage of IGSA and private facilities by ICE is widespread.

IGSAs and private contracts not only offer a solution to ICE's logistical dilemma, but they also encourage local compliance by compensating private, local, and county facilities an average of $119 per day per bed filled (DHS 2015, 60). The majority of the more than 400 Intergovernmental Service Agreements are direct contracts between ICE and local governments, but an analysis of ICE records shows that 33 percent of IGSAs are for use of local jails that are operated by private prison companies. In such cases, local governments receive a bed-fee from ICE as well as annual compensation from the prison companies they have contracted. For example, an internal audit by ICE (2012) of its contract with Pinal County, Arizona, reveals that the county made approximately $9.6

million from its contract in 2012. In the same year, Pinal County's contract with private prison giant, Corrections Corporation of America (CCA), contributed $1.1 million to the county's general fund (Kirkham 2012).

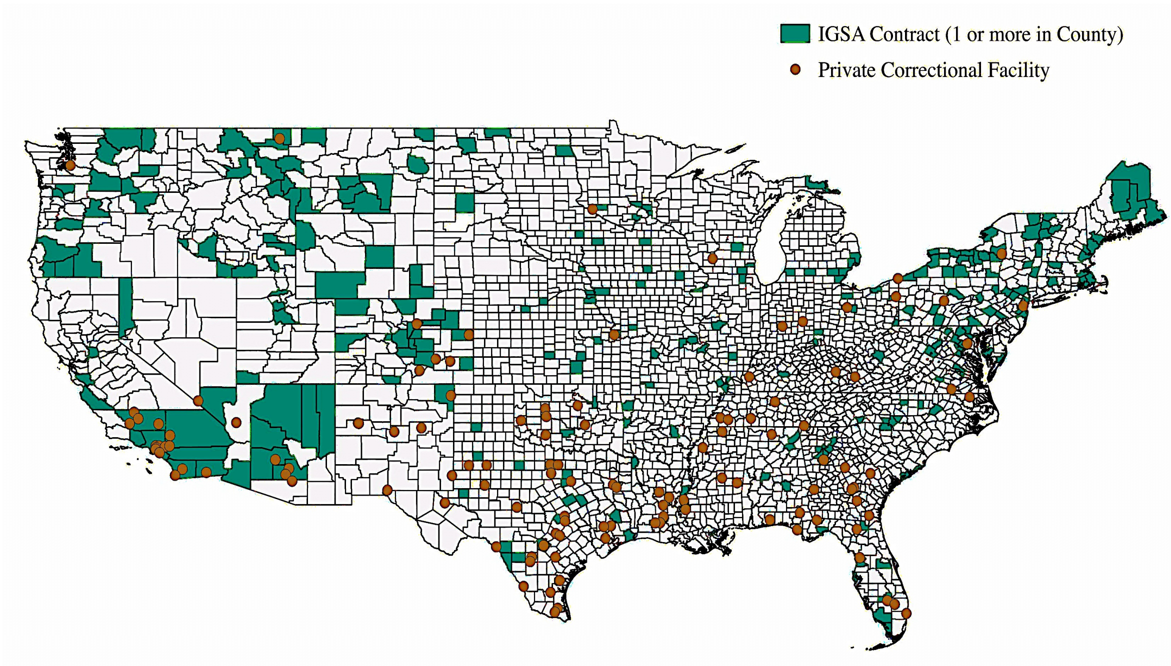

Figure 2.1. IGSA and Private Correctional Facility Locations. This map displays the counties that have entered into Intergovernmental Service Agreements with the federal government in order to house detainees (green-shaded regions) and the location of private correctional facilities (brown dots). As shown, a substantial number of the private facilities are located within IGSA counties.

The relationship between private prison corporations and federal and local authorities is becoming even more convoluted. In 2014, Eloy, Arizona, agreed to be the financial go-between for ICE and its new CCA immigrant detention facility in Dilley, Texas – yes, Texas. Despite being located more than 900 miles from the facility, the city of Eloy modified an existing IGSA with ICE to expand its contract to cover the Dilley facility. According to an ICE spokesman, the use of an IGSA allowed ICE to avoid a competitive bidding process, saving them an estimated “18 months to get the facility up and running” (Burnett 2014). For the city of Eloy, which is promised $0.50 per bed per day (regardless of whether its filled), this translates to about $438,000 a year to, as Eloy City Manager Harvey Krauss, puts it, “manage the money” (Harris 2015).

Even when IGSAs are not part of the story, ICE contracts with private detention facilities may still increase the number local deportations resulting from Secure Communities, albeit indirectly. In 2009, 49 percent of detention beds were in private facilities. Today, private beds house 62 percent of all detainees (Carson and Diaz 2015). Indeed, of the ten largest immigrant detention centers, nine are privately run and as the demand for bed space continues to grow so too does bidding for contracts to build new private facilities. Knowing the potential financial return, local governments often compete for the right to build facilities within their jurisdictions. This is what led to the Willacy County Detention Center in Texas, once the largest immigrant detention center in the United States. In return for a daily fee for each inmate held, Willacy County agreed to assume the financial risk of building the facility, which was contracted by ICE and is run by Management & Training Corporation (MTC). In 2014, revenue from the facility injected $2.7 million into the county’s $8.18 million general and capital funds (Del Valle 2015).

By contracting out detention services, ICE created a solution to their capacity problem and a financial incentive for local cooperation. If resources truly are foundational to cooperation, then incentives are logically the second key aspect to this framework:

> ***Financial Incentives hypothesis****: jurisdictions with IGSAs or private correctional facilities will have more deportations.*

Stoker (1991) claims that to bring ideologically reluctant actors on board the federal government must alter the conditions of the implementation process in a way that incentivizes them to cooperate with the implementation procedures as intended. I argue that the case of Secure Communities illustrates that incentives do not require policy modification to persuade the reluctant, but simply policy *appeal* to attract not only the ideologically averse, but also the resource deficient. In short, promising dollars for detainment solves two problems at once, fueling cooperation and, as a result, resolving ICE's capacity predicament.

Unlike existing theoretical perspectives, which portray resource and ideological-based explanations as competing theories to explain variation in policy implementation, I suggest that once LEAs have resources, therefore, the *ability* to comply with the Secure Communities directive, it is more likely that county political orientations will help to explain the extent to which they cooperate with ICE. Specifically,

> ***Political Orientations hypothesis****: Republican-leaning counties will yield more deportations if they have larger policing budgets, are part of an IGSA, or have a private correctional facility in their jurisdiction.*

As the above discussion indicates, this paper assumes that differences in levels of compliance result from a county's resource security, first, and their political preferences, second. These predictions break with previous work on local immigration policy and compliance with the Secure Communities program specifically, which emphasize the dominant role played by partisan preferences. In the following section I discuss the Secure Communities deportations data and how its unique structure warrants a particular type of statistical analysis. Correctly analyzing this data is crucial to accurately test the merits of the resource-based theory advocated here against prevailing theories that emphasize political ideology.

Research Design and Data

To assess the impact of federal-local partnerships on the implementation of the Secure Communities program, I engage in a large-N analysis of county-level deportations from 2008 until the program's termination in 2014. The dependent variable, the total number of deportations within each county, ranges from zero to thousands across the activated jurisdictions with the average number of deportations at 127.[7] It should be noted that the

[7] The appendix provides a more detailed discussion and series of robustness checks to reassure the reader that the dependent variable's large range is not affecting the results.

dependent variable has several particular characteristics that influenced my choice of modeling strategy. First, county deportations are both discrete and non-negative counts, suggesting the use of a Poisson model. However, because the variance is much greater than the mean in this particular data, the standard assumption of the Poisson model, that the variance is equal to the mean, is violated (Hilbe 2014).

The negative binomial regression model (NBRM) is a well-equipped alternative to account for overdispersion. Unfortunately, the NBRM is based on the expectation that the percentage of zero counts in the data will go down as the mean increases. In this case, however, the data produces far more zeros than expected: 26 percent of the counties experienced no deportations while NBRM predicts nearly 0 percent. To account for excess zeros and overdispersion, I turn to the zero-inflated negative binomial model (ZINB). Rather than making inferences about the relationship between the number of 0 counts and the mean, the ZINB model estimates the number of deportations by incorporating the zero counts into both a binary *and* a negative binomial model. Specifically, for each observation, there are two possible data generating processes: one that is generated by a binary distribution producing only zero counts and one that is governed by a negative binomial distribution, some of which are zero. In other words, it assumes that some counties will always produce 0 deportations, while others might report 0 deportations, but in fact have a positive probability of having more than 0 deportations. Additionally, comparisons of NBRM and ZINB using the AIC and a Vuong test reveal that the ZINB model is the better choice.[8]

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[8] Results from these tests are shown in the model outputs below.

My overview of the modeling strategy is important not only for understanding how I arrived at my results, but also why I expect my results to differ from those of Chand and Schreckhise (2014), currently the only other empirical study examining why deportations deriving from Secure Communities vary across counties. While Chand and Schreckhise provide a nice foundation for inquiry, we should be cautious of their results for a couple of reasons. First and more simply, their data is incomplete. Since it contains deportation outcomes only through May 2013, it leaves out about a year and a half of additional data. Second and more importantly, Chand and Shreckhise use OLS to model a count outcome – never explaining how their data fit the model or avoid violating key assumptions of OLS. This in and of itself warrants that their findings be interpreted with a considerable amount of caution. In the section that follows, I test their model using OLS and ZINB regression in order to demonstrate the necessity of modeling the deportations data with a ZINB model.

As the hypotheses make clear, there are four primary explanatory variables that are examined. Summary statistics for these variables and other control variables can be found in Table 2.1. The first independent variable, the operational budget for law enforcement in the county, comes from the Department of Justice's 2008 Census of State and Local Law Enforcement Agencies. This data is especially pertinent as it provides budget information for not only the county Sheriff's office, but also local police departments within the county. Combining statistics from all LEAs with the capacity to hold or transfer inmates, I created a variable for the operating law enforcement budget per county resident.

The second predictor is a dummy variable, with 1 indicating that the jurisdiction has at least one private correctional facility in its borders. This data was collected using the facility location information found on the websites of the three largest private prison companies: CCA, GEO Group, and MTC. Similarly, I compiled an indicator variable for whether or not a county has an IGSA with ICE using a DHS list of ICE contracted detention facilities. The final explanatory variable is each county's average share of Republican votes in the 2008 and 2012 presidential election.

If the implementation of Secure Communities has been carried out as its purpose states, to target unlawful immigrants, then we should see markedly more deportations take place in counties with higher proportions of unauthorized immigrants and higher rates of crime. Unfortunately, approximations of the size of the unauthorized population are only available at the state level (Passel and Cohn 2011). Thus, I supplement this predictor with the county's proportion of foreign-born residents (U.S. Census 2010) to indicate the percentage of *potentially* deportable individuals. For the crime rate, a composite variable of the number of violent and property crimes was compiled using the 2011 FBI Uniform Crime Reporting Statistics.

In addition to the main predictors of interest, there are several control variables. Following Chand and Schreckhise (2014), the first group attempts to capture established attitudes toward immigration within counties. The first variable is coded 1 if the county includes any 287(g) participants. I also include a measure of state-level voter support for Arizona's SB1070 (a 2010 Rasmussen Poll whose results are listed at the *Federation for Immigration Reform* website). Support for SB1070 and 287(g) participation are both meant to identify those counties that might be more predisposed to compliance. Finally, a dummy variable representing whether or not the county is located in a state that has passed a Dream Act (National Conference of State Legislatures 2014) is included to account for jurisdictions that may be more reluctant to comply.

Table 2.1. Summary Statistics for All Variables

Variables	Observations	Mean	St. Deviation	Min	Max
Total Deportations	3129	126.74	1100.166	0	35468
Budget per resident	2939	170.236	214.885	1.127	5804.384
IGSA Contract	3129	0.087	0.282	0	1
Private Correctional Facility	3129	0.033	0.179	0	1
Republican support	3129	58.28	14.02	6.89	94.25
287(g) Participant	3129	0.217	0.412	0	1
% Favors SB1070	2703	56.67	11.73	25	73
Dream Act	3010	0.385	0.486	0	1
Border State	3129	0.139	0.346	0	1
County Population	3129	98852	339707	82	9818605
Foreign-Born	3129	4.32	5.49	0	72.2
Latino Population	3129	8.25	13.17	0	95.74
Change in Latino Population	3125	85.40	93.31	-100	1741
% State Unauthorized	3010	2.85	1.81	0.5	7.2
Crime Rate	2701	123.23	397.75	0	15370.7
% GDP Construction	3129	4.02	0.685	1.1	5.7
% GDP Fruits & Vegetables	2855	3.28	4.03	0.1	18
Unemployment Rate	3128	8.53	2.95	1.1	28.9
Median Household Income	3128	45,965.5	11,598.2	21,572	117,680

The second group of control variables is focused on demographic factors. From the U.S. Census and American Community Survey, I gathered data on the county's population in 2010 and the percentage of Latino residents in 2000 and 2010. To account for trends in existing research, I look at the Latino population and its growth in particular. For example, it could be that Secure Communities unfairly targets Latinos because members of their community are more often depicted as "illegals" (Cox and Miles 2013) and that enforcement is more likely to be pursued in areas where the Latino population has grown rapidly (Hopkins 2010; Newman et al. 2012). A dummy variable for whether or not the county is located in a border state (states that typically have large Latino and large unauthorized populations) was also created.

Finally, I include a number of economic variables often linked to immigration and public attitudes. Some local governments justify harsher measures against immigrants because they are perceived as an economic threat (Meuleman, Davidov, and Billiet 2009). To account for this, I include the 2011 unemployment rate from the Bureau of Labor Statistics as well as the state's percentage of GDP coming from construction (U.S. Bureau of Economic Analysis 2014) and fruits and vegetables (U.S. Department of Agriculture 2010), two industries that as Chand and Schreckhise (2014) point out are often reliant on undocumented labor. Additionally, I include the county's median household income (U.S. Census Bureau, American Community Survey 2013) to account for the possibility that because communities with higher incomes are often more concerned about the fiscal burdens that immigration brings (Facchini and Mayda 2009; Hanson, Scheve, and Slaughter 2007), they may be more prone to implementing the Secure Communities mandate.

Replicating Chand & Schreckhise

I have implied that the explanation put forward by Chand and Schreckhise, that more Republican-leaning counties engage in more deportations, is not theoretically sound. In

Table 2.2 I test whether there is statistically support for it. Model 2.1 recreates Chand and Schreckhise's OLS regression model and compares it to Model 2.2,which uses ZINB analysis to test the effect of the same variables on deportation rates.[9]

The OLS results in Model 2.1 match the main effects found in Chand and Shreckhise's original analysis, providing us with confidence that we have adequately recreated their dataset. Some of the relationships tested are consistent in the ZINB model; those that differ significantly are in bold. Notably, the coefficient for Republican support retains its significance, but is *negatively* related to deportations when using ZINB regression. And, while Republican support is negatively related to the odds of being in the "0" deportations group, it is statistically insignificant. This further confirms that the negative relationship in the count model is not simply a product of Republican support being a strong indicator at the zero-inflation stage of the model. The border state, unemployment, and crime variables also change direction, while the presence of a Dream Act continues to be inversely related to deportations, but loses significance. The bottom of the table reports the AIC, showing ZINB to be a vast improvement over OLS. Additionally, Breusch-Pagan and Shapiro-Wilk tests demonstrate, respectively, that the

[9] Each of these variables was obtained from the same sources used by Chand & Schreckhise except for the construction variable, which I could not find from the source they provided in the references section. I obtained this figure, instead, from the U.S. Bureau of Economic Analysis and checked the accuracy of the figures using various statistics from the Department of Labor's website (where Chand & Schreckhise said to have acquired it). All variables are specified according to the explanation provided by Chand and Schreckhise. However, in the ZINB models, *Days Active* is used as an exposure variable.

residuals are highly heteroskedastic and abnormal.[10] Given the negative relationship between Republican support and deportation rates, it is only fitting that we turn to alternative explanations to determine what exactly is responsible for compliance in this case.

Table 2.2. Deportations from Secure Communities, U.S. Counties 2008 through May 31, 2013

	C&S OLS Model 2.1	C&S ZINB Model 2.2
Independent Variables		
Favors SB1070	5.653**(1.90)	0.0242***(0.005)
Dream Act	**-156.638***(35.24)**	**-0.0861(0.091)**
Republican support	**4.415***(1.111)**	**-0.012***(0.003)**
Border State	**264.196***(58.53)**	**-0.154(0.139)**
Latino Population	2.629*(1.275)	0.031***(0.003)
State % Unauthorized	**25.251(13.48)**	0.316***(0.038)
GDP Fruits & Vegetables	19.095(37.907)	0.156(0.109)
GDP Construction	54.956*(23.574)	0.227***(0.065)
Unemployment Rate	8.969(5.288)	**-0.0139*(0.015)**
County Population	0.003***(0.000)	3.79e-06***(2.46e-07)
Crime	**-0.029***(0.003)**	**5.92e-05***(1.31-e05)**
287(g) Participant	31.986(35.316)	0.099(0.092)
Days Active	-0.047(0.0478)	
Constant	-1055.54***(173.966)	-6.955***(0.477)
Inflated Model		
Favors SB1070		0.005(0.022)
Dream Act		0.655(0.365)
Republican support		-0.009(0.011)
Border State		0.211(0.662)
Latino Population		-0.672***(0.174)
State % Unauthorized		-0.158(0.160)
GDP Fruits & Vegetables		-0.213(0.360)
GDP Construction		0.481*(0.217)
Unemployment Rate		0.148**(0.045)
County Population		-1.19e-04***(2.31e-05)
Crime		-0.0026(0.002)
287(g) Participant		-0.331(0.345)

[10] With results from the Breusch-Pagan test reporting chi2(1)= 90151.47 and Prob>chi2=0.000 and results from the Shapiro-Wilk test reporting z=17.443 and Prob>z=0.000.

Constant		1.168(1.784)
Observations	2396	2396
AIC	15.742	6.363

Standard errors in parentheses
[*] $p < 0.05$, [**] $p < 0.01$, [***] $p < 0.001$

Testing the Impact of Resources & Incentives

The hypotheses put forward in this paper suggest that financial resources and incentives may be more effective than ideology to understand jurisdictional variation in deportations. In this section, I examine the total number of deportations from all counties that were active during the tenure of Secure Communities. Due to the broad spread of county-level deportations (with 821 counties reporting 0 deportations and Los Angeles County reporting more than 35,000), I run these models again dropping observations one and two standard deviations above the mean. These models, shown in the appendix, provide robustness checks to confirm that the results found in Table 2.3 are not simply due to the presence of a few counties with extreme deportation counts.[11] To test hypotheses one and two, Model 3.1 of Table 2.3 adds the Private Correctional Facility, IGSA Contract, and Law Enforcement Budget variables to the model specified by Chand and Shreckhise. Model 3.2 varies slightly from Model 3.1: it replaces the Latino population variable with the percentage of foreign-born residents and adds a measure capturing the percent change in each county's Latino population as well as the median

[11] In addition to the reduced models, I provide two other robustness checks in the appendix. First, I created a transformed model, where I use the log of deportations (with counties reporting 0 deportations maintaining their 0 count) as the dependent variable and analyze the model with OLS. Second, I ran a series of diagnostic tests to determine if influential points are skewing the results using Cook's distance. These various re-specifications had no substantive impact on the results.

household income of the county. I discuss the implications of these modifications below. Each model is run with an exposure variable capturing the number of years a county was active in the Secure Communities program in order to account for differences resulting from longer activation times. The results report the coefficients with robust standard errors in parentheses.[12]

Table 2.3. Total Deportations from Secure Communities, U.S. Counties 2008 to 2014

	3.1	**3.2**
Independent Variables		
Private Correctional Facility	0.604*(0.274)	0.588*(0.281)
IGSA Contract	0.616***(0.118)	0.712***(0.131)
Budget per resident	0.073*** (0.018)	0.063***(0.016)
Republican support	-0.008(0.005)	-0.014**(0.005)
Foreign-born Population		0.160***(0.017)
Latino Population	0.043***(0.007)	
Crime	0.00003(0.00002)	0.00001(0.00002)
% Change in Latino Population		0.003***(0.0008)
County Population (centered)	0.004***(0.0005)	0.0024***(0.0006)
GDP Fruits & Vegetables	-0.008(0.122)	-0.179(0.117)
GDP Construction	0.067(0.099)	0.250**(0.086)
Unemployment Rate	-0.044*(0.018)	0.018*(0.024)
Median Household Income (centered)		0.001(0.0006)
287(g) Participant	0.024(0.097)	0.042(0.092)
Border State	0.025(0.189)	0.436**(0.162)
Dream Act	-0.284**(0.095)	-0.085(0.099)
Favors SB1070	0.017*(0.008)	0.009(0.007)
State % Unauthorized	0.304***(0.044)	0.143**(0.046)
Constant	-0.413(0.566)	-0.986*(0.586)
Inflated Model		
Private Correctional Facility	-0.954(1.848)	-0.168(0.974)
IGSA Contract	-0.389(0.514)	-0.462(0.533)
Law Enforcement Budget	-0.149*(0.070)	-0.261*(0.115)
Republican support	0.0004(0.013)	-0.013 (0.013)
Foreign-born Population		-0.472***(0.116)
Latino Population	-0.880***(0.249)	
Crime	-0.0006(0.001)	-0.001(0.001)
% Change in Latino Population		-0.001(0.0007)
County Population (centered)	-0.149***(0.027)	-0.164***(0.032)

[12] Although King and Roberts (2015) have noted that robust standard errors may hide bias, running the models with classical standard errors results in no substantive differences and changes the standard errors negligibly. These results and further discussion can be found in the appendix.

Fruits & Vegetables GDP	0.312(0.381)	0.365(0.399)
Construction GDP	0.435(0.245)	0.564*(0.250)
Unemployment Rate	0.179***(0.056)	0.132(0.072)
Median Household Income (centered)		-0.007**(0.002)
287(g) Participant	-0.491(0.395)	-0.429(0.372)
Border State	-1.080(1.313)	-1.477*(0.674)
Dream Act	0.7524(0.431)	1.155**(0.409)
Percent Favors SB1070	0.011(0.031)	-0.053*(0.027)
State Percent Unauthorized	-0.342(0.186)	-0.815***(0.183)
Constant	-14.711***(3.630)	-11.624**(4.267)
Observations	2271	2271
AIC	7.191	7.077
BIC	16520.1	16283.7
Vuong test vs. NBRM	z=9.20, Pr>z=0.000	z=8.70, Pr>z=0.000

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The Resource Constraints and Financial Incentives hypotheses are both supported by the models: the size of the county's law enforcement budget, private correctional facilities, and IGSA contracts are all positively and significantly related to the number of jurisdictional deportations. Figure 2.2 illustrates quite clearly that in the case of Secure Communities it is financial resources – not political ideology – that has a substantive effect on deportations. Additionally, it suggests that while deportations are certainly more common in areas with larger Latino populations, the crime rate is not necessarily a factor motivating such an outcome. This indicates, as Cox and Miles (2014) and Chand and Schreckhise (2014) have suggested, that the implementation of S-Comm is not completely reflective of its stated objective to target *criminal aliens*.

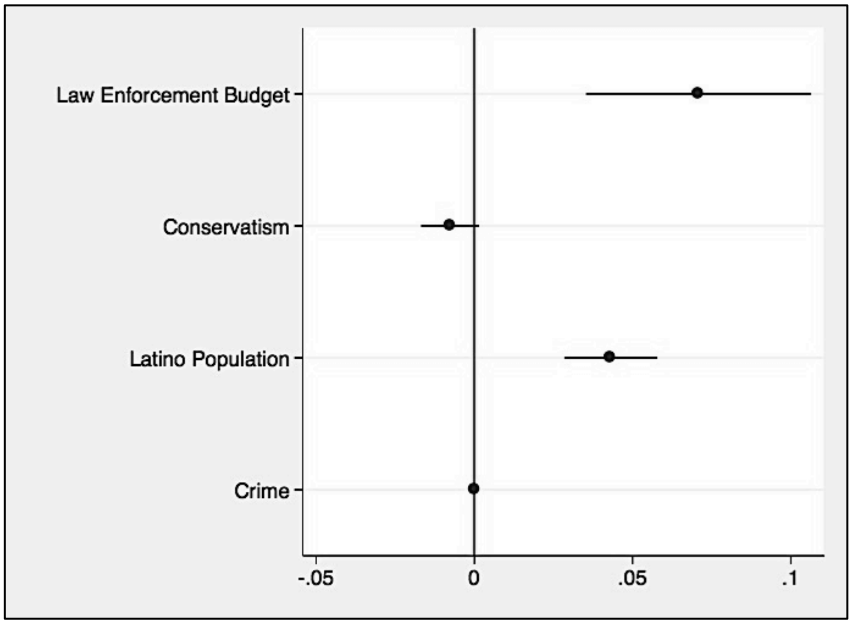
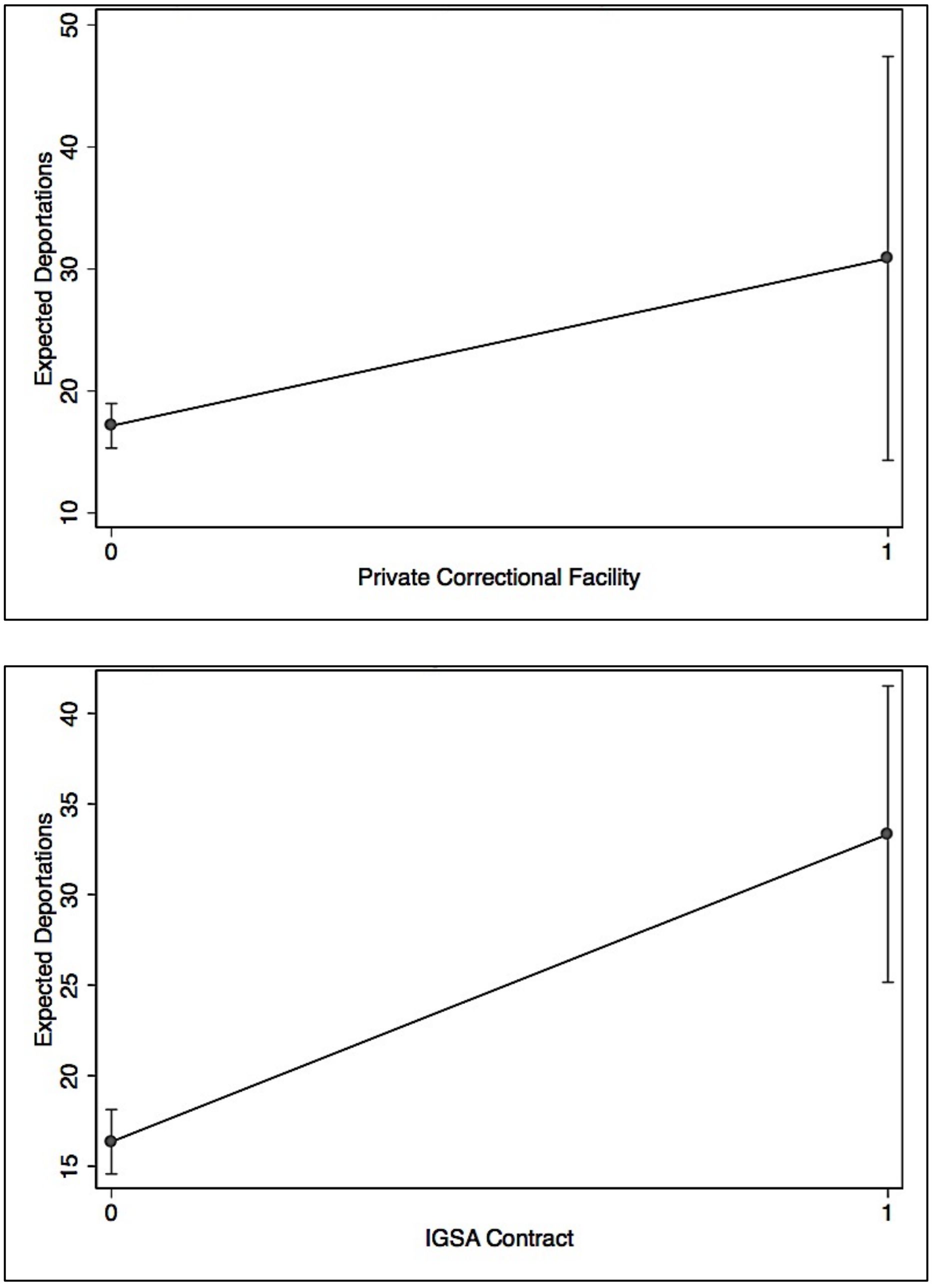


Figure 2.2. Coefficients of Key Variables. Each point shows the coefficient of the corresponding variable from Model 3.1 with a 95% confidence interval. The graph demonstrates considerable support to the proposition that the Resource Constraints hypothesis is a preferable alternative to assessing policy outcomes in terms of political preferences.

Moreover, in the inflated components of Models 3.1 and 3.2, county's with larger law enforcement budgets are significantly more likely to members of the deportations group while, once again, high levels of Republican support are no more or less likely to shift a county from 0 deportations to at least 1 deportation. On the other hand, the coefficients for the other predictors of conservative attitudes toward immigrants/immigration do seem to be in the expected direction. In particular, counties in states that have implemented their own versions of a Dream Act are less likely to deport and apt to deport fewer individuals on average. Contrastingly, counties that are active in the 287(g) program or that are located in states with voters that support Arizona's SB1070 are more likely to deport. Still, the results from the negative binomial model make it unclear if support for these policies also leads to *more* deportations. Consequently, these findings make it no easier to conclude that political preferences alone help us to understand jurisdictional variation in deportation rates. In the next section, I explore the possibility that political inclinations do matter, but that their importance depends on the financial resources available to law enforcement

Like existing resources, the promise of future resources is also positively related to a county yielding more deportations. The presence of a private correctional facility increases the average number of deportations from 17 to 31. Similarly, counties with an IGSA contract have more than twice as many deportations, on average, than counties with no contract. Thus, all else equal, moving to a county with either a private facility or an IGSA contract increases the number of deportations that occur by a factor of 1.8 and 2.1, respectively. Figures 2.3a and 2.3b depict visually the effect of these institutions on deportation rates. As we can see, IGSA contracts have a much stronger and consistent impact on deportation rates. While both counties with an IGSA contract and those with a private prison produce rather wide confidence intervals for the number of deportations yielded, the smallest number of expected deportations in IGSA contracted counties is still far greater than the expected number of deportations in counties without.



Figures 2.3a and 2.3b. Effect of Private Prisons (2.3a) and IGSA Contracts (2.3b) on Deportation Rates. Each graph shows the change in the expected number of deportations (with 95% confidence intervals) when moving from a county without to a county with a private prison or IGSA contract.

Given that for deportations to take place there must be a potentially deportable population, it is not surprising that a county being located in a border state or a county with a larger Latino or unauthorized population matters. However, replacing the Latino population variable with the county's foreign-born population is perhaps a better specification. Model 3.2 in Table 2.3 illustrates this, revealing significantly lower BIC and AIC values than Model 3.1. As the Latino and foreign-born population variables are highly correlated (r=0.71), Model 3.2 captures the variation in county deportations that the Latino population variable is explaining in Model 3.1, but avoids problems of collinearity between the Latino population with the border state and unauthorized population variables.[13] Additionally, because research showing that sudden increases in the Latino population are associated with more negative attitudes toward immigration (Hopkins 2010; Newman et al. 2012), including a variable that captures the change in the Latino population in Model 3.2 continues to account for the possibility that it is the Latino population *in particular* that is increasing the probability of deportation.

The economic variables used by Chand and Schreckhise do a good job of accounting for the possibility that immigrants are viewed as economic competition in certain communities and, thus, more likely to be targeted via programs like Secure Communities. However, other studies conclude that the fiscal burdens associated with immigration strengthen exclusionary attitudes amongst individuals with higher incomes (Facchini and Mayda 2009; Hanson et al. 2007). Thus, by adding the median household income to Model 3.2, I hope to obtain a clearer picture of how economic considerations

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[13] In the appendix, I discuss this and other issues of collinearity in the models that could be affecting the results.

affect compliance more generally. While the median household income does not help us to understand which counties are likely to produce the most deportations, it does tell us that counties with higher median incomes are substantially less likely to be in the zero deportations group (our outcome being tested in the inflated model). Moreover, if the relationship between income and deportations is, as the literature suggests, a reflection of concerns about immigration and financial stress, then finding that deportations are more likely to occur in wealthier counties reinforces that compliance with Secure Communities is largely tied to considerations about resource availability.

Do Resources Moderate the Effect of Republican Support on Deportations?

This section tests whether levels of Republican support in a county have any role to play in predicting deportation outcomes. If the political orientations hypothesis is correct, then we should see that Republican-leaning counties engage in more deportations when they have larger levels of existing resources (i.e. large policing budgets) and when they possess more financial incentives to do so because of an IGSA contract or their hosting of a private prison. These interactions are tested, respectively, in Table 2.4 below.

Model 4.1 centers and interacts the Republican support and Law Enforcement Budget variables so that we can better estimate the main and interactive effects of both variables. We find that a larger law enforcement budget does alter the relationship between Republican support and deportations, indicating that as the size of a county's law enforcement budget increases a more conservative county will produce more deportations. The inflated component of this model also lends credibility to the moderating effect of budget size: Republican-leaning counties are les likely to be in the "0" deportations group as the size of their law enforcement budget increases.

Table 2.4. Moderating Effect of Resources on Republican Support and Deportation Outcome

	4.1: Budget	**4.2: IGSA**	**4.3: Private Facility**
Independent Variables			
Private Correctional Facility	0.534(0.280)	0.618^{*}(0.282)	0.571(0.295)
IGSA Contract	0.723^{***}(0.131)	0.803^{***}(0.137)	0.711^{***}(0.130)
Law Enforcement Budget (centered)	0.050^{**} (0.018)	0.0643^{***}(0.0158)	0.0650^{***}(0.0163)
Republican support (centered)	-0.015^{**}(0.005)	-0.0164^{**}(0.00536)	-0.0139^{*}(0.00542)
Budget X Republican support	0.0034^{**}(0.001)		
IGSA X Republican support		0.0247^{*}(0.0106)	
Private Facility X Republican support			-0.00578(0.0166)
Foreign-born Population	0.161^{***}(0.017)	0.159^{***}(0.0166)	0.160^{***}(0.0168)
Crime	0.000009(0.00001)	0.00330^{***}(0.000825)	0.00337^{***}(0.000807)
% Change in Latino Population	0.003^{***}(0.0008)	0.0000159(0.0000183)	0.0000134(0.0000182)
County Population (centered)	0.002^{***}(0.0005)	0.00241^{***}(0.000598)	0.00241^{***}(0.000613)
GDP Fruits & Vegetables	-0.170(0.117)	-0.235^{*}(0.118)	-0.176(0.118)
GDP Construction	0.239^{***}(0.086)	0.252^{**}(0.0843)	0.246^{**}(0.0861)
Unemployment Rate	0.019(0.024)	0.0192(0.0241)	0.0164(0.0243)
Median Household Income (centered)	0.001(0.0006)	0.00121(0.000635)	0.00118(0.000640)
287(g) Participant	0.039(0.091)	0.0385(0.0920)	0.0451(0.0913)
Border State	0.454^{**}(0.163)	0.425^{**}(0.161)	0.437^{**}(0.161)
Dream Act	-0.097(0.100)	-0.0780(0.0993)	-0.0829(0.0999)
Favors SB1070	0.011(0.008)	0.00939(0.00756)	0.00951(0.00755)
State % Unauthorized	0.153^{**}(0.047)	0.145^{**}(0.0467)	0.145^{**}(0.0459)
Constant	-1.759^{**}(0.642)	-1.814^{**}(0.630)	-1.793^{**}(0.641)
Inflated Model			
Private Correctional Facility	-0.018(0.904)	-0.153(1.047)	-0.145(0.839)
IGSA Contract	-0.400(0.535)	-0.386(0.555)	-0.461(0.509)
Law Enforcement Budget (centered)	-0.238^{*}(0.106)	-0.259^{*}(0.117)	-0.263^{*}(0.115)
Republican support (centered)	-0.018(0.014)	-0.0127(0.0136)	-0.0155(0.0135)

Budget X Republican support	-0.018*(0.007)		
IGSA X Republican support		-0.0657(0.0495)	
Private Facility X Republican support			0.0433(0.0488)
Foreign-born Population	-0.487***(0.114)	-0.479***(0.116)	-0.472***(0.115)
Crime	-0.0008(0.002)	-0.00129(0.000734)	-0.00125(0.000763)
% Change in Latino Population	-0.001(0.0007)	-0.00128(0.00153)	-0.00132(0.00150)
County Population (centered)	-0.167***(0.034)	-0.165***(0.0321)	-0.167***(0.0329)
Fruits & Vegetables GDP	0.459(0.408)	0.326(0.407)	0.385(0.404)
Construction GDP	0.499*(0.245)	0.559*(0.254)	0.556*(0.246)
Unemployment Rate	0.115(0.077)	0.130(0.0720)	0.130(0.0724)
Median Household Income (centered)	-0.007**(0.002)	-0.00675**(0.00229)	-0.00678**(0.00232)
287(g) Participant	-0.487(0.367)	-0.426(0.378)	-0.394(0.374)
Border State	-1.411*(0.637)	-1.493*(0.687)	-1.414*(0.639)
Dream Act	1.022**(0.408)	1.143**(0.413)	1.188**(0.412)
Percent Favors SB1070	-0.050(0.029)	-0.0521(0.0277)	-0.0540*(0.0268)
State Percent Unauthorized	-0.786***(0.190)	-0.808***(0.185)	-0.824***(0.184)
Constant	-12.88**(4.301)	-12.47**(4.073)	-12.50**(4.062)
Observations	2271	2271	2271
AIC	7.073	7.078	7.078
BIC	16287.1	16298.15	16298.15
Vuong test vs. NBRM	z=8.67, Pr>z=0.000	z=8.72, Pr>z=0.000	z=8.72, Pr>z=0.000

Robust standard errors in parentheses
$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

To get a better idea of what the effect of resources on this relationship actually is, Figure 2.4 looks at the marginal effect of Republican support on deportations for an average county at the 25th, 50th, and 75th percentiles of law enforcement budget size. This Figure clarifies what the interaction between these variables implies for deportation rates. Interestingly, the marginal effect of Republican support results in fewer deportations for an average county with a budget in the 25th or 50th percentiles (keeping all other variables at their means). In fact, Republican counties with smaller budgets are expected to produce about 10 fewer deportations than their Democratic counterparts. By contrast, counties with budgets in 75th percentile experience significantly more deportations as the percentage of voters who cast their ballots for the Republican presidential candidate increases. This supports the Political Orientations hypothesis that until a certain level of financial security is reached, political orientations play a negligible role in explaining compliance. This means that even if more conservative counties want to assist ICE officials to a greater extent, they may not be able to do so until they have the necessary financial resources.

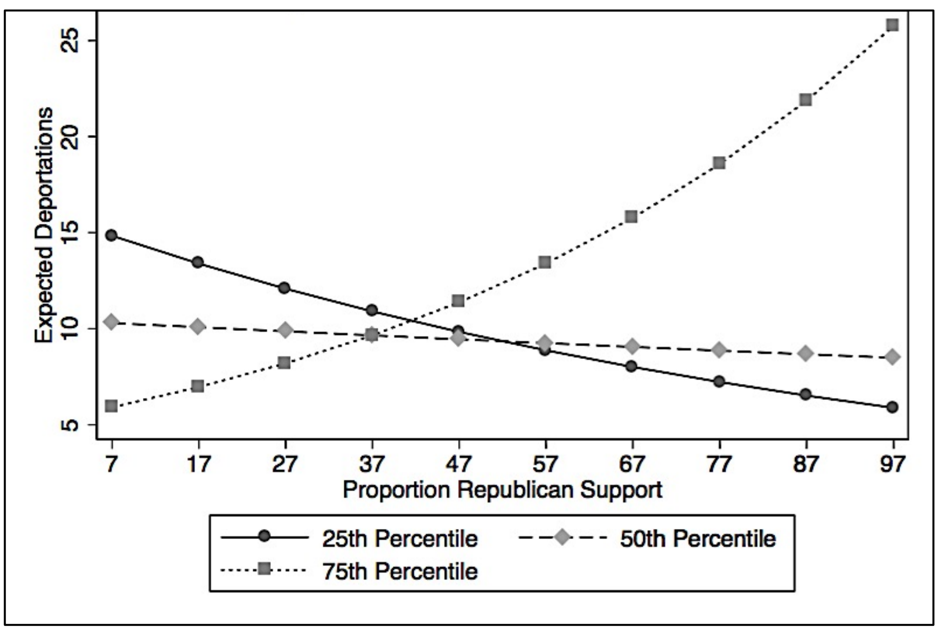

Figure 2.4. Interactive Effect of Law Enforcement Budget and County Levels of Republican Presidential Support on Deportations. This graph demonstrates how the marginal effect of a conservative constituency on the number of deportations reported in county changes depending on the size of the law enforcement budget (all other variables being held at their means). The graph plots county budgets at the 25th, 50th, and 75th percentiles.

Model 4.2 reports a similar finding between Republican counties and IGSA contracts. As shown in Figure 2.5, we find that moving from a county where 7 percent of voters supported the Republican candidate in the 2008 and 2012 elections to a county where 97 percent of voters supported that candidate more than doubles the number of deportations from 24 to 51 for those counties that have IGSA contracts. In counties without, this shift in support drops the number of deportations from 38 deportations to just 9.

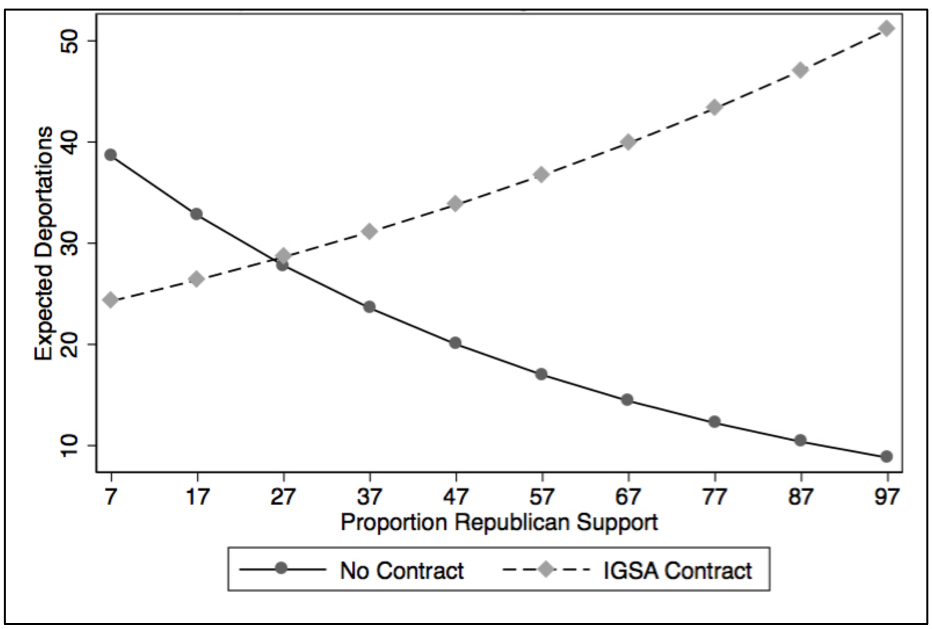

Figure 2.5. Interactive Effect of IGSA Contracts and County Levels of Republican Presidential Support on Deportations. This graph demonstrates how the marginal effect of a Republican-leaning jurisdiction on the number of deportations reported in county changes depending on whether or not that county has an IGSA contract (all other variables being held at their means).

Although we do not find any meaningful relationship between levels of Republican support and the presence of a private prison in Model 4.3, the results from

Models 4.1 and 4.2 are more than sufficient to demonstrate support for the political orientations hypothesis: only once a county has secured resources do political preferences influence compliance.

Discussion

This work contributes to existing theories of intergovernmental policy implementation by showing a need to rethink the relationship between resources, ideology, and local cooperation. Instead of insisting that resource constraints and political culture represent two unique systems for how we might understand federal-local cooperation, this paper shows that they contribute to our understanding of implementation both discretely *and* jointly. The findings suggest that the extent to which local actors cooperate with federal initiatives is largely dependent on resource considerations, first, and political reasons, second. The bottom-line being that in cases where implementation is not so fiscally tasking variation in how localities comply may indeed be along ideological lines, but when implementation is associated with a particularly large resource burden, it may be more difficult for actors to comply with mandates even if they agree with its objective.

A discernible area for further testing of this theory is environmental mandates. In addition to the ideological divisions connected with environmental regulations (Eisner et al. 2000; Jones and Dunlap 1992), these federal directives have imposed a tremendous financial burden on local governments with scarce resources (Laufenberg 1998; Pollans 2015; Weiland 1998). Indeed, in response to the release of Columbus, Ohio's 1991 publication expressing their inability to meet the growing costs accompanying federal environmental legislation, the *New York Times* claimed that "environmental costs were about to swamp Columbus in red ink – or generate a revolt" (Schneider 1992). It seems quite likely that discrepancies in the implementation of federal environmental regulations are linked to a local government's resource capacity, but a prudent path for future research would be to consider if capacity also moderates the association between ideological preferences and compliance with environmental regulations.

The case study of Secure Communities also suggests that fiscal incentives may motivate compliance from actors ordinarily reluctant to implement such policies. Counties with IGSA contracts and private correctional facilities report significantly more deportations than those without. It makes sense that if cooperation is associated with financial opportunity, counties would be more likely to assist ICE with its deportation efforts while trying to improve or prevent their own financial problems. In fact, such an incentive structure is remarkably similar to federal laws that allow police departments to keep a generous portion of assets they seize during drug arrests in order to improve anti-drug enforcement at the local level. As studies show, this incentivized approach to garnering local cooperation not only induces the assistance of LEAs, but also gains support from local governments who use seizures to justify police budget reductions in order to ease their own financial stress (Baicker and Jacobson 2007).

In sum, discrepancies in local cooperation with federal programs may be due more to the fact that some jurisdictions have a larger resource capacity than others or because certain localities are persuaded to collaborate when financial returns are linked to such compliance. This means that even if local actors are hesitant to *adopt* particular types of policies due to political orientations they may not be deterred from assisting with the *implementation* of similar federal policies. These findings have strong implications for considering when and how financial incentives are used as a tool to solve compliancy issues in federal-local partnerships more generally. Furthermore, since detainer requests, the use of IGSAs, and the use of private prisons by ICE are not unique to the agency's Secure Communities program, we can apply the approach taken here to better understand the implementation and outcomes of other immigration enforcement programs.

CHAPTER THREE

Salient Sub-constituencies, Partisan Pressure, and Legislative Responsiveness

Early studies of public influence on state legislative behavior painted a dismal picture of responsiveness. Not only did there appear to be no link between public opinion and policy output (Dye 1966; Plotnick and Winters 1985), the accepted rationale for this disconnect was that voters were uninformed and apathetic (Treadway 1985). Recent research, however, is more optimistic. After Erikson, Wright, and McIver's (1993) foundational study demonstrated that states in fact adopt policy regimes that align with the ideology of their citizens, research has repeatedly shown a strong relationship between public opinion and state policy on highly salient issues (Brace et al 2002; Lax and Phillips 2009; Lupia et al. 2010). Nevertheless, it is unclear whether legislators are also responsive to their constituents when issues are less publically visible. Although some speculate that legislators try their best to estimate public opinion and act accordingly (Uslaner and Weber 1979), most conclude that without a strong position from the general public, legislators shirk constituency preferences and succumb to party-based influences (Jenkins 2010; Kirkland and Harden 2016; Lax and Phillips 2012; LeBlanc 1969; Patterson 1996). While both of these accounts are reasonable, I contend that existing scholarship overlooks an alternative explanation: when it comes to less salient policy issues, legislators respond to the sub-constituency preferences for which that policy matters.

Building on Fenno's (1978) insights that members of congress are influenced in distinct ways by various groupings of constituents, I argue that extant studies focus too much on the relationship between aggregate opinion and legislative responsiveness when an issue need only be salient amongst a particular sub-constituency to influence legislative output. In doing so, I contribute to the bourgeoning work being done on state legislative behavior in two ways. First, I depart from existing studies that rely exclusively on the state legislature as their unit of analysis and instead examine the determinants of both state implementation of public policy *and* legislator voting on it. The state-level analysis allows us to compare our theoretical findings to similar studies of legislative behavior while the legislator-level extends our testing of theory into a context-dependent setting by matching representatives with the unique characteristics of their districts. The consequence of this combination is that we are able to parcel out meaningful differences between the motivations behind a legislative body's responsiveness versus that of an individual legislator. Second, while the potential for sub-constituencies to affect legislative behavior has been considered at the national-level (Bishin 2000; Clinton 2006; Fenno 1978; Hayes and Bishin 2012; Shapiro et al. 1990), state-level analyses have virtually ignored this relationship; instead, limiting their focus to whether state policy reflects public opinion in the aggregate. Since the federalist system arguably places state governments in a better position than national government to tailor their policies to the preferences of their constituents, exploring the relationship between legislative responsiveness and sub-constituencies is a natural and overdue line of inquiry.

To demonstrate the importance of sub-constituencies for understanding legislative responsiveness, I look specifically at the motivations behind state adopted E-Verify laws. E-Verify is an apt case for showing the essential role that sub-constituencies can play in the legislative process for two reasons. First, E-Verify has received little attention from the general public, putting it neatly into the category of low-profile legislation. Yet, while E-Verify yields weak preferences *in general*, it is especially salient to two groups of constituents who are directly impacted by its enforcement: the agribusiness and foreign-born communities. Second, whereas other research has struggled to disentangle the effects of constituency and party-based pressures on legislative behavior (Battista and Richman 2011; Clinton 2006; Lax and Phillips 2012), E-Verify is an issue where party positions and the preferences of salient constituencies are at odds. This conflict makes it possible to separate the relative effect of sub-constituencies from party-based influences on legislative responsiveness.

Using data on state adoption of E-Verify laws since 2006 and legislator roll call voting on these measures, I find that state legislatures and legislators themselves are highly responsive to sub-constituency preferences, but that responsiveness plays out in different ways depending on the level of analysis. At the state-level, E-Verify is kept off the agenda in states where pressure from the agribusiness and foreign-born populations is strongest while such legislation passes in states where the voting power of these sub-constituencies is considerably weaker. This effect occurs regardless of the legislature’s partisan composition. At the individual-level, legislator support for E-Verify is more clearly divided along partisan lines, but even here we find that sub-constituency pressures are capable of altering both Republican and Democratic positions. These findings call into question existing beliefs that legislators shirk their responsibility to their constituents when a policy issue lacks public visibility. Instead, they suggest that without intense aggregate preferences to guide their actions, legislators become particularly sensitive to those constituents for whom a policy matters most and act accordingly.

Responsiveness & Issue Salience

Some level of legislative responsiveness is key to achieving a democracy. Yet, it was not until the early 1990s, when Erickson, Wright, and McIver (1993) demonstrated that state policies reflect the general ideology of their citizens, that a connection between public preferences and state policy direction was firmly established. Gray et al. (2004) replicate and update this model by including measures of organized interests, but still find that public opinion is the strongest predictor of policy direction. Jacoby and Schneider (2001) further show that public opinion not only influences policy direction, but also state policy priorities. Other studies confirm a direct link between public attitudes and the adoption of *specific* policies, such as gay rights, the death penalty, and abortion policy (Arceneaux 2002; Brace et al. 2002; Gerber 1996; Haider-Markel and Kaufman 2006; Lax and Phillips 2009; Norrander 2000).

Although research has taken significant steps in terms of extending Erickson, Wright, and McIver's findings to specific attitudes and subsequent policy adoption, these improvements are not without their own limitations. Thus far any connection between public opinion and state policy has been limited to those issues that are especially salient to the general public. For example, Gerber (1996) finds that members of California's legislature voted in accordance with their district when legislation was highly salient, while those representing districts where the same issues were deemed insignificant to constituents tended to vote against their majority's preferences. And, Lax and Phillips (2009) conclude simply and emphatically that "higher salience means greater responsiveness."

But what about policy issues that lack general salience? Do constituency pressures simply disappear and, if so, what guides legislative behavior? Extant studies have basically ignored this question, at best speculating that as issue salience diminishes party-based pressures takeover to drive legislative behavior (Jenkins 2010; Kirkland and Harden 2016; Lax and Phillips 2012; LeBlanc 1969; Patterson 1996). However, this explanation is rarely foolproof. It could be that what is interpreted as party pressure is actually legislators acting on their personal preferences (Krehbiel 1993), legislative rules making party influence easier (Jenkins 2010), or, as Kingdon (1968) argues, that party pressure "masks an underlying constituency influence." Lax and Phillips (2012) suggest that if it is not party driving policy direction, it could be that legislators "overshoot" their assumptions about where majority preferences lie on a particular issue. Subsequently, states adopt conservative/liberal policy responses in more conservative/liberal states despite majority preferences being more liberal/conservative on that particular issue. However, Lax and Phillips concede that they have no way to discern whether legislators are in fact acting based on their expectations of public opinion or if they are appealing to their ideological base, personal preferences, or party pressure. To address this weakness in the literature and further develop our understanding of legislative responsiveness at the state-level, I turn to the rich and extensive literature on legislative behavior at the national-level.

Studies of Congress also find a heightened role for party-pressure in contexts where representatives are faced with low-salience policy (Clinton 2006; Lee 2013; RePass 1971), however, this body of scholarship points to an additional source to explain legislative behavior: sub-constituencies. Fenno's (1978) study of how representatives view their constituents revealed that the larger geographical constituency is split into multiple groups that serve different functions in terms of helping the representative meet their goals: namely, reelection. Indeed, levels of policy responsiveness and congruence are much stronger when we look at the relationship between legislators and sub-constituency groups than legislators and their constituencies as a whole (Bishin 2009; Bullock and Brady 1983; Powell 1982). Arnold (1990) clarifies this further explaining that representatives sort their constituents into issue-based sub-constituencies based on their perceived preferences and opinion intensity. This preference mapping is done in order to guide legislators in their decision-making so they can avoid isolating sub-constituencies and protect their reelection prospects.

In this way, issue salience once again becomes vital to explaining responsiveness. The main difference is that in this case an issue need only be salient amongst a particular group rather than the constituency at large. For example, Hayes and Bishin (2012) discover that legislators are not any more likely to respond to the general preferences of their constituents as a low-profile issue becomes more visible, but that they do improve their responsiveness to the preferences of those sub-constituencies for which the issue is especially salient. This is because groups with strong preferences on niche issues are more likely to be aware of their legislator's voting record on those issues, punishing or rewarding their representative accordingly (Arnold 1990; Bishin 2009). I use this theoretical foundation to test the possibility that taking into account salient sub-constituencies will also help us to better understand legislative responsiveness at the state-level. I do this by looking at the motivations behind state adopted E-Verify laws.

E-Verify in the States

E-Verify, a system that mandates employers electronically verify the legal status of prospective employees using federal databases, is a fitting case for showing the important role that sub-constituencies can play in the legislative process. To date, nearly every state legislature has introduced some form of E-Verify legislation[14], but only 23 states have adopted mandatory policies, as summarized in Table 3.1. Importantly, variation in adoption has not been tied to the partisan composition of the legislatures or the ideological disposition of their populations (Newman et al. 2012; Zingher 2014). This is in stark contrast to other state-level immigration policies, which have been established under distinctively partisan circumstances (Creek and Yoder 2012; Monogan 2013; Wong 2012).

Table 3.1. State E-Verify Laws, 2006-2015

State	Bill	Year	Employers Targeted

[14] Only 4 states have not: Alaska, Maine, North Dakota, and Vermont.

Alabama	H 56	2011	All employers
Alabama	H 658	2012	All contractors, subcontractors (prime contractors not liable)
Arizona	H 2779	2007	All employers
Arizona	H 2745	2008	All contractors, subcontractors (excluding independent contractors)
Colorado	H 1343	2006	State agencies, contractors
Florida	EO 11-02	2011	State agencies, contractors, subcontractors
Georgia	S 529	2006	Public employers, contractors, subcontractors with 500+ employees
Georgia	H 87	2011	Private employers with more than 10 employees
Idaho	EO 09-10	2009	State agencies; public contractors and subcontractors
Indiana	S 590	2011	State/local agencies and contractors
Indiana	H 1019	2015	Public works contractors
Louisiana	H 342	2011	State contractors
Louisiana	H 646	2011	Private employers
Michigan	H 5365	2012	Contractors, subcontractors of the Department of Human Services and Department of Transportation
Minnesota	EO 08-01	2008	Public employers; contractors in excess of $50,000 (expired)
Minnesota	SF 12	2011	Contracts in excess of $50,000; vendors and subcontractors
Mississippi	S 2988	2008	All employers, contractors, and subcontractors
Missouri	H 1549	2008	Public employers, contractors, subcontractors
Nebraska	L 403	2009	Public employers, contractors, and businesses qualifying for state tax incentives
North Carolina	S 1523	2006	Public employers
North Carolina	H 36	2011	All employers, counties, and municipalities
North Carolina	H 786	2013	All contractors and subcontractors (excluding employment less than 9 months)
Oklahoma	H 1804	2007	Public employers, contractors, subcontractors
Pennsylvania	S 637	2012	Public works contractors and subcontractors
Rhode Island	EO 11-02	2008	Public employers, contractors, subcontractors (rescinded)
South Carolina	S 20	2011	All employers
Tennessee	H 1378	2011	All employers
Texas	S 374	2015	State agencies
Utah	S 81	2008	Public employers, contractors, subcontractors
Utah	S 251	2010	Private employers with more than 15 employees
Virginia	H 737	2010	State agencies
Virginia	H 1859	2011	Public contractors, subcontractors with 50+ employees
West Virginia	S 659	2012	Public employers and contractors

Zingher (2014) suggests that E-Verify is not as “high profile” as other immigrant-related policies and, subsequently, less likely to evoke strong preferences or become politicized. National opinion polls corroborate this explanation, showing that public preferences on E-Verify are weak and highly contingent upon question phrasing (Ekins 2013). Yet, even if E-Verify triggers weaker public preferences than other immigration enforcement efforts *in general*, it is surely important to particular groups of constituents who are directly impacted by its enforcement – specifically, the agribusiness and immigrant communities. In this way E-Verify presents itself as an excellent test case for measuring the effect of sub-constituencies on legislative behavior: it lacks general public salience, but is of crucial importance to specific groups of voters.

According to Krehbiel’s (1993) standards, E-verify is also an appropriate test case because it enables us to differentiate between constituency effects and party pressure. Republican legislators, typically in favor of stronger immigration enforcement policies, are cross-pressured when it comes to E-Verify since its enforcement means targeting U.S. business owners that rely on immigrant labor. This leaves Republican legislators with a dilemma: continue the refrain of the Republican Party and crackdown on undocumented workers or guard the interests of their farmer constituents? The Democratic Party, on the other hand, has yet to claim an official position on E-Verify, showing more flexibility in its support/opposition. Without looming party pressure, Democratic legislators should be especially reactive to the preferences of any large constituencies with a stake in E-Verify. In the next section, I build on the legislative behavior literature to explain why I expect state legislators to respond to the preferences of agribusiness and the foreign-born community and how this is anticipated to affect our understanding of state variation in E-Verify policy.

Salient Constituencies & Legislative Responsiveness

The national-level work on sub-constituencies has developed a strong theoretical foundation to explain why we should expect legislative behavior to be influenced by the preferences of sub-constituencies (Arnold 1990; Fenno 1978; Fiorina 1974), but it is only recently that scholarship has begun to test how specific groups influence the outcome of specific policies (Bishin 2000; Clinton 2006; Hayes and Bishin 2012). In fact, conflicting findings in the legislative voting literature are thought to derive at least in part from scholars' failing to identify precisely which group of constituents legislators are appealing to when they act (Bishin 2000; Jackson and Kingdon 1992). Like the work of Bishin, Clinton, and Hayes on the House and Senate, I apply the theoretical underpinnings of this early scholarship to the state-level by estimating the impact of the preferences of farm owners and the foreign-born community on state implementation of E-Verify and individual legislator voting on such legislation.

If the behavior of elected officials is motivated by reelection (Mayhew 1974), then constituents can have a profound effect on legislative behavior. Yet, not all constituents in a district are equally as likely to affect a legislator's reelection efforts and, consequently, not all opinions are weighted the same (Arnold 1990; Goff and Grier 1993; Kingdon 1973; Miler 2010). In the context of E-Verify, farm owners and the foreign-born community are exactly the types of sub-constituents that Arnold (1990) and others claim can be effective at appealing to their elected officials: they have a clear agenda that they care deeply about and will hold their legislators accountable on any issue that affects that agenda.

The agricultural industry has expressed its concern that a mandatory E-Verify system would mean relying on the H-2A visa system for meeting immigrant labor needs. According to Cathleen Enright, vice president of governmental affairs at the Western Growers Association, the H2-A program is “unbelievably crushing” for the agricultural industry due to its inability to supply access to a constant and dependable workforce (Enright, as quoted in Caldwell 2011). The dairy industry, for example, requires a skilled *year-round* workforce that cannot be supplied by H-2A *seasonal* workers. Consequently, dairy farmers rely almost entirely on unauthorized immigrants (Wickham 2011).

The President of the National Council of Farmer Cooperatives, Chuck Conner, has called for any E-Verify legislation to include a clear plan for an improved H2-A Visa program as well as a process to adjust the status of existing workers (Conner 2011). This two-pronged approach – making the current guest worker program more efficient and allowing existing unauthorized workers to adjust their status – has become the mantra across agriculture.[15] Indeed, the American Farm Bureau Federation, the nation’s largest farmers’ organization, claims that unless E-Verify also includes a worker program its implementation will “cripple agriculture production in America” (Farm Bureau 2017).

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[15] The Agriculture Workforce Coalition and the United Farm Workers union combined forces to show that the employers and employees of America’s agricultural industry stand together on this issue. AWC includes 70 organizations that represent agricultural employers. (See: “The Agricultural Workforce Coalition” 2017).

The Hispanic Federation (U.S. Congress House Committee on the Judiciary 2013, p. 69) and the Asian American Justice Center (U.S. Congress House Committee on the Judiciary 2013, p. 94) have echoed agriculture's position that a legalization program must accompany E-verify. However, in this case the foreign-born community is worried about E-Verify's potential to disproportionately affect work-eligible immigrants, legal residents, and naturalized citizens. Both groups cite a DHS-commissioned study that found employers pre-screen employees to weed out those who may be more likely to receive a non-confirmation notice and that error rates were 20 times higher for foreign-born workers than U.S.-born workers (Westat 2009, p. XXXV). Thus, it is not surprising that nearly 72 percent of foreign-born Latinos report being worried about themselves, a family member, or close friend being deported and 35 percent of U.S.-born Latinos note the same anxiety (Pew 2008).

If election-minded legislators are in fact responsive to their sub-constituencies, then we should find that states with larger agribusiness and foreign-born communities are less prone to adopting E-Verify:

H1: State legislatures with larger proportions of agricultural and foreign-born constituencies will be less likely to adopt E-Verify.

While we expect that sizable agricultural and foreign-born constituencies will prevent state legislatures from adopting E-Verify, it is also likely that the effect of these constituencies on E-Verify adoption will vary depending on a) the employer targeted by the E-Verify mandate and b) the partisan composition of the legislature. As shown in Table 3.1 above, some state mandates apply to all employers while others are specific to private employers, government/public employers, or contractors. Foreign-born workers are overwhelmingly employed by the private sector (Lewis, Liu, and Edwards 2014) while the interests of farm owners are clearly limited to the private and contractor realms.[16] We would expect that the influence of the agricultural and foreign-born constituencies would be greatest for mandates that apply to private employers and contractors. Thus:

> *H2: The agricultural and foreign-born constituencies should have the greatest effect on limiting passage of private employer and contractor-specific E-Verify mandates.*

We also know that Republicans and Democrats have very different relationships with farm owners and the foreign-born community. Agribusiness is one of the more partisan industries in politics, consistently ranking in the top 10 of the Republican Party's most generous and reliable contributors. For example, in the 2010, 2012, and 2014 election cycles, agribusiness contributed more than $2 billion to political campaigns and party committees – about 70 percent of which went to the Republican Party and its

[16] Until the 1980s most workers were hired directly by farms, but after the Immigration Reform and Control Act's (IRCA) passage reliance on contractors, who were wiling to absorb the risk for immigration violations, became the norm. According to Martin (2011), farms in California alone use contractors to secure about half of their employees overall and approximately 70 percent of their seasonal workers.

candidates (Center for Responsive Politics 2016). Even if a direct link between agriculture contributions and voting does not exist (Alvarez 2005; Vesenka 1989; Welch and Peters 1983), legislators do appear to be profoundly influenced by agricultural interests because of the pressure they feel from this constituency at the polls (Bellemare and Carnes 2015). As a result, it is likely that the size of the agricultural constituency will be especially predictive for how Republicans respond to E-Verify:

> *H3: Republican-controlled legislatures with large agricultural constituencies should be less likely to adopt E-Verify laws than those with smaller agricultural constituencies.*

Contrastingly, I expect Democratic preferences for E-Verify to be tied to the size of both the agricultural and the foreign-born constituencies. Although the agricultural industry might be predominantly in the Republican camp, it makes sense that Democrats would protect their interests if farming plays a vital economic role in their states, especially given that Democrats have yet to display unified support or opposition toward E-Verify. By contrast, we would not expect Republicans, who typically favor E-Verify, to be responsive to the foreign-born constituency when such pandering is likely to reap them few – if any – electoral benefits.

Although the Democratic Party has shown a willingness to support E-Verify, they have also expressed their commitment to safeguarding the positions of existing undocumented workers and preventing E-Verify from unfairly targeting legal foreign-born workers (U.S. Congress House Committee on the Judiciary 2013, p. 74). Furthermore, there is considerable reason to believe that Democratic legislators would be particularly concerned with the interests of their foreign-born constituents. A Pew (2015) survey on party identification reports that Latino and Asian voters favor Democrats over the Republicans by as much as 42 percentage points. Furthermore, Gay (2007) finds that Democrats in California's State Assembly are highly responsive to their minority constituents, many of whom are Latino and Asian – the two ethnoracial minority groups that have been the most vocal about their opposition to E-Verify. Therefore, I predict that:

H4: Democratic-controlled legislatures with large agricultural or foreign-born constituencies should be less likely to adopt E-Verify laws than those with smaller agricultural and foreign-born constituencies.

If large, salient constituencies prevent Republican and Democratic legislatures from enacting E-Verify, do they also influence how individual legislators vote on E-Verify legislation? I argue a qualified "yes". The major difference between the state and individual levels of analysis is that legislators within and across the parties experience broad variation in terms of their constituency characteristics, leading to important differences in their proportions of agricultural and foreign-born constituents. Thus, at the individual level, we should not be surprised if we find that constituency effects are secondary party. This logic is supported by Aldrich and colleague's (2008) study of the

Senate, which concludes that the attributes of senate districts – in which they include the proportion of the farming and foreign-born populations – have little bearing on Senator preferences *except* in cases where an issue is particularly crosscutting or salient. When issues lacked salience, party membership explained Senate voting.

While Democrats have shown signs that they would potentially support E-Verify, Republicans have generally taken a hardline stance, spearheading the movement to enact a mandatory E-Verify system (Lind, Rankin and Harris 2016). Absent an incentive to do otherwise, we would expect that Republicans would follow the party line (Battista and Richman 2011; Bullock and Brady 1983; Hill 1983). Jenkins (2008) explains that state legislators are motivated to commit to the positions of their parties on a given issue for the same reason they are motivated to respond to the preferences of constituents: doing so helps them to achieve reelection goals and advance their careers. Thus, in general we should find a clear party divide in how legislators vote on E-Verify:

H5: Republican legislators will be more likely than Democratic legislators to vote in favor of E-Verify legislation.

Nevertheless, there are some Republicans who openly express their concerns about E-Verify. In Florida, where E-Verify passed via Executive Order, Republican State Senator, J.D. Alexander, explains that he was unwilling to vote for E-Verify legislation because of the "tremendous costs to employers" (Alexander, quoted in Mazzei 2011). In North Carolina, Democrats and Republicans who represent large agricultural districts overrode Governor McCrory's veto of legislation exempting agricultural businesses from E-Verify (Wilson 2013). These lawmakers worried that farmers who have grown to depend on (unauthorized) immigrant labor would be irrevocably harmed by E-Verify. Democratic Representative Larry Hall contends that an exception for the agricultural sector was necessary because "it expresses confidence in our farmers and it gives them a stable workforce" (Hall, quoted in Wilson 2013). Rhode Island Governor Lincoln Chafee (2010) claims that in addition to the problems caused for farming and small business, E-Verify "ostracized our Latino communities… and it has done more harm than good." Given the concern that both Republicans and Democrats express for E-Verify's impact on particular communities, we should still find a strong relationship between constituency pressure and legislator voting:

> *H6: Republican legislators with large agricultural constituencies and Democratic legislators with large agricultural or foreign-born constituencies should be less likely to vote in favor of E-Verify laws than those with smaller agricultural and foreign-born constituencies*.

In sum, the theory and hypotheses presented here contribute to our knowledge of state legislative behavior by arguing that legislators are responsive to their constituencies even when dealing with policy issues of lower salience. Conventional notions of responsiveness have assumed constituency influence to be limited to those policy issues that are especially salient to the general public and that party-based pressures drive legislative behavior on issues of low-visibility. However, by shifting our perspective from the aggregate constituency to those sub-constituencies who are uniquely affected by the policy at hand, we can see that constituency pressures are capable of competing with and supplanting party-based pressures.

Data & Methods

I look at E-Verify in two stages. The first part of the analysis includes all states and examines what factors increase the probability of a state adopting an E-Verify law between 2006-2015. Following Newman et al. (2012), I use a discrete event history analysis (DEHA) where the dependent variable is coded 1 if a state adopted E-Verify and 0 otherwise. The DEHA model predicts the occurrence of a particular event by comparing the characteristics of those population members for which the event did occur in a particular timeframe to those for which it did not occur in the same timeframe (Allison 1982). In this case, our population is the 50 U.S. states, the event is passing an E-Verify law, and the timeframe is yearly units from 2006-2015. States are coded 1 if they pass *any* E-Verify policy during one of these yearly timeframes. Since several states have implemented more than one E-Verify policy, gradually widening the scope of employers affected, a state remains in the dataset as long as it is "at-risk" of passing an E-verify law. Once all employers are covered by a state's E-Verify policy, that state has no probability of passing an additional E-Verify law and exits the dataset. So, for example, Arizona is coded 0 in 2006, 1 in 2007, and 1 again in 2008, but then exits the dataset since there are no other employers for which it could apply an E-Verify requirement.

Thus, my method of analysis differs from Newman and colleagues in three important ways. First, the timeframe is extended by five years, which adds 13 observed events to the analysis. Second, whereas states exit Newman et al.'s dataset as soon as they pass a policy, I allow them to remain until they have no probability of passing another E-Verify law. Given that the DEHA model's estimates are based on comparing the characteristics of adoption and non-adoption states within each yearly time-unit, including these additional observations is crucial to accurately estimating why some states pass laws but others do not.[17] Substantively, because these successive laws target different employers, we are able to parcel out any key differences in constituency effects depending on the type of employer targeted by the law. Finally, I exclude E-Verify laws that were implemented via Executive Order, as the determinants behind executive orders are likely to vary considerably from laws adopted by state legislatures.

The second part of the study evaluates the motivations behind a legislator voting for or against E-Verify using a state fixed-effects logistic regression model. The dependent variable is coded 1 if the legislator voted in favor of the E-Verify bill and 0 if they voted against it (legislators who did not vote on the bill were excluded from the analysis). This results in the analysis of 28 bills and nearly 4,000 votes across 19 state legislatures.[18] It should be noted that the votes analyzed are specific to E-Verify legislation that passed. Ideally, we would also be able to examine roll call records for states that proposed, but did not pass E-Verify. However, to the best of my knowledge, E-

[17] Note that the models include a dummy variable for "previous policy" to account for the possibility that once a state passes one policy it is more likely to pass another.
[18] I do not include E-Verify laws enacted through Executive Orders, e.g. Idaho, Florida, Minnesota, and Rhode Island.

Verify has yet to fail due to "no" votes, but, rather, its fate has been repeatedly sealed during committee review. Some may worry (with good reason) that only analyzing voting behavior in states where E-Verify passed biases the results in favor of observing more support for E-Verify and potentially more partisan polarization. Yet, while E-Verify may be adopted by states where agriculture and foreign-born constituencies make up a smaller proportion of the overall population (leading to more unified support from Republican legislators), the size of these sub-constituencies varies markedly at the district-level. Consequently, we should still find constituency effects in those districts with the largest farm owner and foreign-born populations. If anything, by only having voting data from states that passed E-Verify, the bar is raised in terms of proving that sub-constituency preferences have an effect on legislative behavior.

To determine what influences states to pass E-Verify laws and legislators to vote for or against them, I focus on three key independent variables: partisanship, the size of the agricultural constituency, and the size of the foreign-born constituency. At the state-level partisanship is represented by two dummy variables: Democratic-controlled (43 percent) and Republican-controlled (44 percent) legislatures (split legislatures are the omitted baseline category). The most Republican-leaning legislature in the dataset is Indiana's 80th session with 74 percent of members identifying as Republicans. West Virginia's 2012 legislative session is the most Democratic-leaning with 69 percent of members identifying as Democrats. Legislator party affiliation, coded 1 for Republicans and 0 for Democrats, was determined from legislator biographical information found either on the websites of the state legislatures or the individual's personal website.

Approximately 58 percent of the legislators in the dataset identify as Republicans and 42 percent as Democrats.

To measure the size of the agricultural constituency I use the percentage of farm owners within the state and within each state legislative district. These estimates were obtained from the 2007 and 2012 USDA Census of Agriculture. The Census includes farm owner information at the national, state, and county level. To obtain estimates for the state legislative district level, I used the U.S. Census State Legislative Districts by Counties relationship files to merge the county-level data with the appropriate legislative district. Since districts often serve several partial counties, I divided each county's total number of farm owners by the number of districts that each county spans. Next, I combined the farm owner sub-totals for each partial county within a district to arrive at a district total. This total was divided by the district's population to arrive at the percentage of farm owners for each legislative district. While it is certainly true that not every county's population is divided equally between the various legislative districts that represent it, this should be an adequate approximation of agricultural interests at the state district level.[19] Overall, the size of the agricultural constituency ranges from 0 to 7 percent at the state-level and from 0 to 12 percent at the state district-level.

The foreign-born population, measured using estimates from the American Community Survey (ACS), is relatively larger than the agricultural constituency, reaching as much as 27 percent of a state's population and just under 50 percent at the

[19] As an alternative measure, I also estimated the legislator-level analyses using the percentage of farm workers in each district – a statistic that is reported at the district-level by the American Community Survey. The results, available upon request, show no major deviations from the tables reported below.

district-level. I chose the foreign-born population to estimate constituency influences (rather than individual measures for Latino, Asian, and unauthorized populations) for two reasons. As discussed above, the foreign-born population captures a diverse cross-section of racial and ethnic minorities that feel E-Verify would negatively impact their lives and is highly correlated with individual group-based measures (e.g., the Latino or Asian population). Second, there are no reliable estimations of the size of the unauthorized population at the state-district level and even if there were, this population – unlike the foreign-born community – cannot hold its elected officials accountable through voting.

In addition to the explanatory variables, the models employed below are composed of two sets of control variables. The first set captures characteristics of the legislatures and legislators. For both levels of analysis, I include measures of ideology, gender, and ethnicity. Although typically related, some studies have found that a legislator's ideology acts independently of their partisanship to influence how they vote on legislation (Jenkins 2006). I use Shor and McCarty's (2011) updated ideological scores for state legislatures and legislators to control for the influence of ideology on E-Verify preference. These scores are derived from a combination of legislator responses to the Project Vote Smart National Political Awareness Test and 15 years of roll call voting data. Smaller values indicate a more liberal legislator and positive values more conservative.

Given that Latino and female legislators tend to be associated with more liberal voting patterns as well as a proclivity for supporting legislation that advances immigrant interests (Bratton 2006; Bratton and Haynie 1999; Rocca, Sanchez, and Uscinski 2008;

Vega and Firestone 1995), I include dummy variables for each group. At the state-level, the percentage of a state's legislators that are women or Latino were identified using data from the Center for American Women and State Politics and the National Directory of Latino Elected Officials, respectively. At the individual-level, each legislator's gender and ethnicity were identified from legislator biographical information found either on the websites of state legislatures or the personal websites of the legislators.

Finally, I included measures for candidate incumbency and electoral competitiveness at the individual-level. These variables are included to account for the possibility that legislators are less accountable to their constituents when their seats in office are "safe" (Mayhew 1974). Bernstein (1989: 100) boldly claims that incumbent legislators can "generally afford to vote for what they think is right." Relatedly, Berry et al. (2000) find that legislators who run unopposed have a 0.988 probability of winning their next election. The bottom-line being that both of these factors could contribute to a legislator who feels less beholden to their constituents' opinions. For elections that happened in 2010 or earlier, I used Klarner's (2003) updated[20] State Legislative Election Returns data series to determine incumbency and electoral competitiveness (whether or not the election was contested). Election results from post 2010 were acquired via the relevant Secretary of State website allowing the author to calculate incumbency and electoral competiveness for each legislator.

The second set of control variables focus on state and district-level characteristics. Although the preferences of sub-constituencies have proven to be more reliable at

[20] Klarner's updated data (through 2010) are available at: http://academic.udayton.edu/SPPQ-TPR/klarner_datapage.html.

predicting legislative behavior than estimates of average district preferences at the national level (Clinton 2006; Fenno 1978; Fiorina 1974; Goff and Grier 1993; Miller and Stokes 1963), it is possible that state legislative voting patterns are best captured by the state or district's general ideology. As Bishin (2000) points out, the effect of the legal constituency – i.e. the average district ideology – is often underestimated because statistical models fail to also account for sub-constituencies. At the state-level, I use Berry et al.'s measure of citizen ideology, which aggregates state incumbent and challenger ideology scores weighted by voter support. The district estimates I use are from Tausanovitch and Warshaw's (2013) American Ideology Project, which measures the average level of constituent conservatism/liberalism based on responses to the Annenberg National Election Study and the Cooperative Congressional Election Study from 2000 to 2011. Smaller values indicate a more liberal citizenry and larger values more conservative. Consistent with extant studies on state-level immigrant-related policies, I also include the population size, the median household income, the unemployment rate, and the percentage of residents that obtained a bachelor's degree or higher at the state and district-level. Summary statistics for variables used in the state- and legislator-level analyses can be found in the appendix.

State-level Results

Existing scholarship on state legislative responsiveness would lead us to believe that because E-verify lacks general public salience, its passage/failure should be explained by legislative partisanship. I hypothesize, alternatively, that if we reconsider our notion of constituency, constituency pressures are capable of playing a fundamental role in explaining less salient policy as well. Specifically, passage of E-Verify should be contingent on the size of those constituencies most affected by its implementation: farm owners and the foreign-born community. Table 3.2[21] begins our investigation. The first model tests the relationship between legislature partisanship and the likelihood of passing an E-Verify policy (controlling for states that passed a prior E-Verify policy). Consistent with the findings of Zingher (2014) and Newman et al. (2012), Model 2.1 reveals that there is no statistically discernible difference in the probability of Republican and Democratic controlled legislatures passing E-Verify legislation.

In Model 2.2, I add the constituency variables to test our alternative explanation: salient constituencies – not partisanship – determine which state legislatures pursue E-Verify. This model depicts a strong relationship between constituency size and policy output, confirming our first hypothesis: states with larger agricultural and foreign-born constituencies are significantly less likely to adopt E-Verify. Although there is some indication that Republican legislatures are more prone to adopt E-Verify, this relationship

[21] Note that because we are using a discrete event history model, the reported observations in the tables are significantly lower than the 500 (50 states over 10 years) from which the models are estimated. This is due to two factors. First, states that are no longer "at risk" of adopting an E-Verify policy are dropped from the analysis (38 observations). Then, since time periods are treated as year fixed-effects, timeframes in which no states adopted an E-Verify policy are also dropped, leading to 48 observations dropped from 2009 and 42 observations dropped from both 2013 and 2014. I reran the models in Tables 2, 3, and 4 without year fixed-effects. The main effects are unchanged and the full results can be found in the appendix.

disappears after we include our full set of control variables in Model 2.3 while constituency effects remain strong.[22]

Table 3.2. Determinants of State Legislatures Adopting E-Verify

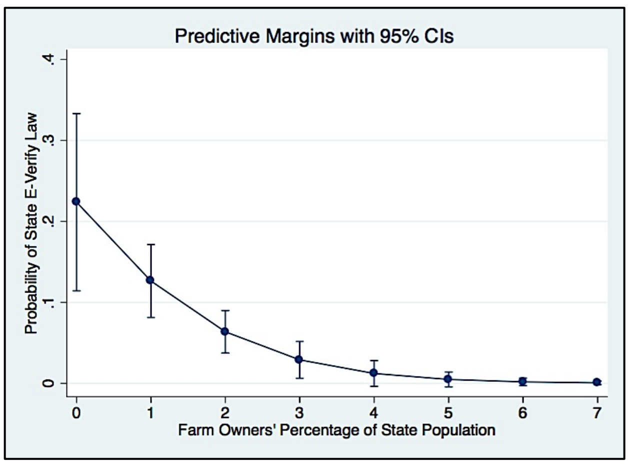
	(2.1)	(2.2)	(2.3)
Republican control	-0.043(0.928)	1.632*(0.733)	0.690(0.784)
Democratic control	1.367(0.796)	0.0933(0.878)	0.610(0.829)
Farm owners		-0.708***(0.186)	-0.969**(0.306)
Foreign-born		-0.136**(0.0502)	-0.196**(0.0743)
Legislature Characteristics			
Legislature ideology			0.471(0.335)
% Women			-4.466(5.304)
% Latino			4.516(4.176)
Prior Policy	1.350** (0.445)	1.160*(0.464)	0.805(0.618)
State Characteristics			
Average citizen ideology			-0.0186(0.0298)
Population (logged)			1.068**(0.410)
Median household income (logged)			-0.897***(0.267)
Unemployment rate			-0.463(0.371)
Bachelor's degree (+)			0.105(0.163)
Constant	-3.518***(0.846)	-1.799(0.945)	-2.402(5.125)
N	330	330	285
R^2	0.142	0.216	0.292

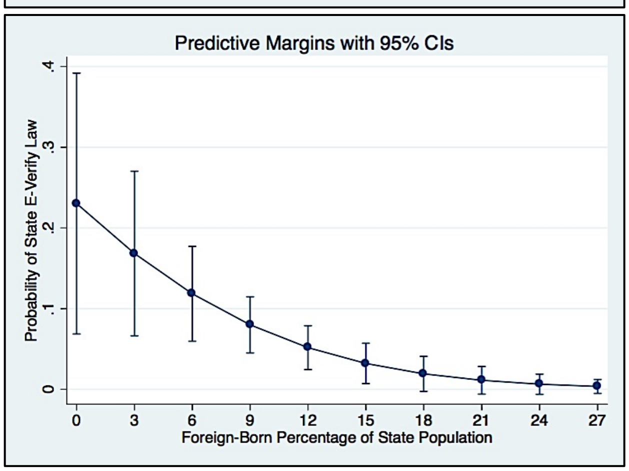
Models use discrete event history models with state clustered standard errors and time-period fixed-effects.
$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

According to Model 2.3 states with the fewest farm owners have about a 24 percent chance of adopting an E-Verify law. This probability drops to 7 percent when that constituency grows to just 3 percent of the population. States with no foreign-born population have a 21 percent probability of implementing an E-Verify policy, but this drops to 15.6 percent when the foreign-born population grows to 3 percent of the state’s population. Figures 3.1a and 3.1b compare the marginal effects (from Model 2.3) of the

[22] To further verify that legislature partisanship has a marginal effect on E-Verify adoption, I substituted party control of the legislature with the legislature proportion of Republican members. Consistent with the shown models, I find that state legislatures with larger proportions of Republican members are no more likely to adopt E-Verify than Republican-minority legislatures. These results are available upon request.

agricultural and foreign-born constituencies on the probability of adopting an E-Verify law. As illustrated, there is about a 1 percent probability of a state adopting E-Verify by the time the agricultural and foreign-born constituencies reach 5 and 21 percent of the population, respectively. Aside from the constituency variables, there is very little that seems to influence states adopting E-Verify.





Figures 3.1a & 3.1b. Marginal Effect of Farm Owners (top) and Foreign-born Constituency (bottom) on Probability of State Adopting an E-Verify Law (with 95% confidence intervals).

In Table 3.3, I replicate Model 2.3 based on the different employers targeted by

E-Verify: private, public, and contractors.[23] Although farm owners and the foreign-born community have a negative effect on the probability of a state passing any type of E-Verify mandate, this relationship is statistically significant in the case of private employers (Model 3.1) and contractors (Model 3.3), as predicted by the second hypothesis. The probability of passing a mandate specific to private employers decreases from 21 to 1.5 percent when the percentage of the farm owners increases from its smallest value to just three percent of the state population. Similarly, for E-Verify legislation specific to contractors, the probability of adoption drops from 14 percent to just over 1 percent as the farm owner population shifts from 0 to 3 percent of the state population. The presence of a large foreign-born community has a similar effect, decreasing the likelihood of adopting a private mandate from 25 percent to 5 percent and a contractor mandate from 13 to 4 percent once that group accounts for just 9 percent of the state population. Once again we find no meaningful relationship between partisan control of the legislature and probability of adopting an E-Verify mandate.

Table 3.3. Determinants of State Adoption of E-Verify Targeting Specific Employers

	(3.1)	(3.2)	(3.3)
	Private Employers	Public Employers	Contractors
Republican control	1.851(1.768)	-0.201(1.678)	-1.327(0.996)
Democratic control	0.189(1.550)	1.339(1.327)	0.282(0.873)

[23] To confirm the robustness of constituency effects, I also analyzed a simpler model that looked at the relationship between farm owners, the foreign-born community and the probability of passing any E-Verify policy, a policy specific to private employers, a policy specific to public employers, and a policy specific to contractors. This analysis was done cross-sectionally rather than overtime. Corroborating the findings in Table 3, larger agricultural and foreign-born populations significantly reduce the probability of a state passing general E-Verify legislation or bills specific to private employers and contractors (but not has no significant effect on legislation specific to public employers). These results are available in the appendix.

Farm owners	-1.383*(0.549)	-0.211(0.421)	-0.942**(0.294)
Foreign-born	-0.351*(0.166)	-0.088(0.187)	-0.181*(0.0918)
Legislature Characteristics			
Legislature ideology	0.152(0.589)	1.068(0.549)	0.930*(0.454)
% Women	-2.237(7.223)	-10.93(6.592)	-2.922(4.046)
% Latino	8.700(4.763)	-22.62(17.60)	3.516(5.043)
Prior Policy	2.578**(0.917)	-2.126(1.669)	-0.650(1.105)
State Characteristics			
Average citizen ideology	0.004(0.039)	-0.077*(0.034)	-0.041*(0.019)
Population (logged)	0.324(0.486)	2.923***(0.670)	2.037**(0.697)
Median household income (logged)	-0.627(0.453)	-0.510(0.744)	-1.616*(0.705)
Unemployment rate	-0.283(0.490)	-0.424(0.831)	-1.045(0.549)
Bachelor's degree (+)	-0.143(0.245)	0.804*(0.330)	0.0498(0.170)
Constant	2.485(7.137)	-46.26**(15.54)	-2.867(6.245)
N	208	251	318
R^2	0.418	0.327	0.253

Models use discrete event history analysis with state clustered standard errors and time-period fixed-effects.
$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Yet, even if party control has no independent effect on whether or not states adopt E-Verify, there is reason to believe that farm owners and the foreign-born population will have unique effects on Republican- and Democratic-controlled legislatures. Model 4.1 provides strong support for the third hypothesis: Republican legislatures are far less likely to adopt E-Verify if they have a large agricultural constituency. A Republican legislature with no agricultural constituents has a nearly 46 percent probability of passing an E-Verify law. This probability drops dramatically to 4 percent when the agricultural constituency reaches just 3 percent of the state's population, as illustrated in Figure 3.2. Indeed, despite the farm owners making up as much as 7 percent of a state's population, E-Verify has not yet been implemented in any state where the total percentage of farm owners is greater than 3.1 percent.

Table 3.4. Effect of Salient Constituencies on Republican & Democratic Legislatures

	(4.1)	(4.2)
	Republican Legislatures	Democratic Legislatures
Farm owners	-1.265*(0.537)	-0.149(0.513)
Foreign-born	-0.187(0.127)	-0.299*(0.141)
Legislature Characteristics		
Legislature ideology	0.319(0.536)	0.0301(0.718)
% Women	-0.888(9.803)	-5.233(7.795)
% Latino	3.165(9.249)	0.419(6.476)
Prior Policy	0.543(1.163)	2.282(1.321)
State Characteristics		
Average citizen ideology	-0.0484(0.0966)	-0.0217(0.0275)
Population (logged)	0.624(0.433)	4.139*(1.834)
Median household income (logged)	-0.511(0.272)	-2.575(1.346)
Unemployment rate	-0.177(0.564)	-2.845**(1.094)
Bachelor's degree (+)	-0.0552(0.360)	0.0967(0.173)
Constant	2.060(7.933)	-15.40(15.10)
N	102	112
R^2	0.276	0.327

Models use discrete event history models with state clustered standard errors and time-period fixed-effects.
$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

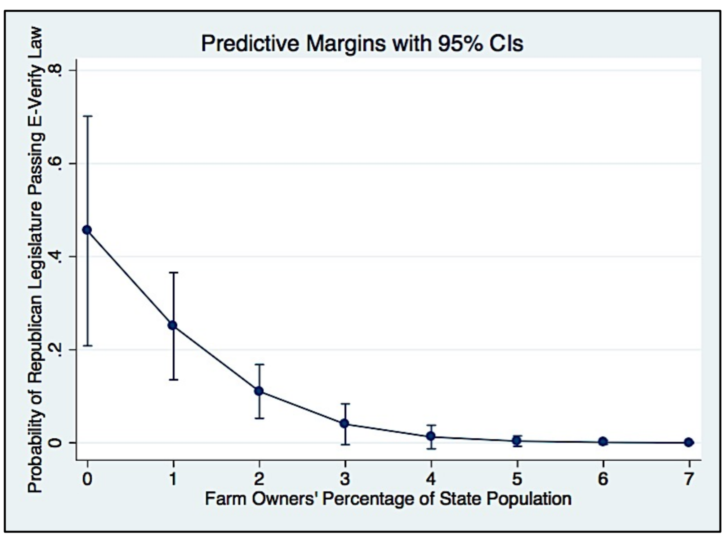


Figure 3.2. Probability of a Republican Legislature Passing E-Verify as the State's Proportion of Farm Owners Increases (with 95% confidence intervals).

Like Republican legislatures, Model 4.2 tells us that salient constituents also impact Democratic legislatures. Although more E-Verify policies were adopted (10) in states with above average percentages of foreign-born constituencies (7.9 percent), the relationship between partisanship and constituency responsiveness becomes even clearer: only 3 of those 10 policies were implemented in states with Democratic legislatures. Indeed, by the time the foreign-born population reaches 10 percent of the state's population, there is less than a 5 percent chance that a Democratic legislature will pursue E-Verify. However, we only find partial support for the third hypothesis: both the foreign-born population and farm owners reduce the odds of a Democratic legislature adopting E-Verify, but only the effect of the foreign-born community reaches conventional thresholds of statistical significance.

Combined, the models above demonstrate that when dealing with a policy issue of lesser general salience, state legislatures do not ignore constituency preferences, but rather respond to those sub-constituents for which the policy matters most. Nearly every state has introduced an E-Verify bill, but only 19 legislatures have passed E-Verify mandates. In all other cases the legislation died while in committee, never making it to the floor for a vote. The implication is clear: large agricultural and foreign-born constituencies play a crucial role in preventing Republican and Democratic legislatures, respectively, from making E-Verify part of their legislative agenda.

Legislator-level Results

The previous section established the strong influence that salient sub-constituencies can have on legislature adoption of E-Verify. Turning to the determinants of an individual legislators' vote on such legislation, I expect that we will continue to find constituency effects for those legislators who represent particularly large farming and foreign-born populations, but that partisanship should be the predominant factor explaining the voting preferences of legislators in general. To test what best explains legislator voting on E-Verify I use state fixed-effects logistic regression models with state clustered standard errors. This controls for all time-invariant state characteristics that might be influencing the relationship between our variables of interest and the outcome. Model 5.1 of Table 3.5 begins this investigation by regressing the legislator's vote in favor (1) or opposition (0) to E-Verify against their party affiliation, coded 1 for Republican legislators and 0 for Democrats. Consistent with the fifth hypothesis' prediction, Republican legislators are more likely to vote in favor of E-Verify than Democrats.

Table 3.5. Determinants of Legislator Support for E-Verify

	(5.1)	(5.2)
Republican	3.208^{***} (0.129)	2.033^{***}(0.201)
Farm owners		0.301(0.646)
Foreign-born		-0.143(0.0932)
Legislator Characteristics		
Ideology		0.369^{***}(0.110)
Female		-0.0651(0.133)
Latino		-0.452(0.343)
Incumbent		-0.137(0.135)
Contested election		-0.0234(0.135)
District Characteristics		
Average ideology		0.876^{***}(0.235)
Population (logged)		0.595^{***}(0.147)
Median household income (logged)		0.181(0.315)
Unemployment rate		-1.554^{***}(0.284)
Bachelor's degree (+)		-0.301^{**}(0.0928)
N	3918	3736

Models use logistic regression with state-fixed effects and state-clustered standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

In fact, as shown in Figure 3.3, the 94 percent of Republicans voting 'yea' on E-Verify is 33 percentage points more than the 61 percent of Democrats who favored the bill. According to the state-level analysis, the probability of E-Verify being implemented under a Republican legislature is 13.9 percent, only about 9 percentage points higher than under a Democratic legislature. Thus, while the state-level analysis alludes to the partisan difference in support for E-Verify, it fails to capture just how dissimilar the parties' preferences are on this subject. Beyond Republican legislators being overwhelming more supportive of E-Verify than Democrats, Figure 3.3 illustrates that Democrats are themselves quite divided. This suggests that even at the legislator-level we may find meaningful constituency effects. I investigate this possibility later on in Tables 3.7 and 3.8.

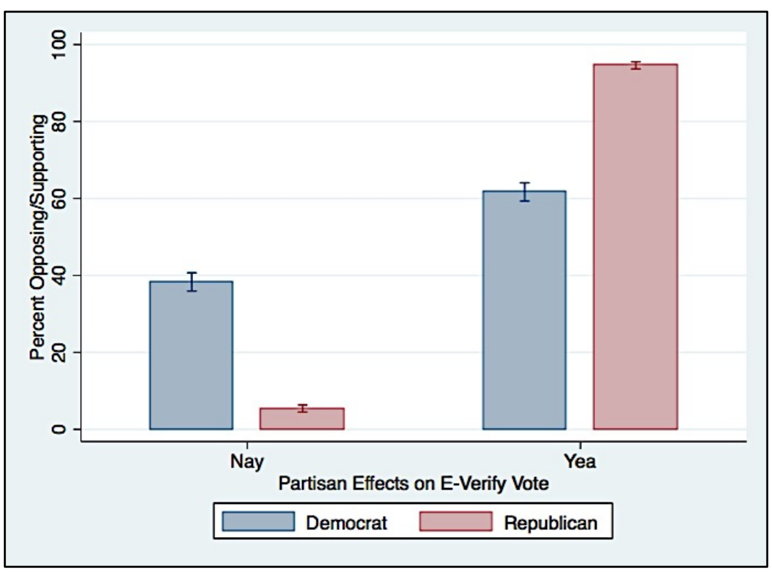


Figure 3.3. Differences in Support for E-Verify Within and Between Parties.

Model 5.2 confirms that the effect of legislator partisanship on support for E-Verify persists even when controlling for a host of other factors. In fact, partisanship is by far the primary determinant of legislative voting behavior. Figure 3.4 provides a telling visual depiction of this finding. It compares the effect of the partisanship coefficient to the constituency variables as well as other statistically significant variables in the model. We see that in most cases the effect of partisanship on the probability of voting in favor of E-Verify is more than twice as large as the other coefficients.[24]

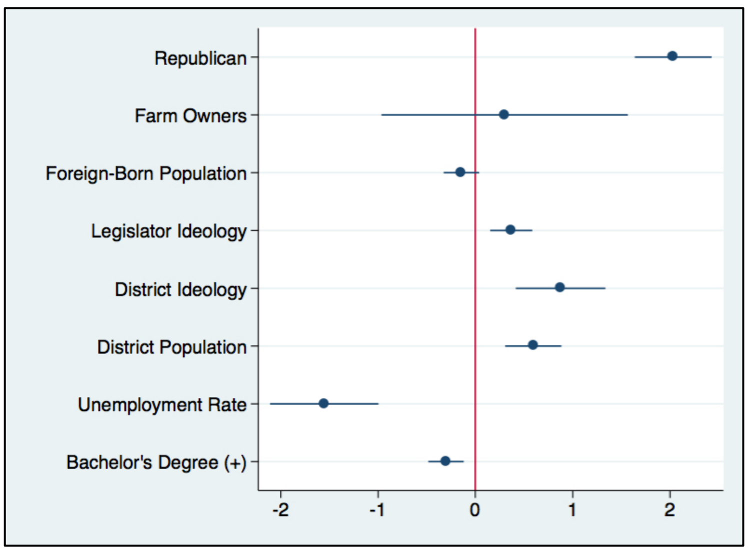


Figure 3.4. Effect of Partisanship vs. Key Variables on E-Verify Support (with 95% confidence intervals).

As pointed out in the state-level analysis, state adoption of E-Verify happens far more frequently in states with fewer farm owners and smaller foreign-born populations. In fact, of the states that enacted E-Verify mandates the agricultural and foreign-born populations did not exceed 3.1 percent and 19.4 percent of the population, respectively,

[24] The variables were transformed to be on similar scales so that we could more easily compare the effect of their coefficients on the probability of a ‘yea’ vote.

while they accounted for as much as 7 and 27 percent of the population in states that did not pass E-Verify. Yet, within states that implemented E-Verify we do find sizeable variation in the district percentages of these populations: in some districts the farm owners make up as much as 12 percent of the total constituency and the foreign-born population 50 percent. Consequently, when we look at how Republican and Democratic legislators vote on E-Verify, we would anticipate, in accordance with hypothesis six, for these groups to influence partisan voting patterns.

Models 6.1 and 6.2 test this expectation formally. Based on Figure 3.3, we know that Republicans in these states are exceedingly committed to supporting E-Verify, making the bar for finding a constituency effect quite high. We also know, based on the state-level results, that constituency factors only mattered for bills targeting private employers and contractors. Subsequently, Table 3.6 uses the same covariates from Model 5.2, but eliminates legislative voting on bills that are specific to public employers. If Republicans are cross-pressured when it comes to E-Verify – inclined to support it from a partisan perspective, but compelled to oppose it because of a loyalty to agribusiness – then we should find a negative relationship between agriculture and Republican support when the proposed legislation targets *private* employers or contractors. Model 6.1 confirms this. Whereas the presence of a large constituency of farm owners appears to have no meaningful effect on the voting behavior of Republican legislators when considering *all* E-Verify bills,[25] we can see from the results in Table 3.6 that the

[25] Results from the same analysis using votes on all bill types can be found in the appendix. While the constituency variables have no meaningful effect on Republican

preferences of this group does matter when the bill specifically affects their interests. This substantiates our expectations in the sixth hypothesis, albeit with a caveat: republicans representing large agricultural constituencies are less likely to vote in favor of E-Verify *when the mandate directly affects those employers*.

Table 3.6. The Influence of Salient Constituencies on Republican & Democratic Support for E-Verify

	(6.1)	(6.2)
	Republican Legislators	Democratic Legislators
Farm owners	-19.04^{*}(22.02)	9.676(16.23)
Foreign-born	-9.160(5.162)	-7.011^{**}(2.229)
Legislator Characteristics		
Ideology	0.772^{**}(0.279)	1.031^{*}(0.502)
Female	0.202(0.294)	0.211(0.216)
Latino	0(.)	0.304(1.078)
Incumbent	-1.032^{**}(0.328)	-0.493(0.575)
Contested election	-0.223(0.606)	0.725(0.457)
District Characteristics		
Average ideology	0.544(0.943)	1.940^{**}(0.685)
Population (logged)	1.659^{*}(0.645)	0.716(0.600)
Median household income (logged)	0.164(2.528)	-2.785^{***}(0.674)
Unemployment rate	1.759(2.798)	-0.879(0.500)
Bachelor's degree (+)	1.559^{*}(0.742)	-0.00519(0.328)
Constant	-17.47(34.20)	23.31^{*}(11.83)
N	738	496

Models use logistic regression with state-clustered standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Unlike Republicans, Figure 3.3 depicted Democrats as noticeably divided on E-Verify. Model 6.2 helps us to sort out this intraparty discord. Consistent with the state-level analysis, a larger foreign-born constituency bolsters Democratic opposition to E-Verify even at the voting stage while the presence of farm owners has no meaningful impact on how Democrats vote. Figure 3.5 shows us that all else equal in Model 6.2 the probability of a Democrat voting in favor of E-Verify drops from 0.65 when representing

legislators, the foreign-born community significantly reduces a Democratic legislator's probability of voting 'yea' on E-Verify legislation.

a district with no foreign-born constituency to 0.05 when nearly half of their district's population is made up of foreign-born constituents.

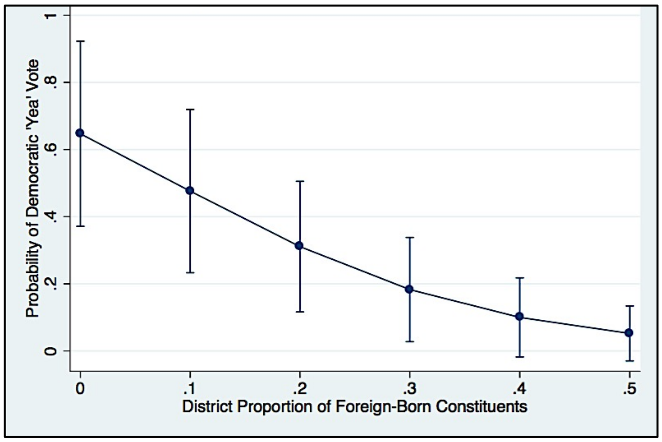


Figure 3.5. Effect of Foreign-born Constituency on Democratic Support for E-Verify (with 95% confidence intervals).

Interestingly, we also discover that both Republican and Democratic legislators are inclined to support E-Verify as the average conservatism of their districts grows. However, the magnitude of this effect is quite different for the two parties. While moving from the most liberal Republican-represented district to the most conservative only increases the probability of a Republican 'yea' vote by 4 percentage points, we can see from Figure 3.6 that the predicted probability of a Democratic 'yea' vote increases from 0.15 in the most liberal districts to 0.89 in the most conservative (keeping all other variables from Model 6.2 at their means). This is consistent with the conclusions of Bishin (2000), who argues that the aggregate opinion of a constituency has more value in predicting responsiveness if we incorporate measures of sub-constituency preferences into our statistical models.

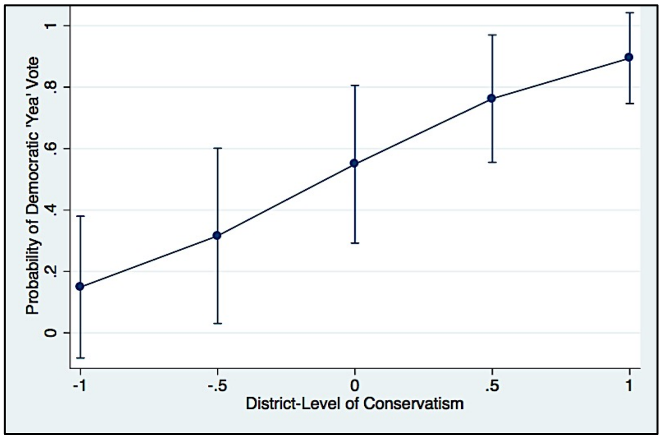


Figure 3.6. Effect of Average District Ideology on Democratic Support for E-Verify (with 95% confidence intervals).

In sum, the legislator-level analysis demonstrates that there is a clear partisan dimension to voting, but that constituency characteristics can further help to clarify why legislators break from their party's position. Republicans, for example, are nearly uniform in their support for E-Verify, but will yield to the pressures of agribusiness if the proposed legislation has a direct impact on agricultural interests. Democrats, on the other hand, vote against E-Verify more often than Republicans, but they are also more strongly swayed by their constituents in general.

Conclusions

This work contributes to growing efforts to better understand legislative behavior in the states by rethinking the connection between constituency pressure and responsiveness. I consider that at the state legislative-level, where it is not unusual for policies to be relevant only amongst a sub-constituency of the district populations, we can learn much from existing theories on sub-constituency pressure and legislative responsiveness at the national-level. Most state-level research has argued that legislative responsiveness is limited to aggregate preferences on broadly salient issues. In contrast, I show that a policy issue need not be salient in general to evoke responsiveness, we just need to narrow our focus concerning whom that response is targeting.

Using the case of E-Verify, I find that state policy and legislator voting is responsive to constituent preferences on less salient issues, but that the target of their responsiveness shifts from the aggregate to those specific sub-constituencies for which the policy is particularly relevant. Furthermore, that responsiveness plays out in different ways depending on the level of analysis. At the state-level, E-Verify is kept off the agenda in states where pressure from the agribusiness and foreign-born populations is strongest while such legislation passes in states where the voting power of these sub-constituencies is considerably weaker. This effect occurs regardless of the legislature's partisan composition. At the individual-level, we find a clear division in legislator support for E-Verify along party lines, but there is also evidence that sub-constituency pressures are capable of altering both Republican and Democratic positions.

To provide greater cogency to this argument, future studies should seek two sources of information. First, actual data on public opinion toward E-Verify at both the state and district level would provide us with a more precise way of measuring legislative responsiveness to constituents. Second, without the roll call records of proposed legislation that failed, we need to know more about the legislative process that led to E-Verify's collapse: how far did E-Verify legislation get in the legislative process and why did it fail? As far as we know, E-Verify legislation dies in committee long before it reaches the floor for a vote. Do the co-sponsors who introduce this legislation and the committee members who prevent its movement have distinct party- and/or constituent-based interests in taking seemingly opposing actions? While this additional information would make for a stronger test of our hypotheses and overarching theory of legislative responsiveness, the models tested above provide ample evidence to warrant future research on the relationship between sub-constituency preferences and state/legislator responsiveness.

These results also have important implications for our understanding of the relationship between the policy process and democratic representation more generally. If legislators practice a form of policy particularism toward groups that care deeply about issues when the larger constituency has weak or unknown preferences on them, should this behavior be considered more or less responsive in the democratic sense? The known alternatives, that legislators act based on their party's preferences or the general ideology of their aggregate constituency, are both potentially antithetical to democratic representation, leading to policies that are highly incongruent with actual attitudes (Lax and Phillips 2012). Absent educating voters about ongoing legislation and improving the lines of communication between legislators and their constituents, responding to the preferences of a salient sub-constituency when the attitudes of the masses are unknown or only weakly defined may be the least-worst option for legislators who want to represent and defend the interests of their constituents.

APPENDIX 1: CHAPTER 1

Table A1.1. Mayoral Elections in Data Set

City	State	Election Year	Election Type	Election Stage	Number of Candidates
Mobile	AL	2005	Nonpartisan	Runoff	2
Mobile	AL	2013	Nonpartisan	General	2
Mobile	AL	2005	Nonpartisan	General	4
Montgomery	AL	2009	Nonpartisan	General	4
Montgomery	AL	2011	Nonpartisan	General	2
San Francisco	CA	1995	Nonpartisan	Runoff	2
San Francisco	CA	1995	Nonpartisan	General	3
Los Angeles	CA	2005	Nonpartisan	General	4
Los Angeles	CA	2013	Nonpartisan	General	4
Oakland	CA	2014	Nonpartisan	General	6
Sacramento	CA	2008	Nonpartisan	General	3
Sacramento	CA	2008	Nonpartisan	Runoff	2
Denver	CO	2015	Nonpartisan	General	3
Denver	CO	2011	Nonpartisan	Runoff	2
Denver	CO	2011	Nonpartisan	General	4
Denver	CO	1991	Nonpartisan	General	3
Denver	CO	1999	Nonpartisan	General	4
Denver	CO	2003	Nonpartisan	General	5
Washington	DC	1994	Partisan	General	2
Washington	DC	2014	Partisan	General	2
Jacksonville	FL	2011	Nonpartisan	General	4
Jacksonville	FL	2003	Nonpartisan	Runoff	2
Jacksonville	FL	2015	Nonpartisan	General	3
Jacksonville	FL	2011	Nonpartisan	Runoff	2
Jacksonville	FL	2003	Nonpartisan	General	4
Orlando	FL	2003	Nonpartisan	General	5
Orlando	FL	2004	Nonpartisan	General	3
St. Petersburg	FL	2009	Nonpartisan	General	5
Tampa	FL	2011	Nonpartisan	General	5
Tampa	FL	2007	Nonpartisan	General	3
Athens	GA	2006	Nonpartisan	Runoff	2
Athens	GA	2010	Nonpartisan	General	4
Athens	GA	2006	Nonpartisan	General	3
Atlanta	GA	2013	Nonpartisan	General	3
Atlanta	GA	2009	Nonpartisan	Runoff	2

Atlanta	GA	2009	Nonpartisan	General	3
Augusta	GA	1995	Nonpartisan	General	2
Augusta	GA	2010	Nonpartisan	General	3
Augusta	GA	1998	Nonpartisan	Runoff	2
Augusta	GA	2002	Nonpartisan	General	4
Augusta	GA	2002	Nonpartisan	Runoff	2
Augusta	GA	1998	Nonpartisan	General	4
Augusta	GA	2006	Nonpartisan	General	3
Macon	GA	2013	Nonpartisan	Primary	4
Macon	GA	2013	Nonpartisan	General	2
Aurora	IL	2005	Nonpartisan	General	4
Aurora	IL	2005	Nonpartisan	Runoff	2
Aurora	IL	2009	Nonpartisan	General	2
Chicago	IL	1999	Nonpartisan	General	2
Chicago	IL	2003	Nonpartisan	General	3
Chicago	IL	2015	Nonpartisan	General	4
Indianapolis	IN	2011	Partisan	Primary	3
Wichita	KS	2011	Nonpartisan	General	2
Wichita	KS	2007	Nonpartisan	General	2
New Orleans	LA	1994	Partisan	Primary	5
New Orleans	LA	1994	Partisan	General	2
New Orleans	LA	1990	Partisan	General	2
New Orleans	LA	2006	Partisan	General	2
New Orleans	LA	1998	Partisan	Primary	2
New Orleans	LA	2010	Partisan	Primary	4
New Orleans	LA	2014	Partisan	Primary	2
New Orleans	LA	2006	Partisan	Primary	4
Boston	MA	2013	Nonpartisan	Primary	3
Baltimore	MD	1995	Partisan	Primary	2
Baltimore	MD	1999	Partisan	Primary	3
Baltimore	MD	1991	Partisan	General	2
Detroit	MI	2013	Nonpartisan	General	2
Detroit	MI	2013	Nonpartisan	Primary	3
St. Louis	MO	1993	Partisan	Primary	4
St. Louis	MO	2001	Partisan	Primary	3
St. Louis	MO	1993	Partisan	General	2
St. Louis	MO	2005	Partisan	General	2
St. Louis	MO	2013	Partisan	Primary	2
St. Louis	MO	2009	Partisan	Primary	3
St. Louis	MO	2009	Partisan	General	2

Charlotte	NC	2001	Partisan	Primary	3
Charlotte	NC	2001	Partisan	General	2
Albany	NY	2005	Partisan	General	2
Albany	NY	2013	Partisan	Primary	2
Albany	NY	2005	Partisan	Primary	2
Albany	NY	2009	Partisan	General	2
Albany	NY	2009	Partisan	Primary	2
Buffalo	NY	1997	Partisan	Runoff	2
Buffalo	NY	2001	Partisan	Primary	2
Buffalo	NY	1997	Partisan	General	3
Buffalo	NY	2005	Partisan	General	2
Buffalo	NY	2009	Partisan	Primary	2
NYC	NY	2013	Partisan	Primary	4
NYC	NY	2009	Partisan	General	2
NYC	NY	2005	Partisan	Primary	5
Syracuse	NY	2009	Partisan	General	3
Syracuse	NY	2009	Partisan	Primary	4
Cincinnati	OH	2009	Nonpartisan	Primary	2
Cincinnati	OH	2001	Nonpartisan	Primary	2
Cincinnati	OH	2005	Nonpartisan	Primary	2
Cleveland	OH	2005	Nonpartisan	General	6
Cleveland	OH	2005	Nonpartisan	Primary	6
Columbus	OH	2011	Partisan	General	2
Columbus	OH	1995	Partisan	General	2
Columbus	OH	1991	Partisan	General	2
Columbus	OH	2007	Partisan	General	2
Columbus	OH	1999	Partisan	General	2
Philadelphia	PA	2007	Partisan	General	2
Philadelphia	PA	1999	Partisan	Primary	3
Philadelphia	PA	2015	Partisan	Primary	3
Philadelphia	PA	2007	Partisan	Primary	5
Philadelphia	PA	2011	Partisan	General	2
Philadelphia	PA	1991	Partisan	Primary	4
Philadelphia	PA	2003	Partisan	General	2
Philadelphia	PA	1999	Partisan	General	2
Pittsburg	PA	2009	Partisan	Primary	3
Pittsburg	PA	2013	Partisan	Primary	3
Memphis	TN	1991	Nonpartisan	Primary	2
Memphis	TN	1995	Nonpartisan	Primary	2
Houston	TX	2011	Nonpartisan	General	5

Houston	TX	1991	Nonpartisan	General	2
Houston	TX	1997	Nonpartisan	Runoff	2
Houston	TX	2013	Nonpartisan	General	3
Houston	TX	1991	Nonpartisan	Runoff	2
Houston	TX	1997	Nonpartisan	General	2
Houston	TX	2015	Nonpartisan	General	6
Houston	TX	1999	Nonpartisan	General	3
Houston	TX	2003	Nonpartisan	General	3
Houston	TX	2009	Nonpartisan	General	2
Seattle	WA	2009	Nonpartisan	Primary	5
Milwaukee	WI	2012	Nonpartisan	Primary	2
Milwaukee	WI	2012	Nonpartisan	General	2
Milwaukee	WI	2004	Nonpartisan	General	2

Table A1.2. Effect of Partisan Cues on Black Vote Share (no Partisan Primaries)

	(2.1)	(2.2)	(2.3)
% Party mentions		40.017***(11.571)	31.52***(6.938)
% Race mentions			2.009(7.763)
Nonpartisan elections	-10.61*(4.143)	-2.157 (7.678)	7.801(5.075)
% Party mentions x nonpartisan elections		5.313 (15.357)	
Incumbent			26.14***(4.808)
3(+) Candidates			-8.249*(3.510)
Election Type			0(.)
General			6.133(5.380)
Runoff			12.29(8.474)
Population (logged)			1.233(2.266)
% Black			0.131(0.142)
% White			0.0141(0.178)
% Democratic vote			0.0948(0.152)
Bachelor's degree			0.0800(0.216)
Median household income (logged)			-2.846(6.833)
% Unemployed			-0.0610(0.464)
Constant	46.18***(3.718)	25.596**(7.555)	15.79(93.53)
N	101	101	100
R^2	0.083	0.318	0.614

Note: All Models use OLS regression with a dummy variable for contests that occurred in the same election sequence and robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A1.3. Effect of Partisan Cues on Black Election Victory (no Partisan Primaries)

	(3.1)	(3.2)	(3.3)
% Party mentions		4.313^{*}(1.835)	4.537^{**}(1.478)
% Race mentions		0.244(1.070)	1.413(0.836)
Nonpartisan elections	-0.722(0.471)	-0.0847(2.290)	
% Party mentions x nonpartisan elections			1.994(1.821)
Incumbent			3.672^{**}(1.227)
3(+) Candidates			0.0191(0.851)
Election Type			
General			0.209(0.913)
Runoff			1.013(1.309)
Population (logged)			0.122(0.443)
% Black			0.00808(0.0269)
% White			0.00611(0.0356)
% Democratic vote			0.0507(0.0313)
Bachelor's degree			0.0282(0.0424)
Median household income (logged)			-0.584(1.444)
% Unemployed			-0.130(0.0963)
Constant	0.323(0.440)	-1.829(1.017)	-3.364(17.71)
N	101	101	100
R^2	0.018	0.120	0.288

Note: Models are logistic regressions with a dummy variable for contests that occurred in the same election sequence and robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A1.4. Effect of Partisan Cues on Black Vote Share (Only 1 Black Candidate in Election)

	(4.1)	(4.2)	(4.3)
% Party mentions		31.98^{**}(10.68)	24.18^{***}(6.679)
% Race mentions			-7.061(8.358)
Nonpartisan elections	-8.266^{*}(3.768)	-6.653(6.220)	-0.468(4.465)
% Party mentions x nonpartisan elections		12.07(14.18)	
Incumbent			20.94^{***}(4.328)
3(+) Candidates			-14.28^{***}(3.236)
Election Type			
General			0.889(4.078)
Runoff			8.267(8.610)
Population (logged)			1.166(1.724)
% Black			0.0635(0.124)
% White			-0.0378(0.168)
% Democratic vote			-0.188(0.163)
Bachelor's degree			0.0810(0.150)
Median household income (logged)			0.359(7.287)
% Unemployed			0.379(0.475)

Constant	41.70***(3.122)	28.25***(5.800)	22.59(80.27)
N	106	106	104
R^2	0.076	0.287	0.626

Note: All Models use OLS regression with a dummy variable for contests that occurred in the same election sequence and robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A1.5. Effect of Partisan Cues on Black Victory (Only 1 Black Candidate in Election)

	(5.1)	(5.2)	(5.3)
% Party mentions		2.573(1.318)	2.928*(1.363)
% Race mentions			0.630(1.955)
Nonpartisan elections	-0.439(0.403)	1.389(1.946)	
% Party mentions x nonpartisan elections		-0.482(0.834)	1.024(0.893)
Incumbent			3.892**(1.317)
3(+) Candidates			-0.318(0.728)
Election Type			
General			0.779(0.734)
Runoff			1.621(1.306)
Population (logged)			0.473(0.381)
% Black			0.0363(0.0280)
% White			0.0371(0.0343)
% Democratic vote			0.0298(0.0357)
Bachelor's degree			0.0469(0.0356)
Median household income (logged)			0.469(1.534)
% Unemployed			-0.0636(0.103)
Constant	-0.133(0.325)	-1.222(0.677)	-20.08(17.31)
N	106	106	104
R^2	0.009	0.079	0.303

Note: Models are logistic regressions with a dummy variable for contests that occurred in the same election sequence and robust standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A1.6. Effect of Partisan Cues on Black Vote Share (no "Linked" Contests")

	(6.1)	(6.2)	(6.3)
% Party mentions		34.05**(10.32)	30.46***(6.120)
% Race mentions			-6.898(6.468)
Nonpartisan elections	-7.722(4.107)	-5.359(6.504)	6.394(3.562)
% Party mentions x nonpartisan elections		20.29(14.77)	

Incumbent			26.94^{***}(4.290)
3(+) Candidates			-10.76^{***}(2.827)
Population (logged)			0.400(1.660)
% Black			0.0339(0.120)
% White			-0.0559(0.164)
% Democratic vote			-0.0482(0.141)
Bachelor's degree			-0.0204(0.187)
Median household income (logged)			-5.626(7.503)
% Unemployed			-0.0444(0.440)
Constant	41.91^{***}(3.394)	25.83^{***}(5.988)	84.84(86.81)
N	101	101	100
R^2	0.043	0.280	0.675

Note: Models A7.1, A7.2, and A7.3 use OLS regression. All models use robust standard errors in parentheses.
$p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A1.7. Effect of Partisan Cues on Black Victory (no "Linked" Contests")

	(7.1)	(7.2)	(7.3)
% Party mentions		3.098^{*}(1.281)	4.161^{**}(1.367)
% Race mentions		0(.)	0.0669(1.894)
Nonpartisan elections	-0.218(0.424)	0.116(0.868)	1.917^{*}(0.838)
% Party mentions x nonpartisan elections		1.283(1.988)	
Incumbent			4.184^{***}(1.238)
3(+) Candidates			-0.734(0.638)
Population (logged)			0.0532(0.408)
% Black			-0.00539(0.0283)
% White			-0.00512(0.0418)
% Democratic vote			0.0230(0.0341)
Bachelor's degree			0.0401(0.0435)
Median household income (logged)			-0.369(1.641)
% Unemployed			-0.179(0.126)
Constant	-0.307(0.343)	-1.814^{*}(0.707)	-1.062(19.06)
N	101	101	100
R^2	0.002	0.100	0.368

Note: Models A7.4, A7.5 and A7.6 use logistic regression. All models use robust standard errors in parentheses.
$p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

APPENDIX 2: CHAPTER 2

1. Robustness Check for Model Specifications

The number of deportations ranges from to more than 35,000, with a mean of 126.74. That means that 30 observations are more than 2 standard deviations above the mean and 45 are more than 1 standard deviation above the mean. To reassure readers that the results found in Table's 2.3 and 2.4 are not merely a result of a few extreme outliers, I reran the models dropping all observations 1 and 2 standard deviations above the mean. As the Table's A2.1 and A2.2 demonstrate, when making this adjustment the coefficients and statistical significance of the main explanatory variables increase suggesting an even stronger fit. While Conservatism still has a null effect on deportations, in Model 3.1 dropping these observations resulted in a positive coefficient for Conservatism with p-values of 0.912 and 0.654 in the 1 and 2 standard deviation models, respectively. On the other hand, in Model 3.2 Conservatism maintains its negative relationship, as shown in Table A2.2. Despite a somewhat more ambiguous relationship between Conservatism and deportations, both Tables A2.1 and A2.2 demonstrate a strong relationship between the resource variables and the outcome and a weak relationship between Conservatism and Deportations.

Table A2.1. Dropping All Observations 1 & 2 Standard Deviations Above Mean, Model 3.1

	Model 3.1: Dropping if >1 SD	**Model 3.1 Dropping if > 2 SD**
Independent Variables		
Private Correctional Facility	0.828^{**}(0.284)	0.806^{**}(0.282)

IGSA Contract	0.633***(0.133)	0.643***(0.128)
Budget per resident	0.082***(0.019)	0.085***(0.021)
Conservatism	0.00008(0.0035)	0.001(0.004)
Latino Population	0.038***(0.004)	0.043***(0.006)
Crime Rate	2.01e-05(2.55e-05)	2.27e-05(2.31e-05)
County Population (centered)	0.006***(0.0006)	0.006***(0.0005)
GDP Fruits & Vegetables	0.131(0.134)	0.089(0.136)
GDP Construction	-0.029(0.083)	-0.045(0.082)
Unemployment Rate	-0.022(0.017)	-0.028(0.018)
287(g) Participant	-0.040(0.089)	-0.051(0.090)
Border State	-0.141(0.141)	-0.163(0.141)
Dream Act	-0.280**(0.095)	-0.288**(0.095)
Favors SB1070	0.028***(0.005)	0.027***(0.005)
State % Unauthorized	0.280***(0.040)	0.279***(0.040)
Constant	-1.067**(0.473)	-0.948*(0.473)
Inflated Model		
Private Correctional Facility	-0.558(1.678)	-0.595(1.712)
IGSA Contract	-0.392(0.485)	-0.390(0.490)
Budget per resident	-0.149*(0.069)	-0.148*(0.070)
Conservatism	0.003(0.012)	0.004(0.013)
Latino Population	-0.807***(0.245)	-0.810***(0.251)
Crime Rate	-0.0004(0.0009)	-0.0004(0.001)
County Population (centered)	-0.144***(0.025)	-0.146***(0.025)
GDP Fruits & Vegetables	0.327(0.358)	0.312(0.360)
GDP Construction	0.370(0.229)	0.369(0.231)
Unemployment Rate	0.180***(0.051)	0.180***(0.052)
287(g) Participant	-0.519(0.354)	-0.522(0.362)
Border State	-0.979(1.131)	-1.00(1.154)
Dream Act	0.719(0.386)	0.717(0.393)
Favors SB1070	0.022(0.026)	0.021(0.027)
State % Unauthorized	-0.330(0.170)	-0.332(0.173)
Constant	-14.88***(3.250)	-15.07***(3.275)
Observations	2226	2241
AIC	6.749	6.851

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2.2. Dropping All Observations 1 & 2 Standard Deviations Above Mean, Model 3.2

	Model 3.2: Dropping if >1 SD	**Model 3.2: Dropping if > 2 SD**
Independent Variables		
Private Correctional Facility	0.746**(0.278)	0.725**(0.272)
IGSA Contract	0.777***(0.139)	0.785***(0.133)
Budget per resident	0.0735***(0.016)	0.073***(0.016)
Conservatism	-0.003(0.004)	-0.004(0.004)
Foreign-born Population	0.133***(0.010)	0.134***(0.009)
Crime Rate	1.23e-05(2.34e-05)	1.21e-05(2.11e-05)
% Change in Latino Population	0.003***(0.0007)	0.003***(0.0007)
County Population (centered)	0.004***(0.0006)	0.004***(0.0005)

GDP Fruits & Vegetables	-0.024(0.119)	-0.036(0.118)
GDP Construction	0.107(0.078)	0.106(0.078)
Unemployment Rate	0.029(0.021)	0.027(0.021)
Median Household Income (centered)	0.001**(0.0004)	0.001**(0.0004)
287(g) Participant	0.014(0.084)	0.014(0.084)
Border State	0.276(0.142)	0.288*(0.143)
Dream Act	-0.076(0.100)	-0.071(0.100)
Favors SB1070	0.021***(0.005)	0.020***(0.005)
State % Unauthorized	0.153***(0.043)	0.147***(0.043)
Constant	-1.784***(0.480)	-1.697***(0.479)
Inflated Model		
Private Correctional Facility	-0.069(0.819)	-0.081(0.823)
IGSA Contract	-0.418(0.509)	-0.424(0.509)
Budget per resident	-0.247*(0.108)	-0.249*(0.109)
Conservatism	-0.008(0.012)	-0.009(0.012)
Foreign-born Population	-0.478***(0.113)	-0.478***(0.113)
Crime Rate	-0.001(0.001)	-0.001(0.001)
% Change in Latino Population	-0.001(0.0007)	-0.001(0.0006)
County Population (centered)	-0.157***(0.029)	-0.156***(0.029)
GDP Fruits & Vegetables	0.435(0.369)	0.433(0.369)
GDP Construction	0.477*(0.233)	0.475(0.232)
Unemployment Rate	0.133*(0.067)	0.131(0.068)
Median Household Income (centered)	-0.007**(0.002)	-0.006**(0.002)
287(g) Participant	-0.444(0.351)	-0.439(0.349)
Border State	-1.412*(0.617)	-1.419*(0.615)
Dream Act	1.072**(0.384)	1.079**(0.382)
Favors SB1070	-0.040(0.026)	-0.041(0.026)
State % Unauthorized	-0.755***(0.173)	-0.759***(0.172)
Constant	-11.74**(3.97)	-11.53**(3.956)
Observations	2226	2241
AIC	6.643	6.732

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

To further check the robustness of the findings, I log transformed the dependent variable, the number of deportations (replacing log(0) with 0), and then ran an OLS regression model. As Table A2.3 reveals, the directional relationships between our independent variables and the outcome remain unchanged. Furthermore, none of the key explanatory variables lose significance. Comparing the log-transformed version of Model 3.1 to Model 3.2, we see that once again Model 3.2 is a better fit. Both the R-squared and the adjusted R-squared report that Model 3.2 accounts for approximately 10 percent more of the variance in the dependent variable, indicating that the addition of the change in Latino population and median household income variables as well as the replacement of the Latino population variable with the foreign-born population were all constructive modifications.

Table A2.3. OLS Models of 3.1 & 3.2 with Logged Deportations as Outcome

	Model 3.1: Logged Deportations	**Model 3.2: Logged Deportations**
Independent Variables		
Private Correctional Facility	$0.489^{**}(0.161)$	$0.468^{***}(0.145)$
IGSA Contract	$0.777^{***}(0.102)$	$0.612^{***}(0.092)$
Budget per resident	$0.062^{***}(0.013)$	$0.035^{**}(0.011)$
Conservatism	$-0.018^{***}(0.002)$	$-0.014^{***}(0.002)$
Foreign-born Population		$0.133^{***}(0.007)$
Latino Population	$0.02^{***}(0.003)$	
Crime Rate	$0.00004^{***}(0.000008)$	$0.00003^{***}(0.000007)$
% Change in Latino Population		$0.002^{***}(0.0002)$
County Population (centered)	$0.001^{***}(0.0001)$	$0.0007^{***}(0.00009)$
GDP Fruits & Vegetables	0.104(0.086)	$-0.194^{**}(0.078)$
GDP Construction	$-0.394^{***}(0.058)$	$-0.154^{**}(0.053)$
Unemployment Rate	$-0.076^{***}(0.012)$	0.019(0.012)
Median Household Income (centered)		$0.003^{***}(0.003)$
287(g) Participant	-0.112(0.080)	0.025(0.072)
Border State	-0.253(0.132)	0.177(0.116)
Dream Act	$-0.394^{***}(0.080)$	$-0.288^{***}(0.071)$
Favors SB1070	$0.034^{***}(0.004)$	$0.029^{***}(0.004)$
State % Unauthorized	$0.329^{***}(0.031)$	$0.173^{***}(0.029)$
Years Activated=3	$0.803^{***}(0.158)$	$0.632^{***}(0.142)$
Years Activated=4	$1.234^{***}(0.154)$	$1.086^{***}(0.138)$
Years Activated=5	$2.380^{***}(0.164)$	$2.087^{***}(0.148)$
Years Activated=6	$3.028^{***}(0.236)$	$2.709^{***}(0.211)$
Years Activated=7	$2.835^{***}(0.409)$	$2.534^{***}(0.368)$
Constant	0.616(0.435)	$-0.901^{**}(0.404)$
Observations	2271	2271
R-Squared	0.5448	0.6336
Adj. R-Squared	0.5407	0.6300
AIC	3.461	3.246

Standard errors in parentheses
$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

2. Checking the Effects of Potentially Influential Observations

Following Garay et al. (2011) and Xie et al. (2008), I use generalized Cook's distance to explore the possibility that my results are a product of influential observations. The models in Table A2.4 use a cutoff of D>4/N= 0.002, which reveals 141 potentially influential observations, while the models in Table A2.5 use D>1 as a cutoff, leading to one suspect observation. In neither case do the directional effects or statistical significance of the main explanatory variables change.

Table A2.4. Models without Influential Observations (According to Generalized Cook's Distance)

	Model 3.1 **D>0.002**	**Model 3.2** **D>0.002**
Independent Variables		
Private Correctional Facility	0.372^{*}(0.147)	0.487^{*}(0.213)
IGSA Contract	0.456^{***}(0.0982)	0.529^{***}(0.106)
Budget per resident	0.0888^{***}(0.0233)	0.0372(0.0253)
Conservatism	-0.00746^{**}(0.00284)	-0.0125^{***}(0.00330)
Foreign-born Population		0.119^{***}(0.00844)
Latino Population	0.0382^{***}(0.00331)	
Crime Rate	0.00004^{*}(0.00002)	0.00003(0.00002)
% Change in Latino Population		0.00190^{**}(0.000598)
County Population (centered)	0.00485^{***}(0.000423)	0.00334^{***}(0.000407)
GDP Fruits & Vegetables	-0.0738(0.0928)	-0.204^{*}(0.0946)
GDP Construction	-0.209^{***}(0.0571)	0.00599(0.0682)
Unemployment Rate	-0.0459^{***}(0.0129)	0.0182(0.0192)
Median Household Income (centered)		0.00150^{***}(0.000348)
287(g) Participant	-0.0627(0.0737)	0.0351(0.0744)
Border State	0.169(0.108)	0.537^{***}(0.112)
Dream Act	-0.493^{***}(0.0728)	-0.326^{***}(0.0777)
Favors SB1070	0.0298^{***}(0.00413)	0.0223^{***}(0.00462)
State % Unauthorized	0.380^{***}(0.0304)	0.258^{***}(0.0352)
Constant	-0.338(0.371)	-1.089^{**}(0.419)
Inflated Model		
Private Correctional Facility	-0.552(1.967)	0.172(0.760)
IGSA Contract	-0.330(0.537)	-0.470(0.501)
Budget per resident	-0.0828(0.0889)	-0.273^{*}(0.125)
Conservatism	0.000803(0.0124)	-0.0139(0.0122)
Foreign-born Population		-0.478^{***}(0.111)
Latino Population	-0.904^{***}(0.253)	
Crime Rate	-0.000202(0.000454)	-0.000600(0.00125)
% Change in Latino Population		-0.002(0.0009)
County Population (centered)	-0.143^{***}(0.0246)	-0.153^{***}(0.0292)
GDP Fruits & Vegetables	0.343(0.368)	0.380(0.379)
GDP Construction	0.246(0.242)	0.442(0.234)

Unemployment Rate	0.151**(0.0493)	0.109(0.0630)
Median Household Income (centered)		-0.006**(0.002)
287(g) Participant	-0.583(0.356)	-0.392(0.339)
Border State	-0.735(1.078)	-1.202*(0.562)
Dream Act	0.609(0.385)	0.953*(0.374)
Favors SB1070	0.0325(0.0248)	-0.0410(0.0235)
State % Unauthorized	-0.204(0.164)	-0.667***(0.167)
Constant	-14.69***(3.143)	-10.74**(3.909)
Observations	2130	2130
AIC	6.361	6.291

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A2.5. Models without Influential Observations (According to Generalized Cook's Distance)

	Model 3.1 D>1	Model 3.2 D>1
Independent Variables		
Private Correctional Facility	0.684**(0.261)	0.664*(0.274)
IGSA Contract	0.628***(0.119)	0.728***(0.133)
Budget per resident	0.0835***(0.0220)	0.0672***(0.0158)
Conservatism	-0.00518(0.00435)	-0.0145**(0.00552)
Foreign-born Population		0.163***(0.0184)
Latino Population	0.0499***(0.00574)	
Crime Rate	0.00003(0.00002)	0.000009(0.00002)
% Change in Latino Population		0.00328***(0.0008)
County Population (centered)	0.00402***(0.000553)	0.00243***(0.0006)
GDP Fruits & Vegetables	-0.0127(0.127)	-0.179(0.120)
GDP Construction	-0.0149(0.0824)	0.231**(0.0870)
Unemployment Rate	-0.0439*(0.0183)	0.0296(0.0229)
Median Household Income (centered)		0.0017**(0.0005)
287(g) Participant	-0.0123(0.0925)	0.0598(0.0899)
Border State	-0.145(0.143)	0.379*(0.154)
Dream Act	-0.283**(0.0947)	-0.0804(0.101)
Favors SB1070	0.0241***(0.00585)	0.0154*(0.00615)
State % Unauthorized	0.283***(0.0407)	0.128**(0.0451)
Constant	-0.453(0.505)	-1.326*(0.532)
Inflated Model		
Private Correctional Facility	-0.821(1.784)	-0.145(0.937)
IGSA Contract	-0.395(0.503)	-0.466(0.526)
Budget per resident	-0.148*(0.0697)	-0.262*(0.116)
Conservatism	0.00158(0.0131)	-0.0146(0.0134)
Foreign-born Population		-0.480***(0.116)
Latino Population	-0.839***(0.252)	
Crime Rate	-0.000494(0.00128)	-0.00119(0.00151)
% Change in Latino Population		-0.00129(0.000725)
County Population (centered)	-0.150***(0.0265)	-0.160***(0.0314)
GDP Fruits & Vegetables	0.288(0.372)	0.378(0.395)
GDP Construction	0.401(0.239)	0.551*(0.247)

Unemployment Rate	0.179^{***}(0.0543)	0.134(0.0729)
Median Household Income (centered)		-0.00645^{**}(0.00227)
287(g) Participant	-0.502(0.382)	-0.408(0.366)
Border State	-1.106(1.272)	-1.483^{*}(0.675)
Dream Act	0.738(0.415)	1.156^{**}(0.401)
Favors SB1070	0.0169(0.0294)	-0.0484(0.0269)
State % Unauthorized	-0.342(0.181)	-0.814^{***}(0.180)
Constant	-15.06^{***}(3.469)	-11.41^{**}(4.219)
Observations	2270	2270
AIC	7.131	7.022

Robust standard errors in parentheses
$p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3. Assessing Standard Errors and Model Parameters

Although King and Roberts (2015) have noted that robust standard errors may hide bias, running the models with classical standard errors produces no substantive differences, negligible difference between the errors, and consistent AIC statistics. Furthermore, as others have pointed out (Cameron and Trivedi; Cantoni and Ronchetti 2001; Hilbe 2001; Mebane and Sekhon 2004), robust standard errors are an appropriate choice for ZINB models because they are able to overcome inconsistent and high levels of variance resulting from the heteroskedasticity characteristic of ZINB models. Indeed, Mebane and Sekhon (2004) go so far as to say that “nonrobust estimation should be avoided whenever possible” when dealing with overdispersed data (408). Based on the results in Tables A2.6 and A2.7 below, the classical standard errors seem to be slightly underestimated when compared to the robust standard errors, inflating the significance of the coefficients and illustrating the preference for a robust estimation method.

Table A2.6. Robust vs. Classical Standard Errors for Model 3.1

	Robust SEs	**Classical SEs**
Independent Variables		
Private Correctional Facility	0.604^{*}(0.274)	0.604^{***}(0.160)

IGSA Contract	0.616***(0.118)	0.617***(0.105)
Budget per resident	0.073*** (0.018)	0.0728***(0.0182)
Conservatism	-0.008(0.005)	-0.00827*(0.00329)
Foreign-born Population		
Latino Population	0.043***(0.007)	0.0427***(0.00345)
Crime	0.00003(0.00002)	0.0000324*(0.0000142)
% Change in Latino Population		
County Population (centered)	0.004***(0.0005)	0.00384***(0.000262)
GDP Fruits & Vegetables	-0.008(0.122)	-0.00848(0.106)
GDP Construction	0.067(0.099)	0.0669(0.0641)
Unemployment Rate	-0.044*(0.018)	-0.0444**(0.0161)
Median Household Income (centered)		
287(g) Participant	0.024(0.097)	0.0237(0.0915)
Border State	0.025(0.189)	0.0246(0.143)
Dream Act	-0.284**(0.095)	-0.284**(0.0897)
Favors SB1070	0.017*(0.008)	0.0167**(0.00513)
State % Unauthorized	0.304***(0.044)	0.304***(0.0370)
Constant	-0.413(0.566)	-0.131(0.456)
Inflated Model		
Private Correctional Facility	-0.954(1.848)	-0.954(1.663)
IGSA Contract	-0.389(0.514)	-0.389(0.560)
Law Enforcement Budget	-0.149*(0.070)	-0.149*(0.0747)
Conservatism	0.0004(0.013)	0.0004(0.0126)
Foreign-born Population		
Latino Population	-0.880***(0.249)	-0.880***(0.191)
Crime	-0.0006(0.001)	-0.000599(0.00108)
% Change in Latino Population		
County Population (centered)	-0.149***(0.027)	-0.150***(0.0239)
Fruits & Vegetables GDP	0.312(0.381)	0.312(0.352)
Construction GDP	0.435(0.245)	0.435(0.244)
Unemployment Rate	0.179***(0.056)	0.179***(0.0492)
Median Household Income (centered)		
287(g) Participant	-0.491(0.395)	-0.491(0.381)
Border State	-1.080(1.313)	-1.081(1.173)
Dream Act	0.7524(0.431)	0.755(0.390)
Percent Favors SB1070	0.011(0.031)	0.0112(0.0257)
State Percent Unauthorized	-0.342(0.186)	-0.342*(0.166)
Constant	-14.711***(3.630)	-14.71***(2.962)
Observations	2271	2271
AIC	7.191	7.191

$^{*}\ p < 0.05$, $^{**}\ p < 0.01$, $^{***}\ p < 0.001$

Table A2.7. Robust vs. Classical Standard Errors for Model 3.2

	Robust SEs	**Classical SEs**
Independent Variables		
Private Correctional Facility	0.588*(0.281)	0.588***(0.155)
IGSA Contract	0.712***(0.131)	0.712***(0.0998)
Budget per resident	0.063***(0.016)	0.0635***(0.0136)
Conservatism	-0.014**(0.005)	-0.0142***(0.00321)
Foreign-born Population	0.160***(0.017)	0.160***(0.00923)

Latino Population		
Crime	0.00001(0.00002)	0.0000132(0.0000120)
% Change in Latino Population	0.003***(0.0008)	0.00333***(0.000540)
County Population (centered)	0.0024***(0.0006)	0.00241***(0.000242)
GDP Fruits & Vegetables	-0.179(0.117)	-0.179(0.0960)
GDP Construction	0.250**(0.086)	0.250***(0.0633)
Unemployment Rate	0.018*(0.024)	0.0177(0.0178)
Median Household Income (centered)	0.001(0.0006)	0.001**(0.0004)
287(g) Participant	0.042(0.092)	0.0422(0.0857)
Border State	0.436**(0.162)	0.436**(0.141)
Dream Act	-0.085(0.099)	-0.0853(0.0863)
Favors SB1070	0.009(0.007)	0.00958*(0.00485)
State % Unauthorized	0.143**(0.046)	0.143***(0.0355)
Constant	-0.986*(0.586)	-0.986*(0.458)
Inflated Model		
Private Correctional Facility	-0.168(0.974)	-0.168(0.860)
IGSA Contract	-0.462(0.533)	-0.462(0.540)
Law Enforcement Budget	-0.261*(0.115)	-0.262*(0.114)
Conservatism	-0.013 (0.013)	-0.0134(0.0118)
Foreign-born Population	-0.472***(0.116)	-0.472***(0.115)
Latino Population		
Crime	-0.001(0.001)	-0.00132(0.00129)
% Change in Latino Population	-0.001(0.0007)	-0.00130(0.000796)
County Population (centered)	-0.164***(0.032)	-0.165***(0.0269)
Fruits & Vegetables GDP	0.365(0.399)	0.365(0.352)
Construction GDP	0.564*(0.250)	0.564*(0.236)
Unemployment Rate	0.132(0.072)	0.132*(0.0622)
Median Household Income (centered)	-0.007**(0.002)	-0.007**(0.002)
287(g) Participant	-0.429(0.372)	-0.429(0.359)
Border State	-1.477*(0.674)	-1.477*(0.633)
Dream Act	1.155**(0.409)	1.155**(0.376)
Percent Favors SB1070	-0.053*(0.027)	-0.0531*(0.0226)
State Percent Unauthorized	-0.815***(0.183)	-0.815***(0.162)
Constant	-11.624**(4.267)	11.62***(3.318)
Observations	2271	2271
AIC	7.077	7.077

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

4. Accounting for Possible Problems of Collinearity

The main source of collinearity from the model is from demographic and attitudinal variables related to immigration. Table A2.8 shows the most troublesome of these relationships, namely the correlation between the Latino and foreign-born population variables and the correlation between the percentage of state residents that are unauthorized and the percentage of voters favoring SB1070. The former was taken care of by not including the Latino population variable in Model 3.2. Surprisingly, there is relatively little correlation between Latino population growth and the foreign-born and Latino population variables. Furthermore, being a 287(g) participant or from a county that votes for the Republican presidential candidate is not strongly correlated with these variables.

Table A2.8. Correlation between Variables Related to Immigration

	Foreign-Born	Latino	% Change Latino	Unauthorized	Favors SB1070	Border State	287(g)
Foreign-born	1						
Latino	**0.6976**	1					
% Change Latino	-0.0641	-0.1567	1				
Unauthoriz d	0.4969	**0.5953**	-0.1072	1			
Favors SB1070	-0.332	**-0.5237**	0.1621	**-0.7046**	1		
Border Stat	0.3707	**0.6142**	-0.1754	**0.6600**	**-0.6385**	1	
287(g)	0.0680	0.0548	0.0797	0.1091	0.1278	-0.0912	1
Conservatis m	-0.1915	-0.0607	0.0383	0.0740	0.0457	0.1734	0.0129

To address the potential impact of collinearity on the results, I ran multiple models, excluding these variables one at a time and in groups. In general, this resulted in certain control variables gaining or losing statistical significance, but in only one case does the directional effect of a variable change: the percent favoring SB1070 becomes negative (but statistically insignificant) in model 3.1 when you exclude the unauthorized variable. I am happy to share these results, but did not include them here, as there is basically no change in the main effects of the models. Overall, the mean VIF is 1.78 for both Model 3.1 and 3.2, indicating that even in the full models collinearily is not inflating the variance of the coefficients much.

APPENDIX 3: CHAPTER 3

Table A3.1. Replication of Table 3.2 with No Year Fixed-Effects.

	(A3.1)	(A3.2)	(A3.3)
Republican control	1.152(0.798)	1.391(0.810)	0.378(0.854)
Democratic control	-0.245(0.912)	-0.209(0.924)	0.697(0.875)
Farm owners		-0.602***(0.160)	-0.746*(0.304)
Foreign-born		-0.0878**(0.0304)	-0.102*(0.0506)
Legislature Characteristics			
Legislature ideology			0.762*(0.348)
% Women			-3.303(7.462)
% Latino			1.405(5.343)
Prior Policy	0.903*(0.413)	0.744(0.423)	0.125(0.668)
State Characteristics			
Average citizen ideology			-0.0220(0.0363)
Population (logged)			0.441(0.314)
Median household income (logged)			-0.0768(0.155)
Bachelor's degree +			0.0679(0.193)
Unemployment rate			0.0754(0.151)
Constant	-3.487***(0.737)	-2.146**(0.769)	-7.314(6.229)
N	462	462	381
R^2	0.085	0.137	0.191

State clustered standard errors in parentheses
$^{\wedge} p < 0.1$, $^{*} p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

Table A3.2. Replication of Table 3.3 with No Year Fixed-Effects.

	Private Employers	Public Employers	Contractors
Republican control	0.809(1.434)	-0.226(1.520)	-0.752(0.951)
Democratic control	0.0198(1.139)	1.217(1.198)	0.543(0.840)
Farm owners	-0.802$^{\wedge}$(0.461)	-0.236(0.318)	-0.755*(0.311)
Foreign-born	-0.192*(0.0931)	-0.0245(0.0826)	-0.115$^{\wedge}$(0.0714)
Legislature Characteristics			
Legislature ideology	0.585(0.501)	1.191(0.616)	1.102**(0.421)
% Women	5.325(8.567)	-13.48(6.897)	-1.299(5.128)
% Latino	5.183(3.357)	-25.55(13.84)	0.0355(5.467)
Prior Policy	1.653*(0.804)	-2.857(1.657)	-1.190(0.975)
State Characteristics			
Average citizen ideology	-0.00657(0.0461)	-0.0703(0.0364)	-0.0364(0.0276)
Population (logged)	0.113(0.354)	2.058**(0.651)	0.771*(0.310)

Median household income (logged)	-0.140(0.177)	0.127(0.285)	-0.186(0.205)
Bachelor's degree +	-0.289(0.272)	0.722^{**}(0.238)	0.0372(0.165)
Unemployment rate	0.319(0.228)	0.0282(0.270)	-0.217(0.212)
Constant	-0.259(7.372)	-42.96^{**}(14.46)	-8.868(6.761)
N	381	381	419
R^2	0.300	0.288	0.178

State clustered standard errors in parentheses
$^{\wedge} p < 0.1$, $^{*} p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

Table A3.3. Replication of Table 3.4 with No Year Fixed-Effects.

	Republican Legislatures	Democratic Legislatures
Farm owners	-1.144^{*}(0.582)	0.186(0.395)
Foreign-born	-0.0783(0.0540)	-0.149^{**}(0.0531)
Legislature Characteristics		
Legislature ideology	0.737(0.423)	0.201(0.557)
% Women	0.454(9.576)	-9.435(6.963)
% Latino	-0.485(7.775)	-2.820(6.172)
Prior Policy	-0.0619(0.0697)	0.00755(0.0333)
State Characteristics	-0.204(0.926)	1.953(1.074)
Average citizen ideology		
Population (logged)	0.254(0.424)	1.636(1.181)
Median household income (logged)	0.0797(0.165)	-0.532(0.682)
Bachelor's degree +	-0.184(0.333)	0.0951(0.332)
Unemployment rate	0.115(0.184)	-0.755(0.712)
Constant	-0.420(9.328)	-17.30(13.08)
N	153	176
R^2	0.197	0.241

State clustered standard errors in parentheses
$^{*} p < 0.05$, $^{**} p < 0.01$, $^{***} p < 0.001$

To confirm the robustness of the constituency effects, Table A3.4 uses a simpler model to test the relationship between constituency size and passing E-Verify. I do this by looking at the states cross-sectionally rather than over time. Thus, we have 50 observations to determine whether constituency characteristics influenced whether an E-Verify bill was passed in general as well as whether these constituencies have different effects depending on the type of bill passed: those applying only to private employers, to contractors, or to public/state agencies. Corroborating our previous findings, the presence of both a large agricultural and a large foreign-born population significantly reduces the probability of a state passing general E-Verify legislation or bills specific to private employers and contractors.

Table A3.4. Effect of Constituencies on State Legislature Passing E-Verify Legislation (Cross-Sectional)

	Any Employer/ All Bills	Private Employers	Contractors	Public/State Employers
Farm owners	-0.901** (0.337)	-2.385*(1.140)	-0.616*(0.249)	-0.214(0.209)
Foreign-Born	-0.253*(0.105)	-1.230*(0.499)	-0.170*(0.077)	-0.026(0.056)
Constant	2.605*(1.167)	7.128*(3.362)	1.524(0.860)	-0.317(0.745)
N	50	50	50	50
R^2	0.202	0.546	0.113	0.01

Robust standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.5. The Influence of Salient Constituencies on Republican & Democratic Support for *All* E-Verify Bills

	(6.1)	(6.2)
	Republican Legislators	Democratic Legislators
Farm owners	4.052 (12.64)	2.402 (8.515)
Foreign-born	-2.821 (3.249)	-2.297* (1.133)
Legislator Characteristics		
Ideology	-0.0589 (0.225)	0.107 (0.150)
Female	-0.0433 (0.307)	-0.156 (0.165)
Latino	0 (.)	-0.622 (0.366)
Incumbent	0.0970 (0.235)	-0.139 (0.191)
Contested election	-0.143 (0.256)	0.0174 (0.183)
District Characteristics		
Average ideology	-0.0431 (0.532)	1.705*** (0.314)
Population (logged)	0.359 (0.268)	0.872*** (0.202)
Median household income (logged)	-0.0277 (0.659)	0.0859 (0.425)
Unemployment rate	-2.862*** (0.712)	-0.989** (0.359)
Bachelor's degree (+)	-0.0605 (0.203)	-0.354** (0.122)
N	1691	1486

Models use logistic regression with state-fixed effects and state-clustered standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3.6. Summary Statistics for Legislature-Level

Republican Control	500	0.438	0.497	0	1
Democratic Control	500	0.43	0.496	0	1
Farm owners	500	1.497	1.359	0.145	6.429
Foreign-born	500	7.867	6.278	0.050	27.305
Legislature ideology	419	0.075	1.303	-2.752	2.246
Percent women	500	0.238	0.069	0.088	0.42
Percent Latino	500	0.041	0.078	0.000	0.446
Citizen ideology	500	52.274	16.010	13.482	93.248
Population (logged)	500	15.163	1.010	13.150	17.483
Median household income (logged)	500	12.180	1.742	10.517	16.318
Unemployment rate	500	4.763	1.269	2.2	9.3
Bachelor's degree (+)	500	17.609	2.787	10	24.8

Table A3.7. Summary Statistics for Legislator-Level

Republican	4225	0.578	0.494	0	1
Farm owners	4188	0.775	1.14	0	11.549
Foreign-born	4173	6.795	6.734	0	49.667
Ideology	4158	0.266	0.856	-3.045	2.662
Female	4221	0.197	0.398	0	1
Latino	4225	0.019	0.137	0	1
Incumbent	4168	0.692	0.462	0	1
Contested election	4225	0.664	0.472	0	1
Average ideology	4215	0.069	0.313	-1.124	0.967
Population (logged)	4223	10.988	1.293	4.192	12.787
Median household income (logged)	4223	10.564	1.122	4.349	11.917
Unemployment rate	4223	8.323	3.12	2.9	30.2
Bachelor's degree (+)	4223	8.442	7.12	0	65.0

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CURRICULUM VITAE

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