Elective Enforcement: The Politics of Local Immigration Policing

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Abstract

The last two decades have seen a pronounced shift in the focus of American immigration law from patrolling the nation's border to policing its interior. This development has involved local law enforcement agents gaining powers previously held centrally and exclusively by the federal executive. Yet neither the payoffs to the president from this regime nor localities' incentives to participate in it are explained by the current scholarship on immigration or federalism. This paper develops a theoretical framework for analyzing intergovernmental policymaking in a federated system. An empirical test using the 287(g) program highlights the model's central trade-off. By deputizing local officers with the powers of Immigration and Customs Enforcement (ICE) agents, 287(g) induced a dramatic increase in immigration enforcement at almost no federal expense. But the localities that selected into the program wielded their newfound agency differently from their federal counterparts, shifting the focus of policing efforts from felonies to traffic offenses and misdemeanor drug possession. I further present evidence that these decisions were driven by electoral politics, in particular accountability to nativist constituencies.

Keywords: immigration, federalism, local political economy, law enforcement, 287(g)

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1 Introduction

The last two decades have seen a pronounced shift in the implementation of American immigration law. Whereas for most of its history, U.S. immigration policy operated almost exclusively at the nation's territorial boundaries, controlling population flows at their initial point of entry, more recently the focus has turned to policing the nation's interior (Provine et al. 2016; Stuesse and Coleman 2014). This development has involved a high degree of intergovernmental cooperation: lacking the requisite internal capacity, federal authorities rely on support from subnational agents in identifying, apprehending, and deporting undocumented residents from the country.

Why did the federal government choose to incorporate local actors into the immigration policymaking process, and how did it induce them to participate in this new regime? The answers do not lie in either financial incentives or national legislation—the most frequently used and studied levers of vertical influence. Instead, the federal executive turned to a novel strategy: selectively endowing subnational actors with powers it had previously held centrally and exclusively in its own hands. Through a series of federal initiatives, specific rights such as the ability to police immigration status were extended downward on a voluntary basis, to be taken advantage of at localities' own discretion, and at their own expense. The result has been an emergent set of strategic interactions between local and federal policymakers that has yet to receive much scholarly attention. But understanding these dynamics is of critical importance, not only for explaining outcomes in the complex domain of immigration, but for illuminating recent developments in intergovernmental conflict and cooperation more broadly.

This paper proposes a new analytical framework for studying collaborative policymaking across multiple tiers of government in a federated system. I formally show that when subnational agents are empowered to make costly investments in policy outcomes, high demanders for the policy are the ones who will voluntarily contribute. If the federal government wants to induce more policy production, it can benefit from delegating power to these preference outliers. However, this strategy comes with an important trade-off: the more local agents are willing to contribute, the more extreme their preferences relative to the central authority, and the more policy will shift away from the federal ideal point. I illustrate this with an empirical analysis of the 287(g) program, which deputized local law enforcement officers with the powers of federal Immigration

and Customs Enforcement (ICE) agents. Consistent with theoretical expectations, I find that the counties that participated were high demanders, as evidenced by a record of escalating immigration enforcement efforts in the year before they joined. Nevertheless, using a matching with difference-in-differences design, I recover a dramatic causal effect of first-time 287(g) entry on the volume of participating jurisdictions' immigration enforcement activity. I also show that, when placed in these preference outliers' hands, policy outcomes moved away from national priorities: whereas the focus of immigration policing under exclusive federal control had been on felony crimes, localities turned their attention to misdemeanor violations, namely traffic offenses and drug possession.

My work contributes to the literatures on immigration, federalism, and local politics in several ways. First, it supplies a theoretical framework that reconciles the somewhat puzzling coexistence of two central themes of the recent immigration literature: on the one hand, that local authorities have gained a significant degree of agency in the enforcement of immigration law, and on the other hand that the federal executive's powers over local policing have similarly expanded. Rather than examining these trends in isolation, my theory takes a broader view of the strategic interactions between the two tiers of government. I propose that intergovernmental cooperation in immigration enforcement can be understood as a principal-agent relationship, in which the federal principal seeks to outsource policymaking efforts to subnational agents who are willing to undertake the task, but localities have their own set of preferences that may diverge from the president's. In section 2, I briefly review the recent scholarship on immigration federalism, and describe how insights from the bureaucratic delegation literature may illuminate the dynamics documented therein. I also consider how traditional models of delegation may fail to generalize to this context, focusing on the role of electoral politics in driving the agent's behavior. In so doing, this paper joins a small but growing number of recent studies that have found evidence of democratic accountability in local governance, contrary to a much larger body of earlier work that questioned its existence.

Section 3 presents the formal model and closely examines an equilibrium in which both the president and a subset of jurisdictions contribute toward policy production. The model illustrates two mechanisms simultaneously at work in this equilibrium: a *selection effect* and a *treatment effect*. First, it predicts that the localities that will voluntarily participate are the ones with the highest demand for the policy. Second, as a result of their participation, total policy production in those jurisdictions (from both local and federal sources) will be higher than what would have been

realized under a regime of central control, in which the federal government unilaterally chooses a policy for all jurisdictions and bears the entire cost. Finally, I derive the key condition under which the president prefers this regime to central control: the ideal points of the high-demanding jurisdictions must be somewhat higher than his own, but not so high as to push policy outcomes out to the extreme.

To assess the validity of the theory, I closely examine a real-world policy innovation—the federal 287(g) initiative—that corresponds to this equilibrium. Section 4 describes the program and maps its essential features to the assumptions of the model. Then, I design an empirical test around three questions that evaluate its predictions. First, did selection into the program operate according to theoretical expectations—that is, did the counties that participated reveal immigration enforcement preferences that were somewhat, but not dramatically, higher than the president's? Second, what was the causal effect of the program on localities' contributions? I expect that the volume of immigration enforcement activity in participating counties increased, and that this was driven disproportionately by local rather than federal efforts. Third, did participants' outlying preferences translate into post-treatment divergence from federal priorities? If the theory is correct, I expect local agents to wield their authority differently than the federal government, and for policy outcomes to reflect those choices.

Section 5 details the empirical strategies I use to answer these questions, and presents results. Throughout this analysis, my primary outcome of interest is immigration detainers, which are formal requests for jails to hold people who have been arrested for any offense in custody beyond their scheduled release time in order to investigate their immigration status. In 287(g) counties, both federal and local authorities are empowered to initiate detainers, so I conceive of them as substitutable inputs into the production of restrictionist immigration policy. My data contains the complete universe of detainers issued from 2002 to 2015—over two million detainer-level observations—coded by location and date, along with information about the individual, the criminal charge and/or conviction, and the ensuing legal process. First, I show that program participants were indeed high demanders, as illustrated by their pre-treatment detainer issuance rate. Next, I use a matching with difference-in-differences design to estimate the causal effect of first-time 287(g) entry on the number of detainers issued and honored in a jurisdiction. Not only do I find a dramatic increase in this outcome under 287(g)—an effect as high as 671 more detainers than a counterfactual of no

participation—but I can plausibly trace this uptick to local rather than federal activity. Finally, I disaggregate detainers by the types of crimes for which they were issued in order to understand how 287(g) entry changed law enforcement priorities. I find that local authorities placed a much greater emphasis on misdemeanor crime than their federal counterparts, devoting the largest share of their resources to traffic violations and drug possession.

Finally, Section 6 addresses how electoral motivations drove local officials' behavior, including both their likelihood of entering the program and their subsequent use of 287(g) authority. I find that joining the program carried with it at least the perception of electoral reward: the odds of 287(g) signing more than doubled in the years when the county sheriff stood for election. Moreover, the counties with the most dramatic treatment effects tended to have 287(g)s signed by elected sheriffs; with one exception, those with contracts signed by appointed police chiefs did not substantially increase their volume of immigration enforcement. I conclude with a discussion of some of the normative implications of my findings for intergovernmental relations, criminal justice, and democratic representation.

2 Contributions to the Literature

Two themes have emerged from the recent literature on "immigration federalism" (Motomura 1999), which studies the "multilayered interjurisdictional patchwork of... overlapping and potentially conflicting authority" over immigration policy (Varsanyi et al. 2012). The first is that the importance of subnational governments is rising. Increasingly, states are "implementing policy innovations and controls amidst inertia at the national level" (Filindra and Tichenor 2008). National legislation has devolved the enforcement of federal mandates to local actors (Coleman 2012, 2007; Waslin 2007; Thacher 2005), and grassroots policies such as identification laws and sanctuary ordinances have been enacted by city councils and state legislatures (Ramakrishnan and Wong 2010; Varsanyi 2010; Chavez and Provine 2009; Ridgley 2008). At the same time, a separate strand of the literature is concerned with the growing powers of the president (Gulasekaram and Ramakrishnan 2016). Studies in this vein tend to interpret localities' agency as a "force multiplier" on federal efforts rather than being in service of their own independent goals (Provine et al. 2016; Kobach 2005–2006; Hethmon 2004), and ethnographic accounts contribute to the view that federal policies

have fundamentally reshaped local enforcement culture and practices (Armenta 2017).

While these claims are not contradictory per se, their coexistence demands an explanation. I propose that intergovernmental cooperation in immigration enforcement is best understood as a principal-agent relationship, in which the federal principal seeks to enlist local agents in carrying out its policy goals. A large research agenda on bureaucratic delegation has developed the tools to analyze precisely this class of strategic interactions (see Huber and Shipan (2008) for a review), with a great many transferable insights to federal politics. My theoretical framework imports two technologies from this literature. First, the central conflict between principal and agent hinges on their preferences in an ideological space, with both parties wanting to minimize the distance between their ideal points and realized policy. Second, the principal's problem is whether to delegate or maintain central control, and her payoffs from delegation increase when the agent is more ideologically aligned.[1] Building on these basic concepts, my model explains why we would see an increase in local policy production that simultaneously promotes the agenda of the president.

Of course, local politicians are not bureaucrats. Thus, it is also important to consider the ways in which traditional theories of delegation must be adapted to fit the intergovernmental context. One key divergence lies in what drives the motivations of the agents: whereas for bureaucrats, scholars tend to focus on ideology and career concerns (Gailmard and Patty 2007), any study of local politicians must at least consider the role of electoral politics. Until recent empirical advances in the measurement of local public opinion (Tausanovitch and Warshaw 2013), there was a widespread consensus that local governance is not characterized by a great deal of democratic responsiveness: municipalities have their hands tied by competition with neighboring jurisdictions (Peterson 1981) and constraints imposed by higher levels of government, including state legislative politics (Gamm and Kousser 2013; Brooks and Phillips 2010; Mullins and Wallin 2004; Frug 1979; Desmond 1955) and federal guidelines (Advisory Commission on Intergovernmental Relations 1984; Derthick 1970). Although it does not entirely negate the presence of well-known barriers to local representation, my work joins a small but growing number of recent studies amassing evidence that there is nonetheless a local electoral connection, both in immigration policy specifically (Provine et al. 2016; Ramakrishnan and Wong 2010) and across a range of other political dimensions (de Benedictis-Kessner and

[1] Another key feature of delegation problems—the informational asymmetry between the politician and the bureaucrat—is not as relevant for the case at hand, but may be useful in future work.

Warshaw 2016; Einstein and Kogan 2016; Tausanovitch and Warshaw 2014; Palus 2010; Arceneaux and Stein 2006; Stein, Ulbig and Post 2005; Howell and Perry 2004).

3 A Theory of Collaborative Policymaking in a Federated System

In this section, I model a collaborative policymaking process in which federal and subnational governments may each contribute policy inputs, and an output is generated by a production function. Each of the governments has single-peaked preferences defined over the outputs. I am interested in how the outcomes and utilities realized under this regime compare to an alternative regime of centralized control, in which federal authorities unilaterally set policy in all jurisdictions and bear all the associated costs. The model yields two key insights. First, when subnational governments are empowered to participate in costly policymaking, there is both a *selection effect* and a *treatment effect*: those that voluntarily contribute in equilibrium are the ones with the highest demand for the policy, and total policy production in those jurisdictions (from both local and federal sources) is higher than what would have been realized under a regime of centralized control. Second, the federal government can benefit from joint policymaking when these high demanders want *more policy production* than the central authority, but are still *sufficiently proximate* to its ideal point.

The model is a modified public goods game in which the players are jurisdictions $j \in \{1, ..., J\}$ and a federal government f. Each subnational government inherits the preferences of the median voter in its constituency, while the federal government has its own exogenous ideal point. All players simultaneously make a contribution $x_f \in \mathbb{R}_+$ that represents a commitment of resources toward the implementation of a policy, or else they opt out and contribute nothing. Resource investment monotonically shifts policy in a certain ideological direction: specifically, costly contributions toward law enforcement correspond to more restrictionist immigration outcomes. The federal government makes one investment choice that applies uniformly to all jurisdictions; it cannot differentially target resources to specific regions.

The setup diverges from a public goods game because preferences over policy outputs are single-peaked rather than nonsatiable: for both federal and subnational governments, there is negative utility from overprovision even absent costs. Each actor has an ideal point $\bar{y}_f$ or $\bar{y}_j \in \mathbb{R}_+$. There are no spillovers, so subnational governments care only about their own policy outcomes, while the

federal government wants to minimize the sum of squared deviations from its ideal point across all jurisdictions. To reduce the complexity of the problem, I fix initial conditions, costs, and production functions to be the same across jurisdictions, allowing only their ideal points to vary. I further assume linear costs and an additive production function in which the inputs are pure substitutes: $y_j(x_j, x_f) = x_j + x_f$.

The basic insights of the model can be derived with two jurisdictions, L and H, with $\bar{y}_L < \bar{y}_f < \bar{y}_H$. Under these assumptions, the utilities are given by:

$$u_j(x_j) = -\frac{1}{2}\left(x_j + x_f - \bar{y}_j\right)^2 - cx_j \tag{1}$$

$$u_f(x_f) = -\frac{1}{2}\sum_{j\in\{L,H\}}\left(x_j + x_f - \bar{y}_f\right)^2 - 2cx_f \tag{2}$$

Following from the first-order conditions, the federal government's utility-maximizing contribution, x_f^*, equates marginal benefit with marginal cost:

$$x_f^*(x_L, x_H) = \max\left(0, \bar{y}_f - \frac{x_L + x_H}{2} - c\right) \tag{3}$$

This is an intuitive result: the federal government supplies the difference between the average input across all jurisdictions and its own ideal point, net of costs. Similarly, each jurisdiction's best response is defined by:

$$x_j^*(x_f) = \max\left(0, \bar{y}_j - x_f - c\right) \tag{4}$$

Proposition 1: There is no generic equilibrium in which all three players contribute.[2]

The proof is presented in the appendix, but the underlying logic is the same as that of a public goods game: the lowest demander free-rides on the others' efforts.

Proposition 2: In any equilibrium in which the federal government and one jurisdiction contribute,

[2]There is a knife-edge equilibrium in which all three players contribute under the condition that $\bar{y}_f$ equals exactly $(\bar{y}_H + \bar{y}_L)/2$.

that jurisdiction *must be* the high demander. Specifically, in this equilibrium, contributions are:

$$\begin{aligned} x_f^* &= 2\bar{y}_f - \bar{y}_H - c \\ x_L^* &= 0 \\ x_H^* &= 2(\bar{y}_H - \bar{y}_f) \end{aligned} \tag{5}$$

And this equilibrium holds under the following conditions:

$$\begin{aligned} &\text{(i) } \frac{\bar{y}_L + \bar{y}_H}{2} < \bar{y}_f < \bar{y}_H \\ &\text{(ii) } \bar{y}_H < 2\bar{y}_f - c \end{aligned} \tag{6}$$

The proof of Proposition 2 lies in the fact that deriving these conditions did not assume any rank-ordering of preferences; rather, Condition (i) emerged endogenously, and it can only hold if $\bar{y}_H$, the ideal point of the (arbitrarily) assumed contributor, is greater than $\bar{y}_L$, the ideal point of the noncontributor.

Intuitively, this equilibrium rests on there being a high-demanding jurisdiction that wants more output—but not too much more—than the federal government; if $\bar{y}_H$ is significantly higher than $\bar{y}_f$, then only the high demander will contribute. Derivations of these and all other equilibria are provided in the appendix. However, from this point forward, I focus on the one stated in Proposition 2, since it is most relevant for the empirical case: relative to the Obama Administration, there was not a large enough set of jurisdictions with sufficiently high enforcement preferences to obviate the need for federal efforts, and in practice the federal government continued to make costly contributions after localities gained authority.

How does output under joint policymaking compare to the counterfactual of centralized control? Interestingly, granting localities authority does not change aggregate output under the assumed production process, but introduces variance: output increases in the high-demanding jurisdiction and decreases in the low-demanding jurisdiction, with some of the burden of provision shifting from the federal government toward the high demander. Without the possibility of local participation, the federal government contributes:

$$x_f^{c*} = \max(0, \bar{y}_f - c) \tag{7}$$

With sufficiently low costs, this yields the output:

$$\begin{aligned} y_L^{c*} &= y_H^{c*} = x_f^{c*} = \bar{y}_f - c \\ y^{c*} &= y_L^{c*} + y_H^{c*} = 2\bar{y}_f - 2c \end{aligned} \tag{8}$$

By comparison, under the joint policymaking regime, equilibrium output is:

$$\begin{aligned} y_L^{*} &= x_f^{*} = 2\bar{y}_f - \bar{y}_H - c \\ y_H^{*} &= x_f^{*} + x_H^{*} = \bar{y}_H - c \\ y^{*} &= y_L^{*} + y_H^{*} = 2\bar{y}_f - 2c \end{aligned} \tag{9}$$

Note that both $y_L^{c*} > y_L^*$ and $y_H^{c*} < y_H^*$ as long as $\bar{y}_H > \bar{y}_f$, which is true by assumption, but that y^{c*} and y^* work out to be the same quantity.

Finally, I compare the federal government's utility under the two regimes. Under centralized control, it has a total utility of:

$$u_f^c(x_f^{c*}) = -(x_f^{c*} - \bar{y}_f)^2 - 2cx_f^{c*} = c^2 - 2\bar{y}_f c \tag{10}$$

assuming again that $\bar{y}_f > c$. Under the joint policymaking equilibrium, its utility is:

$$u_f(x_f^*) = -\frac{1}{2}\left((x_f^* + x_H^* - \bar{y}_f)^2 + (x_f^* - \bar{y}_f)^2\right) - 2cx_f^* = -(\bar{y}_H - \bar{y}_f) + c^2 - 2c\bar{y}_H - 4c\bar{y}_f \tag{11}$$

Comparing these utilities, I find that $u_f > u_f^c$ when $\bar{y}_H - \bar{y}_f < 2c$. Put simply, the federal government is better off under the joint-policymaking regime when the high-demanding jurisdiction's preferences are above, but sufficiently close to, its own.[3]

4 287(g) as a Case of Intergovernmental Policymaking

As a test of my theory, I analyze the 287(g) program. This section provides some background on 287(g) and explains how it aligns with the structure and assumptions of the model. Named after the clause of the Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) of 1996 that

[3]See Appendix Figure A.2 for an illustration of how relative welfare changes as a function of $\bar{y}_f$ across the various equilibria of the model.

established it, 287(g) was loosely conceived as a way for federal authorities to selectively delegate authority to states and localities. At its inception, the program was intentionally open-ended: the specific rights and responsibilities afforded to participating law enforcement agencies (LEAs) were to be negotiated with the Director of ICE on a case-by-case basis and codified in a Memorandum of Understanding (MOA).

However, this brief clause in IIRIRA went largely unnoticed for many years; no subnational agency took advantage of the opportunity to gain federal powers under 287(g) until 2002, and there was not significant local participation in the program until 2007. Two factors explain its eventual rise. The first is the attacks on September 11th, 2001, when the Bush Administration turned the focus of its immigration policies to detecting terrorist activities and began enlisting states and localities as partners in its national security efforts.[4] Second, and relatedly, the program began to take concrete shape as a larger ecosystem of crime and immigration practices developed around it, all with the original intent of combating international terrorism.

The Bush Administration put in place the infrastructure for intergovernmental cooperation in these efforts through the Secure Communities program, which extended preexisting information-sharing capabilities that had been used for criminal investigation to the realm of immigration enforcement. Previously, when a person was booked into a local jail, his or her fingerprints would enter a biometric database of criminal records maintained by and shared with the FBI. Under Secure Communities, this information would simultaneously be shared with ICE. The fingerprints would be matched to a federal database that maintains records of all immigration applicants, allowing ICE to verify the individual's legal status. Upon failing to do so, ICE could choose to issue a detainer, or a formal request for the LEA to hold the individual for up to 48 hours beyond his or her scheduled release date so that it could investigate the matter further, take custody of the offender, and initiate deportation proceedings. Though participation in Secure Communities was voluntary when the program was introduced in 2008, the Obama Administration made it a priority to phase in biometric data sharing capacity in every LEA across the nation, a goal that was achieved in January 2013.

Developing contemporaneously with the spread of Secure Communities, partnerships under the

[4] https://www.migrationpolicy.org/article/post-911-policies-dramatically-alter-us-immigration-landscape

287(g) program coalesced around two models: a "jail model" and a "task force model." The former deputized local officers who staffed the jails with ICE authority, allowing them to initiate detainers, investigate and issue warrants for immigration violations, and start deportation proceedings for anyone who was booked into custody for any reason. The latter went one step further, giving local officers the power to check legal status on the streets and in the course of their regular policing, and to arrest people for suspected immigration violations absent any criminal activity.[5]

It is useful to explicitly state how the context of the 287(g) program maps onto the formal model presented in Section 3. I conceive of immigration policing, which may include activities such as checking identification or issuing detainers, as the costly input x, and control over the number of undocumented immigrants in a community as the policy output y. Since y is increasing in x in the formal model, one should think of it as a measure of a community's restrictionism, reaching zero undocumented immigrants at its maximum. Similarly, the ideal points can be thought of as tolerance toward undocumented immigrants, which is decreasing as $\bar{y}$ rises. Detainers—the outcome of interest in my empirical analysis—are a particularly good example of substitutable inputs, since the exact same activity can be undertaken by federal or local ICE-deputized authorities, and an increase in inputs from either side pushes policy outputs upward.[6]

I focus my attention on the Obama Administration, since 287(g) and Secure Communities only took off at the tail end of the Bush presidency and at the time of writing, data from the Trump Administration are not fully available.[7] Where did Obama's immigration preferences fall relative to the national distribution? As he made clear through his speeches, executive orders, and dealings with Congress, he favored an aggressive immigration policy that included increasing the number of border control agents to stem immigration inflows and enhancing penalties against illegal border crossers by channeling them into formal deportation proceedings. At the same time, he emphasized that the targets of his enforcement efforts were recent border crossers and criminals—"felons, not

[5] `https://www.americanimmigrationcouncil.org/research/287g-program-immigration`

[6] However, there is an argument to be made that they are also complements, particularly in the case of the task force model: federal authorities have a larger pool of individuals for whom they might issue detainers when local agents bring more people of uncertain immigration status into the jails, and ICE-issued detainers ultimately require the cooperation of local agencies to have any impact, which is why sanctuary cities refuse to honor them. In follow-up work, I develop an extension of the model that incorporates complementarities into the production function.

[7] In follow-up work, I compare the immigration enforcement preferences of counties that joined under Obama to those that joined under Trump to see whether LEAs came into alignment with the president when there was a change in administrations.

families"—and proposed a pathway to legal status for long-time unauthorized residents.[8] Thus, while the president was without question a high demander for immigration enforcement, his position was not at the most extreme end of the ideological spectrum.

It is also clear that most voluntary participants in Secure Communities and 287(g) had enforcement preferences that were at least as high, and often higher than, the president's. Since deportation proceedings are conducted independently of the criminal process, Secure Communities offered a novel opportunity for localities interested in channeling their law enforcement resources toward policing immigration. Among early adopters like Maricopa County, Arizona and Harris County, Texas, a common mode of operation was to draw as many potentially undocumented people into the jails as possible through traffic stops, vehicle searches, and identification checks, even if there would not ultimately be cause to charge them with any criminal wrongdoing (Armenta 2017). 287(g) participation interacted with Secure Communities to make these policing efforts even more effective: whereas federal agents were selective in issuing detainers, focusing primarily on suspected terrorists and felons, local authorities deputized with the same powers could detain and investigate everyone who was brought into their jails.

As the preceding discussion highlights, a one-dimensional ideological space may fail to accommodate complex real-world interactions among multiple policy areas—in this case, between crime and immigration. Quite often, the tools that decisionmakers have at their disposal for implementing one set of policies are inextricably linked to other domains, so these tangential preferences also matter. Under the Bush Administration, an intense preference for national security activated a higher demand for immigration enforcement, and by the same token, authorities in rural Arizona who had little first-order concern for international terrorism cooperated with federal goals out of a primary interest in immigration. Further exploration of these interactions is a fruitful avenue for future work. For the present analysis, however, I conceive of the actors' preferences as fundamentally about immigration, but filtered through the criminal justice system: whereas the highest demanders would expend their resources removing everyone who is in the country illegally, lower demanders would choose to remove only those who entered illegally and also committed crimes.

The subsequent history of the 287(g) program confirms that many of the early participants

[8] https://www.washingtonpost.com/politics/transcript-obamas-immigration-speech/2014/11/20/14ba8042-7117-11e4-893f-86bd390a3340_story.html?utm_term=.fb0207b331b9

were higher demanders for enforcement than the president—so much so that the federal executive eventually took measures to regain control.[9] In 2009, the Government Accountability Office conducted an investigation of the program and concluded that participants were using their authority to process individuals for minor crimes such as speeding, contrary to the original intent of the program.[10] In the same year, amidst allegations of racial profiling and civil rights violations, Secretary of Homeland Security Janet Napolitano voided all existing 287(g) agreements and created a new, standardized MOA that more clearly circumscribed LEAs' rights and responsibilities, and eliminated the controversial task force model.[11] The Department of Justice (DoJ) conducted an investigation of Maricopa County Sheriff Joe Arpaio and concluded that he "engage[d] in a pattern or practice of unconstitutional policing" including racial profiling of Latinos, unlawful stops, detentions, and arrests, and operating jails in a discriminatory manner,[12] and Maricopa's 287(g) authority was subsequently rescinded. A separate DoJ investigation of Sheriff Terry Johnson of Alamance County, North Carolina also found his department's policing practices unconstitutional, and its 287(g) contract was similarly revoked.[13]

Indeed, at any given time, the extent to which the federal executive has taken advantage of the infrastructure established by Secure Communities and 287(g) has been a strategic choice determined by the respective preferences of high-demanding jurisdictions and the president. Facing intensifying pressure from critics, and becoming increasingly convinced that the benefits of downward delegation were being outweighed by costly monitoring—or else by policy outcomes shifting far to the extreme—the Obama Administration ultimately rolled back both programs. In November 2014, the Department of Homeland Security issued a memo suspending Secure Communities. Though biometric data sharing capacity between ICE and LEAs was now firmly in place and would not be dismantled, federal authorities would no longer encourage localities to use this infrastructure, and ICE would no longer issue detainers except for the highest level felonies. Similarly, 287(g) signings slowed down, with only prior participants renewing their contracts but very few new jurisdictions

[9] This is consistent with the parameter space of the model where the devolved regime no longer makes the federal government better off than central control—namely, where the contributing jurisdiction's ideal point is higher than the federal government's ideal point by more than twice the marginal cost.

[10] See GAO report, "Better Controls Needed over Program Authorizing State and Local Enforcement of Federal Immigration Laws," available at `https://www.gao.gov/new.items/d09109.pdf`.

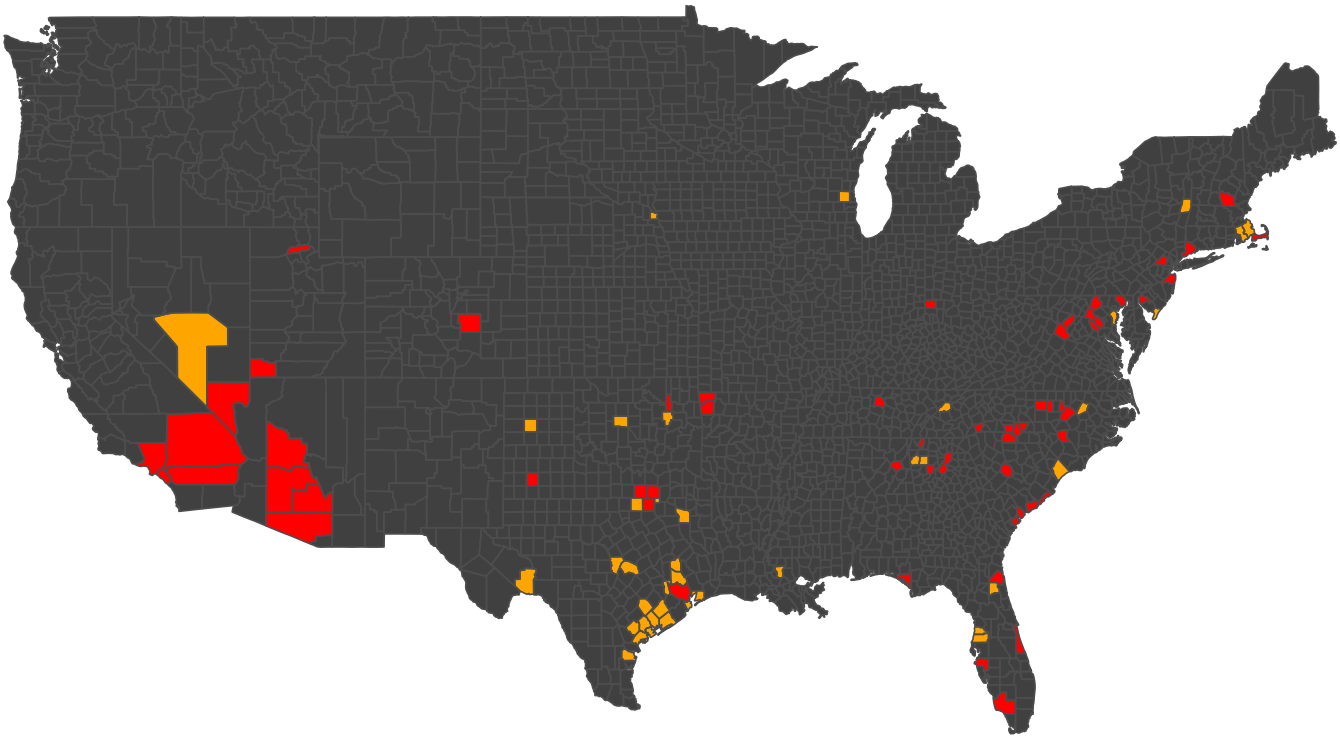
[11] `https://www.dhs.gov/news/2009/07/10/secretary-announces-new-agreement-state-and-local-immigration-enforcement`

[12] `https://www.justice.gov/sites/default/files/crt/legacy/2011/12/15/mcso_findletter_12-15-11.pdf`

[13] `https://www.justice.gov/iso/opa/resources/171201291812462488198.pdf`

entering the program between 2010 and 2016 (see Appendix Figure C.1 for a timeline of 287(g) signings). When Donald Trump assumed the presidency, he saw these dormant programs as an opportunity to move immigration policy back in a restrictionist direction. His 2017 executive order, "Enhancing Public Safety in the Interior of the United States,"[14] reinstated Secure Communities and encouraged renewed local cooperation with federal immigration enforcement through 287(g). Since he took office, 42 new jurisdictions have signed MOAs; as Figure 1 shows, this is a dramatic increase over the 60 jurisdictions that had ever previously participated in the program.

Figure 1: Counties that Signed 287(g) Agreements, 2005-2018



Notes: The 60 counties that had a 287(g) agreement in place at any point from 2005 to 2016 are shown in red. 42 additional counties that signed a 287(g) agreement for the first time in 2017 or 2018 are shown in orange.

5 Empirical Analysis

In this section, I test whether the extension of federal authority to LEAs operated according to theoretical expectations in the 287(g) context. Specifically, I ask three questions. First, where did the

[14] Available at `https://www.whitehouse.gov/presidential-actions/executive-order-enhancing-public-safety-interior-united-states/`.

counties that selected into the program fall in the national distribution of immigration enforcement preferences? Second, how did extension of authority affect policy in those jurisdictions, compared to a counterfactual in which the federal government maintained sole control? And third, how did the preferences of these local decisionmakers condition their use of their newfound authority—that is, how did the focus of immigration policing change when they took the reins?

5.1 Data and Measurement

I operationalize immigration enforcement using the number and type of detainers that were issued in a jurisdiction and not denied by the LEA. Through the Transactional Records Access Clearinghouse (TRAC) at Syracuse University,[15] I have obtained the entire universe of detainers issued from 2002 through 2015, observed at the detainer level and coded by location and date.[16] I measure enforcement volume using counts of detainers not denied in a county, aggregated to the month and year. Additional variables allow me to measure law enforcement priorities: for each detainer, I can observe whether there was ultimately a criminal charge and/or conviction, as well as the highest level crime, if any, for which the individual was convicted. Appendix Figure C.2 illustrates trends in the raw detainer data over time, with the total monthly number of detainers issued in the U.S. plotted on the y-axis.

To adjust these counts by the "supply" of potential detainers, I require county-level estimates of the undocumented immigrant population. Since no reliable measure of this variable exists, I construct an approximation in the most accurate way that available data sources will allow. I obtain state-level estimates of the undocumented immigrant population from Pew,[17] which I combine with Census ACS estimates of the total number of foreign-born to compute the proportion of the immigrant population that is undocumented in every state/year. I then multiply this proportion by the county-level counts of foreign-born people from the ACS. This measure is imperfect to the

[15] TRAC is an organization of researchers and lawyers whose mission is to collect records on the day-to-day operations of the federal government in order to increase public accountability. They obtain most of their data by filing requests under the Freedom of Information Act (FOIA). When those requests are denied, they may sue the relevant agencies for access, also under FOIA provisions; see, for example, Long et al v. Immigration and Customs Enforcement (2017).

[16] There are additional records from 2016, but they are incomplete: I no longer have certain key variables, nor am I confident that the dataset captures all of the detainers that were issued in that year. TRAC is engaged in ongoing litigation with the Department of Homeland Security to obtain complete records, but for now I exclude 2016 from my analysis.

[17] Estimates are available for the years 2000, 2007, 2009, 2012, and 2014, and I use linear interpolation to fill in the missing years. Source: `http://www.pewhispanic.org/2016/09/20/appendix-b-additional-tables-4/`.

extent that there is within-state heterogeneity in the share of all immigrants that is undocumented; however, as I show after I present my main results, none of the findings hinge on the particular measure of the immigrant population that I use.

I collect data on the treatment—287(g) participation—from ICE, which maintains a record of all past and present MOAs online.[18] From these documents, I record the calendar date of every signing, the LEA that signed the agreement (usually, but not always, the sheriff's office), and whether it was the county's first signing or a renewal of a preexisting agreement. Finally, I collect a number of county-level demographic and political covariates to use as statistical controls. My county population estimates come from the Census, and Democratic vote share in the last presidential election comes from Leip.[19] I additionally track when each state entered Secure Communities as well as whether every county or a smaller jurisdiction within it had a sanctuary ordinance in place in every year of my time series.[20] As detailed in Section 3 above, states developed the capacity to share biometric information with the federal government at different times in the lead-up to January 2013, when Secure Communities became fully operational across the country. The Urban Institute's State Immigration Policy Resource documents the year of entry into the program going back to 2008, the first year the opportunity was available.[21]

5.2 Constructing the Counterfactual

With repeated observations over a time series before and after treatment, it is possible in principle to conduct any number of comparisons. Let $D_{it} \in \{0, 1\}$ represent the treatment status for unit i at time t, and assume for illustrative purposes that we can observe three pre- and post-treatment periods. We can compare outcomes between any two sequences of D_{it} for $t \in \{-3, -2, -1, 0, 1, 2, 3\}$.

[18] ICE FOIA Library, `https://www.ice.gov/foia/library`.

[19] Available at `https://uselectionatlas.org/`.

[20] I rely on my own definition of "sanctuary" jurisdictions, since the term has no precise or commonly agreed-upon meaning. There are several organizations that maintain lists of sanctuary cities, counties, and states (see, for instance, the Center for Immigration Studies (`https://cis.org/Map-Sanctuary-Cities-Counties-and-States`) or the Federation for American Immigration Reform (`https://fairus.org/sites/default/files/2017-08/Sanctuary_Policies_Across_America_Report.pdf`)) and the same places tend to be identified by sources across the ideological spectrum. In general, two kinds of policies earn jurisdictions sanctuary status: policies preventing local law enforcement from asking about immigration in the course of regular policing, and policies that limit cooperation and communication with federal authorities, including compliance with ICE detainers. I include jurisdictions with either type of policy in my definition, but require that there be a legally binding document that institutionalizes it: a resolution or ordinance from the city council, an executive order from the mayor, or a memo or directive from the sheriff's office. I begin with existing lists, then check and supplement them using online sources, which are documented in detail in Appendix B.2.

[21] Available at `https://www.urban.org/features/state-immigration-policy-resource`.

Throughout my analysis, I choose the intuitive comparison of a signer's true trajectory—a sequence of $D_{it} = 0$ in the time up to signing ($t < 0$), and a sequence of $D_{it} = 1$ from $t = 0$ onward—to a counterfactual of never signing over the same period (a sequence of $D_{it} = 0 \; \forall \; t$).[22] The vector $\mathbf{D}$ for the control and treated groups can be written as follows:

	t						
	-3	-2	-1	0	1	2	3
$\mathbf{D}_C$	0	0	0	0	0	0	0
$\mathbf{D}_T$	0	0	0	1	1	1	1

An important issue that arises is the proper selection of pre- and post-treatment observations within the control group: because counties signed 287(g)s at different times, there is no uniform "post-treatment" period for the controls. Thus, in my analysis, each treated observation gets its own control set, in which t is defined as the number of months relative to the month of signing. For instance, consider Maricopa County, Arizona, which signed its first 287(g) agreement in February 2007. I am interested in Maricopa's law enforcement record in the 12 months before and after 287(g) entry, and the relevant comparison is similarly nonparticipants over the period of February 2006 to February 2008. I put all these observations in my analysis set, recoding the time variable relative to February 2007 ($t = 0$). Then, I repeat the process with Charleston County, South Carolina, which signed its first 287(g) in January 2012. The observations that enter the analysis set for Charleston are from January 2011 to January 2013, with t coded relative to January 2012. When I have done the same thing for every county's first 287(g) signing, I compute the means by treatment status within every time bin from $t = -12$ to $t = 12$.[23]

5.3 Estimating Selection Effects into the 287(g) Program

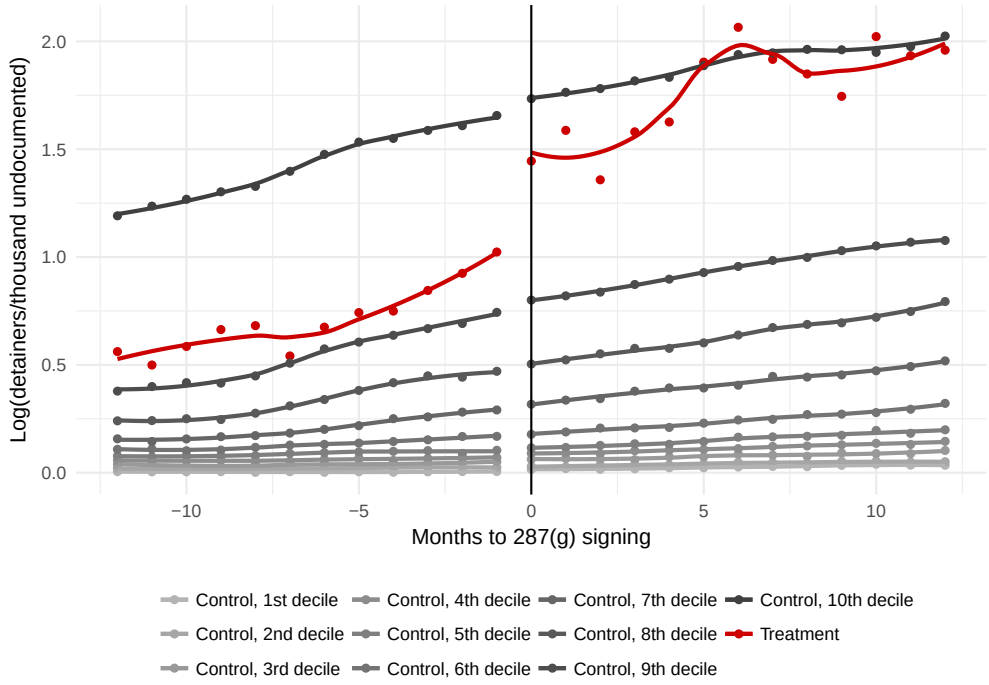
How do first-time 287(g) signers compare to the rest of the nation in their pre- and post-treatment enforcement trajectories? Consistent with theoretical expectations, they are high demanders, though not the most extreme preference outliers. In Figure 2, I use the approach outlined above

[22] There is not enough time for an agreement to expire in the small window (no more than three years, the usual length of a contract) to which I limit my post-treatment analysis.

[23] Note that the usual methods of statistical inference cannot be applied to this analysis set, since control observations enter multiple times for different treated units. For the graphical analysis shown in Figures 2 and 3, I simply report binned means; when I conduct the final diff-in-diff analysis, I compute properly adjusted standard errors using a weighted bootstrap.

to compare treated units to all other counties. I plot the logged number of detainers not denied per thousand undocumented immigrants on the y-axis, with time to 287(g) signing on the x-axis. I divide the untreated observations into deciles based on each county's average outcome over the entire pre-treatment period ($t = -12$ to $t = -1$), then plot the means within each time bin and decile in gray; means for the treated group by time bin are shown in red.

Figure 2: Detainers per Thousand Undocumented Immigrants, 287(g) vs. All Other Counties



Notes: Points represent means within bins for every time period from $t = -12$ to $t = 12$ months from first 287(g) signing. Treated units, defined as those that signed a 287(g) agreement for the first time from 2005 to 2012, are shown in red, and all other counties are shown in gray. Deciles are computed based on county-level averages of the outcome variable over the entire pretreatment period of $t = -12$ to $t = -1$. Plotted outcome is the natural log of the number of detainers issued that were not denied by the LEA, scaled by the estimated number of undocumented immigrants in the jurisdiction (in thousands) and aggregated by month.

It is evident from Figure 2 that 287(g) signers were at the upper end of the national distribution in their demand for immigration enforcement, surpassed only by the top decile of nonsigners. There is already evidence, moreover, that these signers saw a discontinuous increase in enforcement immediately after 287(g) entry—catching up to or even surpassing the top decile of nonsigners—while the control counties did not see the same effect. While the president's preferences for immigration enforcement cannot be measured on the same scale, I have argued in Section 4 that from the totality

of the evidence, including Obama's speeches and executive orders, his position should be seen as somewhere at the high end, but not the very top of, the national distribution. This pool of participants is therefore broadly in line with the group that would maximize federal welfare according to my theory.[24]

5.4 Estimating the Causal Effects of 287(g) Entry

Because treated counties signed 287(g) agreements with the specific intention of shifting their focus toward immigration enforcement—and had been making strides toward this goal even before doing so—careful construction of the counterfactual is required to identify the causal effect of the program. I use a matching design to select the subset of counties with the most similar pre-treatment trends to the treated units, then compute a difference-in-differences estimate with the control set. The assumption for identification is that, absent the program, both treated and control units would have continued along the same pre-treatment trajectories. If this assumption is satisfied, I can compare differences in the means of the immigration enforcement outcomes of interest before and after 287(g) entry among treated and control units, and this estimate represents the effect solely attributable to the intervention.

To construct a comparison group for a county that signed a 287(g) at time t, I first take the entire pool of counties that had never participated in the program up to time t, and would not do so for at least three subsequent years.[25] Then, I implement nearest neighbor matching on the number of detainers not denied per thousand undocumented immigrants estimated to live in that county, aggregated to each month from $t = -12$ to $t = -1$. I select the six control counties that minimize the Mahalanobis distance to the same sequence for each treated county. I additionally include two key characteristics of counties as matching covariates: their total population as well as their undocumented immigrant population in the year of signing. The inclusion of these covariates ensures that the matched sets contain counties that are similar to one another in size and supply

[24] The extent to which the president could actually control who participated in the program is the next avenue that I am exploring in my research. I am working on obtaining data on *applicants* to the program—not just participants, to which I am restricted in my current data—in order to investigate whether the executive was actively rejecting applicants from this highest-demanding decile. (According to Wong (2012), the rejection rate as of 2012 was 31% (28 of 89 counties.)) More generally, I am exploring who these very highest demanders were and why they chose not to participate. It is possible that their enforcement demands were already satiated without the additional authority.

[25] I do include counties that would sign a 287(g) at some point after the relevant period in the pool of potential controls; to omit these counties would constitute selection on the dependent variable, a possible source of post-treatment bias.

of potential detainers, lending more plausibility to the diff-in-diff identification assumption.

The approach is very effective at finding comparable matched sets. In Figure 3, I plot the month-binned means for the treated units and matched controls, with a loess-smoothed curve fitted to either side of $t = 0$. The pre-treatment trends are very similar. However, this alone does not assure identification of a causal effect; rather, it must also be true that the trends would have *continued* in parallel in the absence of the intervention. Although this assumption is fundamentally untestable, it becomes likelier to hold the more similar the groups are to one another in their observable characteristics. Indeed, a two-sample t-test detects only one statistically significant difference between groups (with $p < .05$) out of fifteen tested population, immigration, and enforcement outcomes measured in the year of 287(g) signing (see Appendix Table C.1). When plotted on a map of the U.S., the two sets of counties tend to cluster in the same geographical regions, with a large share of treated counties sharing a border with at least one control (see Appendix Figure C.3).[26] Most encouragingly, the control group captures 14 counties that would eventually sign their first 287(g) under the Trump Administration, or 33% of all Trump Administration signers—further reason to believe that the treatment and control sets contain analogous political units.

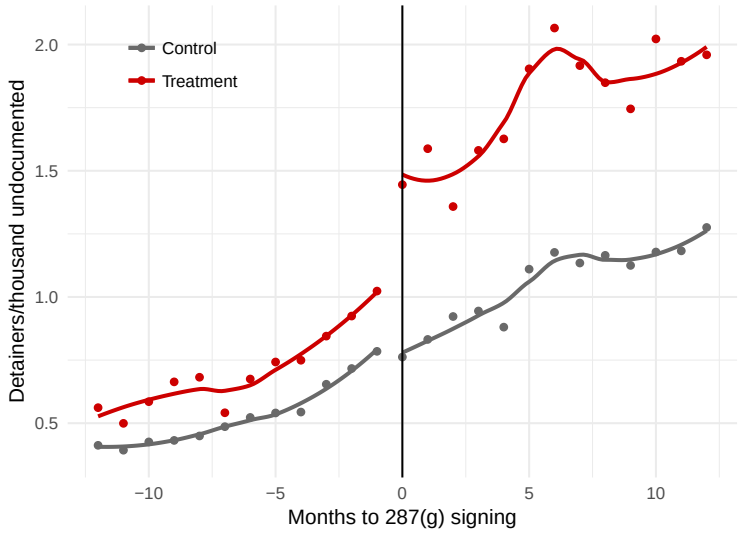
The effects of 287(g) on the volume of local immigration enforcement are dramatic. Table 1 presents diff-in-diff estimates computed using the approach outlined above for each of four periods: the year of signing (t), and each subsequent year until $t + 3$.[27] Standard errors are calculated by weighted bootstrap, accounting for the fact that counties may enter the control set multiple times for different treated units (Kim, Wang and Imai 2018). The number of detainers issued in 287(g) counties grows steadily over time relative to the counterfactual, reaching 671 additional detainers in the third full year of the program ($p < .05$). And though the patterns of statistical significance vary slightly, the trend is the same however one scales detainers to the local immigrant population: when accounting for the denominator, the treatment effect peaks two years out from signing, at 12.7 and 8.7 detainers per thousand undocumented immigrants and noncitizens, respectively ($p < .05$).

When LEAs gain 287(g) powers, it is not only the volume of immigration enforcement that changes; rather, the focus of law enforcement shifts from policing crime to policing immigration. My analysis shows that the types of crimes targeted by local authorities are qualitatively different

[26] Full lists of treated and control observations are available in Appendix Tables C.2-C.3.

[27] Note that, by contrast to the previous section, t is now measured in years, not months, in order to capture the long-term effects of the program.

Figure 3: Detainers per Thousand Undocumented Immigrants, 287(g) vs. Matched Controls



Notes: Points represent means within bins for every time period from $t = -12$ to $t = 12$ months from first 287(g) signing. Treated units, defined as those that signed a 287(g) agreement from 2005 to 2012, are shown in red, and matched control counties are shown in gray. Plotted outcome is the number of detainers issued that were not denied by the LEA, scaled by the estimated number of undocumented immigrants in the jurisdiction (in thousands) and aggregated by month.

Table 1: Effects of First-Time 287(g) Signing on Detainers Honored, Diff-in-Diff Estimates

	t	$t+1$	$t+2$	$t+3$
Number of detainers, not denied	219.0†	192.2	589.8*	671.0*
	(147.7)	(143.4)	(271.0)	(310.6)
Number of detainers, not denied	4.6	10.7†	12.7*	12.2†
per thousand undocumented immigrants	(3.3)	(5.8)	(6.7)	(7.4)
Number of detainers, not denied	3.1	7.6*	8.7*	8.0†
per thousand noncitizens	(2.0)	(3.7)	(4.0)	(4.4)

Notes: †p<0.10; *p<0.05. Standard errors computed based on 1,000 weighted bootstrap samples are shown below estimates.

from those of interest to ICE: under local control, there is a greater focus on misdemeanors—the vast majority of them traffic offenses and drug possession—as opposed to felonies. Qualitative accounts suggest that this approach to policing, which includes traffic stops and searches in strategic areas where undocumented immigrants are likely to be found, is a mechanism used for drawing people into the deportation pipeline (Armenta 2017).

A few words about immigration law are needed for a complete picture of how 287(g) transforms local policing priorities. In general, immigration violations are handled by two systems: criminal courts and civil immigration courts. Only a few specific types of offenses are covered by criminal statutes: improper entry (8 U.S.C. §1325) and reentry (8 U.S.C. §1326). The former includes entry without inspection by immigration officers or by willfully misrepresenting one's status; the latter involves being found in the U.S. after having been previously denied admission or removed. However, these categories do not capture many of the common ways in which undocumented immigrants end up in the country, such as overstaying their visas. Moreover, these charges have a five-year statute of limitations; thus, immigrants who entered improperly but avoided apprehension in their first five years can no longer be tried under the criminal codes. Outside of these specific conditions, the act of being illegally present in the U.S. constitutes a civil offense and is handled by a system of specialized immigration courts. These courts facilitate the removal of undocumented immigrants through a separate administrative system under the Department of Justice, but there are no criminal charges in these cases (see Owsley (2014–15)).

Thus, the first and most straightforward way to capture an uptick in immigration enforcement is to measure criminal convictions under codes §1325 and §1326. But this represents a limited subset of all immigration enforcement activity in a jurisdiction; for the full picture, it is important to also look at civil deportation proceedings. Unfortunately, my data only includes criminal charges. However, it allows me to construct two good proxies for a jurisdiction's focus on immigration offenses in their own right rather than as a byproduct of policing crime. First, I can see how many detainers never result in a criminal charge. This captures the rate at which officers correctly identify criminals as opposed to engaging in racial profiling or making apprehensions that do not hold up to legal scrutiny—conceptually similar to the metric of "hit rates" in the stop-and-frisk literature (see Goel, Rao and Shroff (2016) for a review). Since detainers lead to deportation proceedings regardless of whether there is any criminal wrongdoing, localities that bring more people in without

cause are likely to be policing immigration rather than crime. A second measure of the same type of strategy is the proportion of convictions that are for misdemeanor crimes as opposed to violent felonies—specifically, criminal charges that are likely to arise from policing tactics such as traffic stops, vehicle searches, and identification checks.

In Table 2, I present diff-in-diff estimates disaggregated by group: those resulting in a criminal immigration conviction, those resulting in no criminal charge, and those resulting in a criminal conviction, disaggregated by type of crime.[28] First, there is little evidence that 287(g) entry increases criminal immigration convictions. This is unsurprising: most 287(g) counties are far from border crossings, so the baseline incidence of illegal entry and reentry satisfying the criminal statutes is low. And notwithstanding the documented cases of 287(g) authority being used for entirely spurious arrests,[29] I do not see systematic evidence that treated counties detain more people without the cause to ultimately charge them. While there are 120 more detainers without a charge in the year of signing ($p < .05$), the effect does not persist into subsequent years.

The most dramatic change resulting from 287(g) participation is a steady increase in criminal convictions for crimes other than immigration offenses, with a disproportionate number of these new convictions being driven by misdemeanors. By the time counties have spent three full years in the program, they are responsible for 212 more misdemeanor convictions than the counterfactual ($p < .05$), which is equivalent to a 13 percentage point increase in the proportion of convictions that were for misdemeanor offenses.[30] Fully one half of these misdemeanors are traffic offenses, with another 21 percent coming from drug possession charges.[31] These findings are consistent with qualitative evidence on how 287(g) changes policing practices. For instance, Nguyen and Gill (2016) recount how traffic checkpoints routinely appeared in front of a field where Mexican and Salvadoran migrants played soccer in Alamance County, NC, and Armenta (2017) recounts how officers in Southeast Nashville began making traffic stops without cause in order to check drivers' identification and search their vehicles.

[28] ICE classifies a subset of felonies as "aggravated felonies," which carry particularly harsh consequences for removal proceedings. I call these crimes "higher level" in Table 2. For a complete list of qualifying offenses, see `https://www.law.cornell.edu/uscode/text/8/1101`.

[29] See `https://www.newyorker.com/magazine/2009/07/20/sheriff-joe`.

[30] See Appendix Table C.5 for diff-in-diff estimates treating the outcomes in Table 2 as proportions of all detainers.

[31] This is computed based on the time period from the year of 287(g) entry to three years after, in treated counties.

Table 2: Effects of First-Time 287(g) Signing on Law Enforcement Priorities, Diff-in-Diff Estimates

	t	$t+1$	$t+2$	$t+3$
Number of detainers with criminal immigration conviction	5.8	2.3	10.3†	9.7
	(5.0)	(2.0)	(6.6)	(7.5)
Number of detainers without any criminal charge	120.3*	24.8	110.8†	104.5
	(68.1)	(30.2)	(62.7)	(69.7)
Number of detainers with criminal conviction (excluding immigration crimes)	62.0	132.6	359.5*	421.1*
	(60.7)	(112.7)	(176.2)	(199.8)
Misdemeanor crime	21.5	69.9	189.1*	212.4*
	(25.2)	(57.6)	(84.6)	(93.8)
Felony crime (lower level)	9.8	17.9	44.9*	50.3*
	(8.3)	(15.3)	(22.1)	(24.8)
Felony crime (higher level)	30.7	44.8	125.5†	158.3*
	(29.3)	(43.5)	(74.6)	(88.0)

Notes: †p<0.10; *p<0.05. Standard errors computed based on 1,000 weighted bootstrap samples are shown below estimates.

5.5 Robustness Checks

A plausibility check on whether the empirical findings are generated by my theory is that local agents are accounting for the new detainers in treated jurisdictions; if it is federal authorities, then I have not successfully accounted for selection, or else another mechanism is at work. While my data cannot distinguish between detainers originating from local and federal authorities, it can provide an imperfect test: I can see the type of facility—county, federal, ICE, state, or local—in which each detainer was issued. This is useful because sheriff's departments—the LEAs responsible for signing 287(g)s in the vast majority of cases—are usually charged with overseeing county jails, but rarely play a role in state and federal facilities. Thus, to lend further support for my theory, I should see the treatment effects in jurisdictions with 287(g)s signed by sheriffs' departments being driven largely by county jails. Table 3 confirms that this is indeed the case: in this subset of 45 counties, the increase in detainers in each year after treatment is driven exclusively by county facilities, with no statistically significant increases in any of the other locations. This translates into a more than

twenty percentage point increase in the share of detainers initiated in county facilities by the second full year of the program.[32]

Identification of the causal effect of 287(g) entry on local law enforcement rests on the assumption that treated and control outcomes would have continued in parallel in the absence of the intervention. This assumption is likelier to hold the more similar the two sets are in their political and demographic characteristics. There are two ways to incorporate additional covariates into the analysis: matching on them in the construction of the control group, and adjusting for them after the fact when computing the treatment effect, using multivariate regression. My results are robust to both approaches. In Appendix Table C.6, I construct control sets using the same approach as the main analysis, but with several more matching covariates: in addition to counties' outcome histories in the twelve months prior to 287(g) signing and their total and noncitizen populations, I include the percentage point change in their noncitizen populations from five years prior; their Democratic vote share in the last presidential election; and binary indicators for whether they contained any sanctuary jurisdictions and whether they had immigration protests in 2006-07. The results look very similar. In Appendix Table C.7, I pool the matched treated and control sets and estimate the linear regression:

$$Y_{it} = \beta_0 + \beta_1 287(\text{g})_i + \beta_2 \text{post}_t + \beta_3 (287(\text{g}) \times \text{post})_{it} + \mathbf{X}_{it} + \varepsilon_{it} \tag{12}$$

where Y is the outcome in county i and year t, 287(g) is a binary indicator for treated counties, post is a binary indicator for the post-treatment period, and $\mathbf{X}$ is the same time-variant vector of county-level controls that I used for matching in Table C.7.[33] The reported coefficient is β_3, the interaction of the treatment indicator with the post-treatment period, and it is comparable to the diff-in-diff estimates reported in Table 1.

When conducting nearest neighbor matching, a decision must be made regarding the optimal number of controls to select for each treated observation. Since the purpose of my matching analysis is to achieve parallel trends, I choose the number of matches, n, that minimizes the variance of the

[32] As a check, I conduct the same analysis on the ten police departments that signed 287(g)s (see Appendix Table C.4). While there are too few observations here to detect many statistically significant effects, the magnitudes of the estimates point to local rather than county facilities experiencing the most dramatic uptick in detainers, with federal facilities seeing a slight but not meaningful reduction in activity.

[33] I also include a binary indicator for whether it was a part of Secure Communities, which could not be used for matching.

Table 3: Effects of First-Time 287(g) Signing on Detainers Honored, Diff-in-Diff Estimates by Facility Type (45 Counties with 287(g) Signed by County Sheriff)

	t	$t+1$	$t+2$	$t+3$
Number of detainers, not denied	255.1†	234.6†	632.2*	726.9*
	(172.5)	(154.0)	(290.3)	(337.1)
County facility	222.4*	212.3	545.7*	602.6*
	(132.2)	(134.8)	(211.0)	(251.0)
Federal facility	-1.5	3.1	24.1	30.1
	(4.5)	(10.0)	(23.1)	(30.6)
ICE facility	1.0	-4.8	-5.2	-1.5
	(4.7)	(5.4)	(6.3)	(10.3)
State facility	50.6	48.1	32.0	56.7
	(39.6)	(41.4)	(44.4)	(47.6)
Local facility	-15.0*	-21.1	38.5	40.1
	(13.6)	(29.2)	(64.7)	(71.4)
Detainers, not denied, by facility as a proportion of all detainers				
County facility	0.18*	0.14†	0.21*	0.18*
	(0.08)	(0.08)	(0.08)	(0.08)
Federal facility	-0.09	-0.06	-0.05	-0.05
	(0.07)	(0.06)	(0.06)	(0.06)
ICE facility	-0.03†	-0.04†	-0.04	-0.02
	(0.02)	(0.03)	(0.03)	(0.03)
State facility	0.00	0.01	-0.04	-0.04
	(0.05)	(0.05)	(0.05)	(0.05)
Local facility	-0.04	-0.04	-0.06	-0.06
	(0.04)	(0.04)	(0.05)	(0.05)

Notes: †p<0.10; *p<0.05. Standard errors computed based on 1,000 weighted bootstrap samples are shown below estimates. Results by facility type may not sum perfectly to totals due to a negligible number of detainers with "other" or no coded facility type.

distance between average outcomes in the treatment and control group from $t = -12$ to $t = -1$. Appendix Figure C.4 shows that this quantity is minimized with $n = 6$, and Appendix Figure C.5, which replicates Figure 3 for one through eight matches, confirms that these sets do not diverge from one another over the period. To make sure that the results are not particularly sensitive to the choice of n, I replicate Table 1 using four instead of six matches, since that achieves the next smallest variance; as Appendix Table C.8 shows, the results do not change substantially.

To guard against potential measurement error in the undocumented immigrant population, I substitute it with the total number of noncitizens in the matching step. As shown in Appendix Table C.9, the resulting estimates are even larger. Finally, to exclude the possibility that my results pick up spurious correlations or secular trends, I conduct a placebo test that treats $t-2$ and $t-1$ as t and $t+1$, respectively. As expected, Appendix Table C.10 shows that there are no statistically significant treatment effects in this analysis.

6 The Role of Local Democracy in Immigration Enforcement

In the majority of 287(g) counties, the key local decisionmakers were democratically-elected sheriffs.[34] In this section, I explore how electoral motivations drove their behavior. I find that localities were more than twice as likely to sign agreements in election years, and that, for the most part, it was elected rather than appointed officials who oversaw the LEAs with the largest treatment effects. Consistent with other recent work on the subject, partisanship predicts neither 287(g) entry nor use of authority.

6.1 Evidence on the Effects of Electoral Politics

Although there has not been much empirical research on the 287(g) program, one recent in-depth study has suggested that the decision to make policing immigration a local priority is driven by constituency pressures (Armenta 2017). Examining the case of Davidson County, Tennessee, Armenta notes that the sheriff's decision to sign an agreement in 2007 occurred at the same time that the Nashville Metro Council was considering several anti-immigrant ordinances, including bills to punish businesses for hiring unauthorized immigrants and landlords for renting to them, as well

[34] 45 of the 56 treated observations in my sample had 287(g)s signed by elected sheriffs, with 11 others signed by police departments and a highway authority.

as an ordinance declaring English the city's official language. Immigration was prominently in the public discourse at this time, with both opponents and proponents of the proposed ordinances mobilizing under the banners "Nashville for All of Us" (N4AOU) and "English Only," respectively. And since sheriffs tend to have jurisdiction over suburban, rural, and unincorporated parts of a county—usually, the more conservative areas—while police forces tend to have jurisdiction over the cities, sheriffs' electoral pressures tend to militate in the pro-enforcement direction even when there is no consensus in the county at large (Ibid.).

If accountability to voters, and in particular to nativist constituencies, is an important factor in sheriffs' decisionmaking, then we should see more 287(g) entry in election years. The proportion of signings from 2006 to 2016 that occurred in election years is 0.34.[35] To assess how unusual this is under a null hypothesis that election years are independent of 287(g) entry, I conduct a permutation test. This test randomly reshuffles the sheriff election calendars of all U.S. counties according to their observed frequencies in the data over 10,000 iterations.[36] I then compute the proportion of 287(g) signings (which are held at their true values) occurring in sheriff election years for each permutation. Figure 4 presents a histogram of this statistic, with the true value of 0.34 shown in red. A mere 0.3 percent of the 10,000 random permutations of the data produce a statistic at least as extreme as the observed value, suggesting that it is very unlikely to have arisen by random chance.

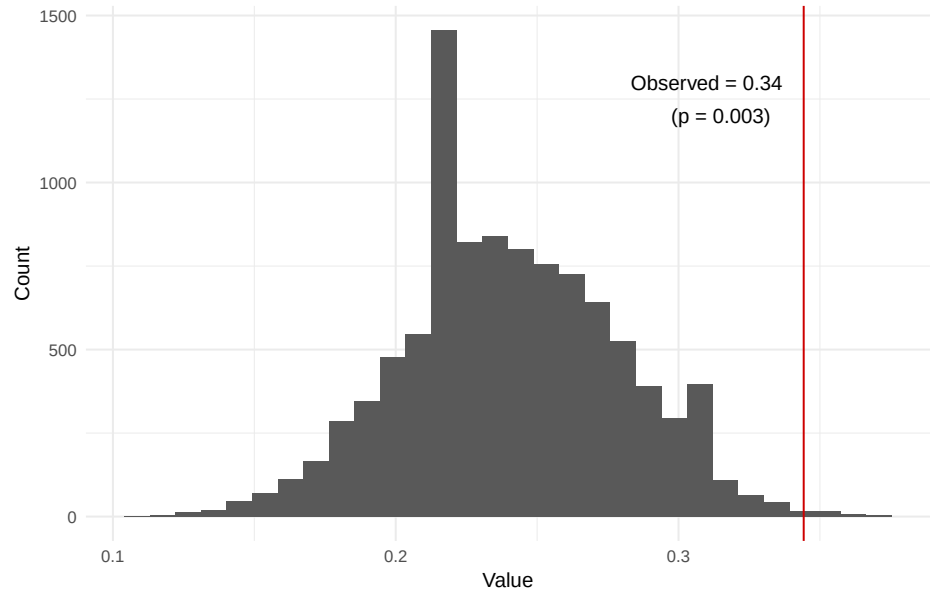
Next, I conduct a more systematic test of the hypothesis that electoral considerations influence 287(g) signing, using logistic regression and controlling for other known predictors of nativist politics. The model includes the county's Democratic vote share in the last presidential election, since localities with high concentrations of Democratic voters are generally less likely to propose and pass exclusionary immigration laws than those with large Republican majorities (Chavez and Provine 2009; Ramakrishnan and Wong 2010). I again include the population, both total and noncitizen, in the county, as well as a control for prior 287(g) signing.[37] Two predictors of anti-immigrant

[35] I select this period because 2006 is as far back as I can get reliable election data, and 2016 is the last year for which I have covariates for the subsequent regression analysis.

[36] Specifically, I assign election calendars to counties according to the following probabilities: 42% of counties are assigned to {2008, 2012, 2016}; 44% are assigned to {2006, 2010, 2014}; 3% are assigned to {2009, 2013, 2017}; and 11% are assigned to {2007, 2011, 2015}. These are approximately the proportions observed in the data, though a handful of counties deviate because of deaths, resignations, and other special elections, as well as a small number of 2-year or 6-year terms. This is a trivial proportion of all counties that does not meaningfully affect the analysis.

[37] Whereas in the previous section I was mainly concerned with first-time signings, since those are the ones that induce the biggest change in local enforcement practices, in this analysis I am interested in all signings including

Figure 4: Permutation Test, Proportion of 287(g) Signings Occurring in Sheriff Election Years (2006-16)



Notes: This test randomly permutes the sheriff election calendars of all U.S. counties according to their observed frequencies in the data over 10,000 iterations. The computed statistic for each iteration is the proportion of all 287(g) signings from 2006 to 2016 that occurred in sheriff election years.

sentiment enter into the model: the unemployment rate among whites, and a binary indicator for counties in which there were immigration protests in 2006. These protests, which occurred across the nation and included marches, demonstrations, and walkouts from school and work, were a reaction against proposed national legislation to raise penalties for illegal immigration, and advocated alternative immigration reform that included a pathway to citizenship. While these events may have mobilized Latinos to political action (Wallace, Zepeda-Millán and Jones-Correa 2014), they also inspired anti-immigrant backlash in white communities (Armenta 2017; Hopkins 2010).

The fully specified model is given by:

$$\Pr(Y_{it} = 1|\mathbf{X}_{it}) = f(\beta_0 + \beta_1 \text{SheriffElection}_{it} + \mathbf{X}_{it}\gamma + \eta_t + \varepsilon_{it}) \tag{13}$$

where Y_{it} is a binary indicator for 287(g) signing by county i in year t, $\mathbf{X}_{it}$ is the vector of controls described above, and η_t is a year fixed effect, included to account for the fact that some years have

renewals, since any kind of signing may bring with it an electoral reward.

especially high baseline rates of 287(g) adoption.[38] I estimate the model on a dataset that includes all county-year observations from 2006 to 2016, excluding those in which a county was ineligible to sign an agreement because it already had one in place.

Table 4: Modeling the Effect of Electoral Considerations on 287(g) Entry

	Sheriff Elections		Presidential Elections	
Election year	2.06**	1.66	2.06	1.97
	(0.52)	(1.15)	(1.35)	(1.71)
Democratic vote share in last presidential election (percentage points)	0.99	0.99	0.99	0.99
	(0.01)	(0.01)	(0.01)	(0.01)
Election year * Democratic vote share		1.01		1.00
		(0.02)		(0.01)
Population (ten thousands)	1.02***	1.02***	1.02***	1.02***
	(0.00)	(0.00)	(0.00)	(0.00)
Noncitizen population (ten thousands)	0.89***	0.89***	0.89***	0.89***
	(0.02)	(0.02)	(0.02)	(0.02)
Had 2006 immigration protests	1.89	1.91	1.81	1.82
	(0.86)	(0.87)	(0.83)	(0.83)
White unemployment rate	0.95	0.94	0.95	0.95
	(0.05)	(0.05)	(0.05)	(0.05)
Prior 287(g) signing	199.75***	199.39***	200.77***	200.56***
	(61.47)	(61.36)	(61.67)	(61.66)
Year FE	Yes	Yes	Yes	Yes
Observations	34,001	34,001	34,001	34,001

Notes: p<0.05; **p<0.01; ***p<0.001.

Table 4 reports exponentiated coefficients from Equation 13, with standard errors computed by delta method in parentheses. I find that being in a sheriff election year more than doubles the predicted probability of 287(g) entry (Column 1). It is difficult to imagine how this effect could be attributed to anything other than sheriff elections, since they do not systematically overlap

[38] This is primarily due to the reform of the program in 2010, at which point all active agreements were nullified and participants were forced to sign new, standardized MOAs if they wanted to remain in the program. These new MOAs had three-year sunset clauses, and for this reason the years 2010, 2013, and 2016 have very high baseline rates of signing.

with elections for any other local, state, or federal office, but just in case, I run a placebo check.[39] Column 3 presents results from reestimating the model with presidential election years instead of sheriff election years. Although the coefficient happens to be the same, it is now imprecisely estimated and statistically indistinguishable from zero.

The other hypothesized predictors of 287(g) entry do not seem to have much explanatory power. The only characteristics of counties that predict 287(g) signing are having a larger population, and, surprisingly, having a smaller noncitizen population. It is possible that although 287(g) signers had fewer immigrants than the average nonsigner, natives in these areas perceived immigrants—either locally, or across the nation—as a more significant threat.

6.2 Evidence on the Effects of Partisanship

While there is already strong evidence that electoral pressures drive 287(g) entry, it is possible that, accounting for partisan forces, they are even stronger. In the current national immigration debate, the Republican party advocates a more restrictionist approach; thus, it may be the case that the overall effect masks responsiveness in the direction of greater enforcement in Republican majority counties, and in the direction of greater leniency where there are Democratic majorities. This expectation is not borne out in the data. In Table 4, I estimate the model in Column 1 including an interaction term of election year with the Democratic vote share in the previous presidential election—the best measure of constituency partisanship that is available at the county level—and the coefficient is not substantively or statistically significant. In large part, this may be explained by the fact that counties are an imprecise political unit for the question at hand: they often encompass the more conservative suburban and rural areas that sheriffs disproportionately serve as well as the urban areas served by police forces (and the urban areas are, of course, more populous). But there is also good reason to take this finding seriously and interrogate it more carefully in future work. Some of the most prominent restrictionist sheriffs in the U.S., including David Clarke of Milwaukee County, WI and Jim Pendergraph of Mecklenburg County, NC, were elected as Democrats, though both have since developed ties to the Republican party. And looking more systematically across multiple outcomes, including compliance with federal detainer requests

[39] I also verify that the signings occurred before elections took place in November; for the most part, that is the case. See Appendix Figure C.6.

and survey responses relating to immigration enforcement, Thompson (2018) finds that sheriffs behave similarly across party lines.

Partisanship also fails to explain how elected officials use federal authority. In Figure 5, I plot all of the 287(g) counties' individual treatment effects on detainers not denied per thousand undocumented immigrants at time $t+2$, when the effects were largest (the same estimates that are in the second row of Table 1). I distinguish counties where the 287(g) was signed by a Republican sheriff (red), a Democrat sheriff (blue), and a police chief (black). There are fewer Democrat signers than Republican signers overall (9 vs. 24), but conditional on participation, partisanship does not predict the effect size: Republican sheriffs are responsible for some of the biggest increases in enforcement, but also some of the biggest decreases. What is abundantly clear is that sheriffs, whatever their partisanship, are the ones driving the positive treatment effect: of the LEAs with the 25 largest estimates, all but one had a sheriff in charge.[40]

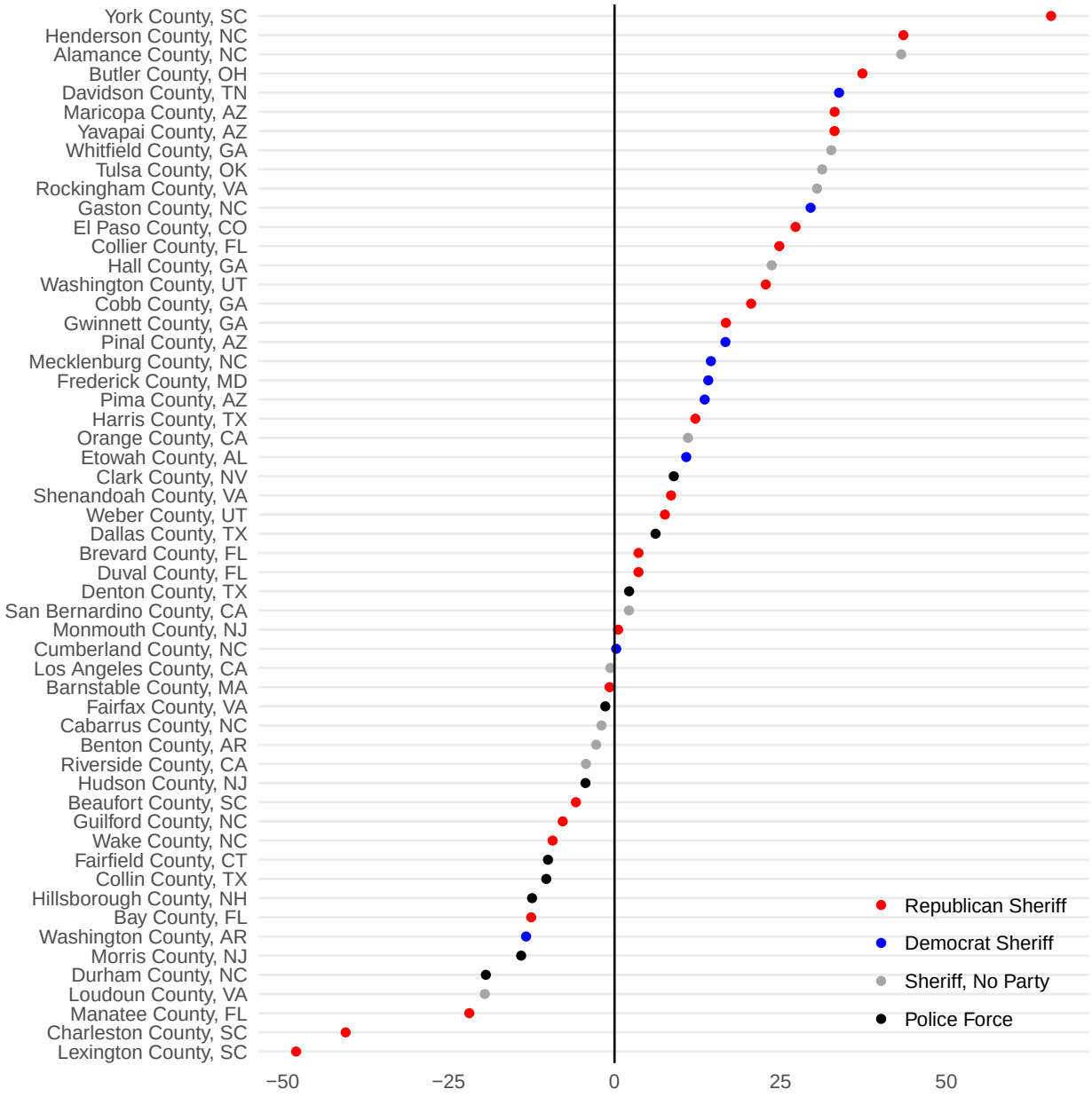
7 Conclusion

I have shown that 287(g) authority dramatically increased the volume of immigration enforcement activity at the county level. It did so specifically by empowering local agents and enhancing local capacity; state and federal efforts, meanwhile, remained at pre-treatment levels. I have also shown that when localities voluntarily take charge, their enforcement priorities differ from those of federal agents: they are more focused on misdemeanor offenses—overwhelmingly, traffic violations and drug possession—than felonies, which reflects a focus on policing immigration rather than policing crime. Finally, I have shown that these dynamics are driven by local electoral forces, which conditioned both 287(g) entry and subsequent use of authority.

In so doing, I have identified an important lever of influence—the strategic downward extension of federal authority—that the executive has at his disposal for advancing his policy agenda. My analysis suggests that this strategy carries with it a central trade-off: while granting powers to subnational agents who are willing to undertake federal responsibilities effectively increases the volume of associated policy production, the implementation of those policies is no longer guaranteed to reflect federal goals. Relevant to the balance of these competing considerations is the

[40] Though interestingly, the single largest effect—a high outlying value of 288 detainers per thousand undocumented immigrants—was the police force of Manassas City, VA.

Figure 5: Diff-in-Diff Estimates at $t + 2$, Detainers per Thousand Undocumented Immigrants



Notes: One additional treated unit, Manassas City in Virginia, is not plotted here because its treatment effect is an outlying value of 288 detainers per thousand undocumented immigrants. The LEA that signed a 287(g) in Manassas City is the police department. Police force category also includes one independent highway patrol authority. California has nonpartisan county sheriff elections; the other gray points represent counties for which data on the partisanship of the county sheriff was unavailable.

federal principal's ability to correctly identify ideologically aligned actors, to clearly circumscribe the limitations of the devolved authority, or else to strictly monitor and credibly punish the agents.

This lever therefore differs in important ways from the other mechanisms of influence familiar to the vertical diffusion literature. Recent work has documented the increasing sophistication of financial incentives as presidents continue to innovate in this domain. For instance, President Obama's Race to the Top Initiative induced extremely precise and prescriptive policy change by trading cash rewards for states' promises to implement entire portfolios of education reforms (Howell and Magazinnik 2018, 2017). By contrast, the high variance of my estimates reveals that lending authority is a relatively blunt instrument. The voluntary nature of the program, coupled with the financial burden it imposed on the agents themselves, introduces not only a baseline level of heterogeneity in that it disproportionately attracts the highest demanders, but even an added dimension of heterogeneity in implementation among the treated. The strategy is therefore most useful in domains where the principal cares about the sum or the average of policy outputs, but not the variance.

As a final consideration, it is worth turning from the interests of the principal to a broader assessment of social welfare compared to an alternative regime of central control. Theoretical work has shown that shifting the locus of decisionmaking downward leads to a closer alignment between constituency preferences and policy outcomes (Crémer and Palfrey 2000). This result is likely to be exacerbated by Congressional polarization and gridlock (McCarty, Poole and Rosenthal 2006; Binder 2003) coupled with geographic ideological sorting (Bishop 2009; Sussell and Thomson 2015).[41] If the federal legislative process is indeed hamstrung from keeping up with the changing facts on the ground, and if polarization is making compromise increasingly unpalatable to all parties, then allowing like-minded communities to make their own policy decisions should, in principle, increase social welfare.

There are two problems with this view. First, there are some policy areas where geographical heterogeneity cannot be so easily tolerated. In general, criminal law derives its legitimacy from universalistic notions of justice, not particularities of place. And second, even with some concessions made for spatial variation in the interpretation of the law, it is harder yet to justify temporal variation, especially when the only relevant variable is the proximity to an election. Although

[41] Though the presence of geographic sorting is controversial; see Klinkner (2004).

it has received limited attention with respect specifically to county sheriffs, the same issue has occupied a much larger literature on democratically elected judges: in Washington State, one study has shown, judges imposed longer sentences at the end of their political cycles than at the start (Berdejó and Yuchtman 2012), and in Alabama judges were more likely to override juries' recommendations with the death penalty in election years (Equal Justice Initiative 2011). As locally elected law enforcement officials continue to gain unprecedented powers, more scholarly attention will be needed to understand the full range of positive and normative consequences.

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