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[Note to readers: as you will see, this is truly a first rough draft. Our results are literally days old, and we have only just begun to think through them. Tom and I appreciate everyone's patience with such an early draft and look forward to your thoughts on how we can make it better. -A.B.C.]

The Real World of Immigration Federalism

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Writing about immigration federalism has, like much federalism scholarship, long been stuck with an outdated model of federal-state relations. Under that old model, states wield power principally by possessing regulatory autonomy. But the reality today is quite different: as a number of writers have begun to document, state and local governments today are often empowered not by their autonomy, but instead by their incorporation into federal statutory schemes. According to these accounts, sovereignty-based federalism is the past; cooperative federalism is the future.[1]

This turn in federalism theory has often been overlooked in immigration law, perhaps because attention has focused on high-profile battles like the recent one between Arizona and the federal government. Frustrated by what it saw as a lack of federal initiative on immigration policy, Arizona decided to go it alone, passing a controversial law that created state penalties for violations of federal immigration law. Nearly all of Arizona's law was invalidated by the Supreme Court in *United States v. Arizona*—a decision that many interpreted as a paean to old notions of dual sovereignty.

The reality, however, is that arguments over state or local immigration sovereignty are largely a sideshow. The most pressing questions of immigration federalism today pretty much all concern "cooperative" arrangements between the federal government and nonfederal officials.[2] This is driven by the fact that immigration law has, in recent years, increasingly incorporated state and local law enforcement officials into federal immigration enforcement.

This is the deep irony of the Supreme Court's decision in *Arizona*. Largely overlooked in all of the controversy surrounding Arizona's law is a surprising fact: even while the Justice Department was arguing to the Supreme Court that local law enforcement officials in Arizona lacked authority to participate in immigration enforcement, the federal

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[1] See Gerken et al.

[2] The scare quotes reflect the reality that these efforts often do not involve cooperation on the part of state officials that is particularly voluntary. Many different forms of state-federal coordination often travel under the cooperative federalism label. Situations in which state or local officials play a role implementing putatively federal policies are a subset of this larger cooperative federalism space.

government was busy rolling out a program that incorporated those same officials into the federal enforcement scheme. That program, known as "Secure Communities," has a straightforward goal: to ensure that every person arrested for a crime by local police anywhere in the country is screened by the federal government for immigration violations.

Secure Communities is in many ways an ideal testing ground for many of the theories that dominate the contemporary federal literature. One such theory is that cooperative arrangements give local officials to much control over federal policy. This is among the charges leveled by critics of Secure Communities: they argue that especially in a world where immigration policy is determined largely through the exercise of enforcement priorities, turning every local criminal arrest into an immigration screening event puts local officials in charge of those priorities.

A second important theory—in some ways the opposite of the first—is that state-federal integration threatens to undermine state priorities and programs. This theory is nascent in much existing federalism literature, and it has been among the central challenges to Secure Communities. The program's critics have argued that incorporating local law enforcement officials into the immigration enforcement system will under the operation of the local criminal justice system. Immigrants, especially those living in the United States without authorization, will come to fear the local police. As immigrant communities become more insular and less willing to engage with local officials, the job of preventing and solving crimes will become tougher for local police. In fact, partly because of such concerns, a growing number of local communities have begun to publicly resist Secure Communities in recent months.

For all of the theorizing about the way in which cooperative federal arrangements might infect or undermine local policies and politics, evidence has been difficult to come by. This Article's goal is to provide just such evidence, and to do so in a policymaking context that is doubly divisive because it involves long-standing debates on twin fronts—heated debates about appropriate role of state and local actors in American immigration law, and sharp disagreements about the consequences of integrating the immigration and criminal justice systems. In short, we seek to expose the real world of immigration federalism by asking a critical question that has thus far gone unanswered: Does integrating criminal and immigration enforcement systems undermine the system of criminal justice and ultimately make it more difficult for local actors to prevent and solve crimes?

To get insight into this general question we focus on the particular program of Secure Communities. Two aspects of the program make it an uniquely valuable vehicle for our inquiry. The first is the sheer scope of the program. Never before has the federal government attempted to integrate every single local law enforcement agency in the country into the project of federal immigration enforcement. This massive intervention means that, rather than investigating cooperation in a handful of jurisdictions, we can observe its effects in over 3,000 counties. Moreover, participation in the core fingerprint screening component of the program is mandatory.

The second feature of the program is that, for our purposes, it approximates a natural experiment. The scope of Secure Communities prevented the Department of Homeland Security from activating the program everywhere at once. Instead, Secure

Communities was rolled out around the country over a period of four years. That staggered activation makes the program a unique, quasi-natural experiment that allows us to isolate empirically the effect of the program on local policing and criminal justice outcomes.

Capitalizing on these twin features yields a startling result: we find no evidence that activating Secure Communities in a community undermines the effectiveness of local law enforcement. The rate at which local police solve FBI index crimes is not affected by the program's implementation. This result is the same when we focus not on program activation but instead on a direct measure of the program's intensity in each community--the number of noncitizens detained by federal immigration agents under the program each month. Thus, there is little evidence that Secure Communities' cooperative federal arrangement undermines local policing by sowing generalized distrust among immigrants, or by shaping their rational risk assessments about police contact. It is possible, of course, that the program's effects are isolated in particular counties (in other words, that the program has heterogeneous treatment effects). But focusing on counties where the theory might predict the largest effects—like counties with large foreign born or Hispanic populations—generates the same result.

When we focus on the individual crimes that make up the FBI index our results remain largely unchanged. A few crimes, like murder and aggravated assault, actually have a slightly higher clearance rate after Secure Communities than before. Motor vehicle theft is the only of the seven FBI index offenses to show a statistically significant drop in clearance rate, but the magnitude of the change is less than one percent—smaller even than the tiny increases we find for murder and aggravated assault, and dwarfed by the generally high levels of volatility in clearance rates across communities.

In addition to undercutting arguments that cooperative immigration federalism programs like Secure Communities make it harder for local police to solve violent crimes and property offenses, our findings call into question a related claim: that increasing immigration into a community makes it harder for the police to solve crimes. This claim, long associated with theories of social disorder, has been repeatedly confirmed in cross-sectional studies of communities with different shares of immigrants. In contrast, our findings, which allow us to isolate the effect of changes in the size of the immigrant population within a community, rather than across communities, suggest that a larger foreign born population is correlated with a *higher* clearance rate for crimes. This suggestive evidence points to the need for a broader reassessment—not just of theories about cooperative immigration federalism's effects, but of more general theories about the relationship between immigration and law enforcement.

The paper proceeds in four parts. Part I lays out the basic claim that local law enforcement can be undermined by involvement in federal immigration enforcement. Part II unpacks the theory implicit in this common claim, showing that it often comes in two flavors—one sounding in rational choice, the other in procedural justice. Part III describes the how the unique policy experiment of Secure Communities permits us to get empirical purchase on the theoretical claims about cooperative immigration federalism. Part IV describes and discusses our results.

I. The Dark Side of Immigration Federalism

American federalism is often understood as a means for giving non-federal political institutions a voice in national politics. Yet American federalism theory also embodies, at least implicitly, a corollary idea: that federalism can undermine localism. Integrating local institutions into a national political and policy environment, the argument goes, can distort local politics and policies. Historical examples of this sort of "infection" are legion. Consider four:

> 1. Prior to the adoption of the Seventeenth Amendment, state politicians worried that tying the election of state legislators to the selection process for U.S. Senators distorted state politics. Voters would select the state legislator whom they thought would pick the right Senator to send to Washington, rather than the legislator who would promote the right state policies.
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> 2. National political actors, knowing that state-level initiatives affect the run of national politics, pour enormous sums of out-of-state money into state initiative campaigns (as with Prop 8 in CA, marijuana legalization in Washington, etc.) To the extent this cash has an effect, it undermines the extent to which the state's policy reflects the preferences of the state polity.
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> 3. The federal government offers up matching funds to subsidize state workplace safety inspections. Even though the state's first preference would instead be to focus on a environmental protection initiative, the availability of matching funds leads the state to pursue workplace safety in lieu of environmental protection initiatives.
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> 4. The federal government obligates local law enforcement officials to undertake background checks on prospective gun buyers. To do so, those officials divert resources that previously had gone to a diversion program for minor drug offenders.

The mechanisms of infection in the above examples are diverse. But the end result is that some form of coordination between our two levels of government ends up shaping non-federal politics or policy outputs.

Concern about this sort of infection has been longstanding in discussions about cooperative immigration federalism. The worry is that integrating local actors into federal immigration enforcement can undermine the functioning of important local institutions—like public education or criminal justice. In the immigration context, a big part of the reason for the worry has to do with the centrality of *information* to immigration enforcement. Information about the identity and whereabouts of unauthorized immigrants is hard for the federal government to come by. Federal agents can go out and look for immigration violators—in workplaces and local jails, on trains and buses, perhaps on street corners—but such efforts are resource intensive. A superior strategy, from a resource perspective, is to capitalize on the fact that states and local governments have many, many more contacts with residents and hence many more opportunities to identify those who are living in the United States in violation of immigration law. These contacts—which occur whenever a local government enrolls a child in public school, arrests and books a person into jail, accepts an

application for a business license, and so on—are all points at which local government officials can collect identity information and pass it along to federal authorities.

Of course, noncitizens who are potentially in violation of immigration law understand this fact well. For that reason, their willingness to interact with local officials may be contingent on their belief that information about their identity and immigration status will not be shared with federal authorities. Plenty of state and local laws reflect this concern. In the 1990s, for example, New York and a number of other cities passed so-called "sanctuary city" laws that barred local officials from communicating information about a person's immigration status to the federal government. Local officials often defended these policies as an effort to protect the proper functioning of local programs by securing the trust of the immigrant community.[3] On the flip side, Alabama recently adopted a law—since invalidated by the courts—which required local schools to collect information about the country of birth and immigration status of all children enrolled in public school. While the Alabama statute did not explicitly authorize the state to share that information with federal authorities, the statute was apparently motivated by the idea that simply collecting the information would discourage the parents of unauthorized immigrant children from enrolling their kids in public school.

Recent efforts to integrate local law enforcement officials into federal immigration enforcement have heightened these concerns. Episodic informal cooperation between state and federal officials goes back decades. But over the last twenty years, two programs have expanded and regularized this cooperation. The first program, created by Congress in 1996, empowered the Attorney General to sign cooperative enforcement agreements with local law enforcement agencies. These so-called 287(g) agreements—named for the provision of the Immigration and Nationality Act that authorized them—could in theory have led to high levels of local involvement in immigration enforcement. But they never became widely used. At the program's peak fewer there were fewer than 75 participating jurisdictions, and over the past four years a large number of agreements have lapsed as the federal government has chosen to focus its efforts on a newer and much larger initiative.

That new initiative is "Secure Communities." Launched in 2008, the program's goal is straightforward: to ensure that every person arrested by local law enforcement anywhere in the United States has her fingerprints checked against a federal immigration database. In other words, the program replaced sporadic and piecemeal integration of local police with a system of universal and automated screening that relies on millions of arrests made every year by local law enforcement agencies.

Critics of the sort of cooperation created by Secure Communities and the 287(g) program—including some local sheriffs and police departments—have argued that the efforts will undermine the efficacy of the local criminal justice system. The ability of that system secure public safety by preventing and punishing crimes turns crucially, they argue, on cooperation with the police by local residents. But that cooperation may be hard to come by if immigrants believe that interacting with local law enforcement officials creates a risk that information about them or their loved ones will be passed along to federal immigration

[3] In 1996 Congress overrode these local ordinances in a provision of a larger immigration bill. That provision provided that "QUOTE"

authorities. The size and concentration of the unauthorized population makes this concern more salient today than at any other point of American history. Nearly half of the 25 million noncitizens living in the United States are here in violation of immigration law. And because of migration patterns and residential segregation, the numbers are often even more startling at the local level; it is easy to find neighborhoods where the majority of residents are either unauthorized immigrants or are related to someone who is.

II. Immigrant Trust and Law Enforcement Effectiveness

Theories of cooperative federalism are often vague about exactly how local involvement in national policies might interfere with democratic accountability or otherwise undermine local policies. Critiques of cooperative immigration federalism are no exception in this regard. Nonetheless, one way to flesh them out is to consider the implications of local immigration cooperation for two standard accounts of crime control: an instrumental account that focuses on expected sanctions, and procedural justice account that focuses on the legitimacy of law enforcement.

As we noted above, critiques of programs like Secure Communities and 287(g) typically employ the following logic:

1. The involvement of local law enforcement officials in federal immigration enforcement changes attitudes or beliefs among immigrants, reducing their expressed willingness to cooperate (or otherwise interact) with local law enforcement officials;

2. this change in attitudes alters immigrant behavior, making at least some noncitizens less likely to cooperate (or otherwise interact) with local police; and

3. this reduction in the level of cooperation with the police makes local law enforcement less effective.

This mechanism can be understood in at least two ways. First, it could be taken as an instrumental claim. Under Premise 1, an immigrant becomes less willing to interact with the police only when doing so makes it more likely that she or a loved one will be harassed, detained, or deported on the basis of immigration status. On this account, immigrants respond to the risk of a particular sanction, consistent with standard rational choice accounts of criminal behavior.[4]

While critics of cooperative immigration federalism sometimes seem to be pressing this sort of instrumental account, more commonly their claims sound in procedural justice.[5]

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[4] See Bentham; Becker.

[5] There is, we should note, a third theory that also crops up in the literature—a theory related to resource constraints. On that theory, local involvement in immigration enforcement diverts resources from crime fighting and thereby reduces policing efficacy. See, e.g., Parra-Chico, Maria Fernanda, An Up-Close Perspective: The Enforcement of Federal Immigration Laws by State and Local Police, 7 Seattle J. Social Justice 321 (2008). Depending on how local law enforcement agencies choose to allocate scarce resources, resource constraints could in theory affect even crimes that do not involve immigrants in any way. For Secure Communities, however, this theory is less directly applicable because the program piggybacks on existing arrest

The procedural justice account of criminal law—pioneered by Tom Tyler and others in response to the rise of rational actor approaches—argues that a person's willingness to obey the law turns mostly on her felt obligation to abide by the law, rather than on her beliefs about the risk of punishment.[6] On Tyler's account, a person's felt obligation to comply with the criminal law turns largely on extent to which she sees the police as legitimate, and that legitimacy is a product of perceptions of procedural justice—that is, of beliefs about whether the police make *just* decisions and treat people *fairly*.

While claims about procedural justice focused initially on *compliance* with the law, in recent years this literature has expanded its argument to encompass *cooperation* with institutions of law enforcement as well. Sunshine and Tyler, for example, deploy survey evidence to argue (a) that a person's belief about whether police practices are procedurally fair best predicts the extent to which the person views the police as legitimate, and (b) that the perceived legitimacy of the police best predicts a person's willingness to cooperate with the police—to report a crime, to provide information to the police, to help them find a suspect, or to otherwise interact with police in one's neighborhood.[7] On this account, once immigrants see the police as immigration enforcers their trust in law enforcement evaporates. Without this trust the police come to be seen by migrants as less legitimate, and this in turn makes immigrants less likely to assist the police—regardless of whether a particular interaction is likely to create a risk of deportation for the immigrant or a loved one.[8]

These competing theories of cooperation produce different predictions about Premise 2: that is, about *who* will cooperate less with police and about *how much less* they will cooperate. On instrumental account, integrating local police into immigration enforcement

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and booking practices. Only if local police change their arrest practices in response to the activation of Secure Communities—say, by choosing to arrest a much larger number of persons stopped for driving without a license—would the program potentially divert local resources from other law enforcement activities.

[6] In a way, the difference between the procedural justice and instrumental accounts tracks loosely a longstanding divide about why people comply with the law (as well as a very old disagreement in legal theory about the concept of law). Thus, we might say that Tyler's procedural justice framework is more Hartian then Holmesian.

[7] Sunshine and Tyler 2003 ("The procedural justice perspective argues that the legitimacy of the police is linked to public judgments about the fairness of the processes through which the police make decisions and exercise authority.").

[8] David A. Harris, *Immigration and National Security: The Illusion of Safety through Local Enforcement Action*, 28 Ariz. J. of Int'l & Comp. L. at 390-91 (2012) (arguing that the use of local law enforcement officials to enforce immigration law is bad law enforcement policy, degrading the ability of departments to accomplish their core mission of providing public safety by undermining the "relationship of trust" between immigrants and law enforcement); Jason G. Idilbi, *Local Enforcement of Federal Immigration law: Should North Carolina Communities Implement 287(g) Authority*, 86 N.C. L. Rev. 1710 (2008); National Council Of La Raza, A. Elena Lacayo, *The Impact Of Section 287(G) Of The Immigration And Nationality Act On The Latino Community*, available at http://www.nclr.org/images/uploads/publications/287g_issuebrief_pubstore.pdf ("In a survey of 54 police chiefs, deputies, and sheriffs, . . . [t]he majority of law enforcement officials . . . believed that [287(g)] agreements often severely hinder the ability of police to earn trust required to implement effective community policing strategies to fight criminal activity."); Robert Morganthau (former DA for Manhattan, 1975–2009), Opinion: The Police and Immigration: New York's Experience, Wall St. J. (May 18, 2010) ("When the immigrants perceive the local police force as merely an arm of the federal immigration authority, they become reluctant to report criminal activity for fear of being turned over to federal officials.").

will affect cooperation only when it creates a risk of deportation for a particular immigrant or her loved ones. The effect will thus grow as the share of unauthorized migrants in a community climbs. In contrast, the procedural justice account (at least as articulated by Tyler et al) predicts a more generalized effect on the level of cooperation by all immigrants, regardless of whether they are at risk of removal.[9] Relatedly, the instrumental account predicts that the magnitude of the effect on cooperation will be correlated with the degree of increased risk of deportation created by a new program of local police involvement. The effect will thus grow as the local enforcement initiative becomes more effective. In contrast, risk assessments are not central to the procedural justice account, which predicts that cooperation will be based on beliefs about whether it is just for local police to assist in federal immigration enforcement, whether such involvement will lead immigrants to be treated unfairly by the police and ultimately about whether involvement undermines the legitimacy of the police.

In practice, of course, one difficulty for the procedural justice literature is that actual levels of cooperation with the police—as opposed to attitudes about cooperation—are largely unobservable. That difficulty has led the procedural justice literature to focus almost exclusively on testing Premise 1, which can be thought of as an input into the procedural justice model. Premise 1 focuses on public attitudes, and most procedural justice scholarship involves large surveys that use a series of questions to tease out attitudes about police practices, about the police themselves, and about one's willingness to cooperate with the police or comply with the law. The connection between attitudes and cooperative behavior is presumed, as is the connection between cooperative behavior and the ultimate outcome of interest—police efficacy.[10] Thus, even if surveys accurately measure attitudes, for example, one can't always be confident that those attitudes will translate into behavior. Nor do we know whether "cooperation," as it is understood in these accounts, is really all that crucial to law enforcement success.

This paper aims to overcome this shortcoming by focusing directly on outputs rather than inputs. This allows us to relax (and potentially to test) some assumptions that are built into the prevailing theory. We capitalize on the fact that the ultimate output—reduced police efficacy as a result of reduced cooperation—is in principle observable. In fact, there is a large literature in criminology concerned with measuring the effectiveness of law enforcement. This literature focuses principally on "clearance" rates, where the clearance of a crime is defined as that crime being solved by an arrest or otherwise. Clearance rates thus provide a measure of how likely the police are to solve any particular category of crime. To be sure, the measure has its limitations, which we discuss more fully below. But relying on it allows us to test more directly the core claim at the heart of the debate over cooperative

[9] The former Los Angeles Chief of Police, William Bratton, has suggested this sort of generalized effect. See Bratton statement ("Although many local agencies have declined to participate in 287g . . . the publicity given to those departments that have signed on has had a chilling effect on communities nationwide."); see also Police Foundation, Anita Khashu, The Role Of Local Police: Striking A Balance Between Immigration Enforcement (2009) (emphasizing the "fragility of the relationship between the police and immigrants," and the way in which word of mouth about an isolated incident can create widespread difficulty in securing the cooperation of members of the immigrant community").

[10] As procedural justice scholars note, the "concern with cooperation develops from the recognition that effective crime control and disorder management depends on public cooperation with the police" Id. (quoting Sampson, Raudenbush, & Earls 1997).

immigration federalism: that integrating local police into immigration enforcement will undermine the local system of criminal justice.

In short, therefore, we combine and build on two literatures—one concerned primarily with the reasons why people cooperate with law enforcement, the other concerned with identifying conditions that promote or impair effective law enforcement. We connect theories of willingness to cooperate with the police (Premise 1) to actual law enforcement outcomes (Premise 3), something that the procedural justice literature has heretofore not done. Moreover, as we explain below, we also employ a more convincing identification strategy than is common in these literatures. Because nearly all of the existing work on procedural justice and clearance rates is cross-sectional, it cannot untangle the causal effects of particular policies on either attitudes about cooperation or on clearance rates. In contrast to this approach, we exploit a natural policy experiment to evaluate the causal effect of a change in police policy on the ultimate efficacy of the police.

III. Secure Communities and Local Policing

Secure Communities provides a unique policy intervention to explore the effect of local immigration cooperation on policing efficacy.

A. Secure Communities and Cooperative Immigration Enforcement

As we explained above, Secure Communities is at heart an information sharing program. It capitalizes on the fact that local law enforcement agencies around the country have long fingerprinted arrestees and forwarded those fingerprints to the FBI to be run against a criminal history database. Secure Communities takes those fingerprints and routes them to the Department of Homeland Security, where they are run through an immigration database. Thus every local arrest becomes a point of immigration screening.

Before Secure Communities, immigrant arrestees were identified principally through individual inmate interviews in local jails and prisons. These interviews were conducted by federal officials pursuant to the Criminal Alien Program, and by deputized local law enforcement officials under so called "287(g)" agreements.[11] These labor-intensive efforts were piecemeal. Federal personnel conducted these screenings in less than 15 percent of local jails and prisons, and local officials were authorized to do the screenings themselves in only about two percent of the nation's counties.[12]

Secure Communities shifted to a system of universal and automated screening such that *every single person* arrested by a local law enforcement official *anywhere in the country* would be screened by the federal government for immigration status and deportation eligibility. The program accomplished this through a technological innovation that piggybacks on standard arrest procedures. Traditionally, when a person is arrested and booked by a state or local law enforcement agency, his fingerprints are taken and forwarded electronically to the Federal Bureau of Investigation (FBI), which conducts a criminal background check and

[11] The name "287(g)" refers to section 287(g) of the Immigration and Nationality Act, 8 USC § 1357(g), the federal statute that authorizes the Attorney General to enter into these agreements.

[12] Cox and Miles 2013.

sends the results to the local agency. Secure Communities' innovation was to take the fingerprints received by the FBI and automatically and electronically forward them to DHS. DHS then compares the fingerprints against its Automated Biometric Identification System (IDENT), a database which stores biometric and biographical information on persons encountered by the agency in the course of its immigration-related or other activities. The database includes fingerprints of three categories of foreign-born persons: (1) noncitizens present in the U.S. in violation of immigration law, such as persons who were previously deported or overstayed their visas; (2) noncitizens who are lawfully in the United States but who might become deportable were they to be convicted of the crime for which they have been arrested; (3) citizens who naturalized at some date after their fingerprints were included in the database.

If the fingerprints match a set in the DHS database, DHS personnel evaluate the person's immigration status and decide whether to place a "detainer" (sometimes referred to as an "immigration hold") on the person. The detainer requests that the local law enforcement agency hold the person for forty-eight hours beyond the scheduled release, in order to permit ICE to transfer the person to federal custody for the initiation of deportation proceedings. The detainer thus allows the federal government to readily apprehend and place in deportation proceedings a noncitizen whom the local criminal justice system would otherwise release. This includes a person who otherwise would have been released because her arrest did not result in conviction, because she was granted bail pending the outcome of her criminal proceeding, or because she had completed her term of incarceration following conviction.

Secure Communities thus increases the likelihood that noncitizens arrested for crimes by local authorities will be identified by the federal government, apprehended by the immigration authorities (rather than released), and ultimately deported from the country. The program's ambitious scope makes it the largest expansion of local involvement in immigration enforcement in the nation's history.

B. Program Rollout as Policy Experiment

Secure Communities, unlike most federal policies and programs, could not be activated everywhere in the country at once. Resource bottlenecks, technological constraints, and the sheer scope of the task of communicating with the roughly thirty-one thousand booking locations around the country necessitated a staggered rollout. Over a period of four years, beginning on October 27, 2008, the federal government rolled out the program on a county-by-county basis. By spring of 2012, Secure Communities had been formally activated in all but a handful of counties, and by January 2013, it was completely activated nationwide. Figure 1 provides a visual representation of the pattern of rollout.

This staggered sequence of rollout creates a natural experiment in the detention and deportation of immigrant offenders, and we use this policy variation to identify the effect on crime of detaining noncitizen offenders. The ideal experiment for measuring the causal impact of immigrant detention on crime would be to assign a program of enhanced enforcement randomly to some jurisdictions and not to others. We argue that the timing of Secure Communities activation approximates this ideal.

The federal government determined the sequence of rollout. It prohibited local governments from formally opting out of Secure Communities even though elected officials in some localities wished not to participate.[13] Moreover, the program's structure made informal noncompliance with the screening system practically impossible. Once Secure Communities is activated in a county, local authorities have no way to share the fingerprints of arrestees with the FBI but not with DHS. The only way a local law enforcement agency could prevent DHS's immigration check from taking place would be to stop fingerprinting arrestees altogether, and we are aware of no local agency that has done so.[14]

In earlier work, we explored in detail the determinants of Secure Communities activation using proportional hazard analysis. We found that, while the timing of activation was not wholly random, it appeared to mirror federal enforcement priorities for *immigration* generally rather than for crime control. The strongest correlates of an early activation were a county's location on the southern border and the fraction of the county's population that was Hispanic. Although Hispanic and foreign-born populations correlate closely with each other, we found that, after controlling for other factors, only the Hispanic population fraction had a statistically significant relationship to activation timing. The federal government's decision to commence the program on the southern border, a flashpoint of popular debate over immigration policy, suggests that despite the allusion to public safety in the program's title, the federal government saw immigration regulation rather than crime control as Secure Communities' main purpose.

C. Measuring Local Immigration Involvement and Police Efficacy

Because Secure Communities was activated each month at the county level, our dataset is a panel of monthly, county-level observations. The observation period runs from 2004 to 2012: it terminates with the final year of available crime data, and it begins in 2004 to roughly balance the number of years before and after Secure Communities launch in late 2008. Each county-month observation includes three types of information: (1) measures of local involvement in federal immigration enforcement under Secure Communities; (2) measures of police efficacy; and (3) demographic and other county-level characteristics.

Our basic measure of increased local involvement in federal immigration enforcement is program activation. In addition, through a series of FOIA requests we obtained detailed data from the Department of Homeland Security on the intensity of the program's intervention in each county. This data includes number of fingerprint requests submitted to the program in each county and month, as well as the number of persons

[13] Initially, there was some confusion about whether Secure Communities was mandatory, in part because DHS failed to provide clear public guidance, and in part because the agency initially employed a practice of entering into Memoranda of Understanding with state governments (though not with local governments or law enforcement agencies). As soon as some states began to resist signing these agreements, however, the government made clear that the agreements were not required because the program required no actions by state or local officials; all that was required was a rerouting of the fingerprint data stream among the federal agencies (Office of Inspector General 2012).

[14] While law enforcement agencies are powerless to stop the immigration check, they can resist the program by refusing to honor detainer requests issued by Immigration and Customs Enforcement. We discuss this possibility below in Section 5.B.

detained by ICE under Secure Communities in each county and month.[15] Which of these measures of the policy's treatment is most appropriate turns in part on whether one adopts the instrumental or procedural justice account of cooperative immigration federalism's potential costs. If immigrant cooperation with local police is driven primarily by their trust in the police and belief in the legitimacy of the local criminal justice system, then the simple fact of Secure Communities activation might be the most appropriate treatment measure.[16] If, on the other hand, cooperation with the police is based primarily on beliefs about the risk that interacting with the police will lead to negative immigration consequences, then the rate at which noncitizens are actually detained under the program in any given county may be a more appropriate measure. Because this detention rate varies considerably across counties and within counties over time, we explore both in our analysis below.

We use the rate at which crimes are solved as our measure of law enforcement efficacy. Criminologists and other scholars have long argued that police become more likely to solve crimes when there are higher levels of community engagement and cooperation with the police.[17] The idea is that it police investigations will often fail in the absence of witnesses to provide leads, informants to provide tips, or victims who are willing to testify. Data on the rates at which crimes are solved, or "cleared," was obtained from the FBI's Uniform Crime Reports (UCR). The UCR provides detailed reports on crimes, arrests, and clearances for an index consisting of seven offenses: four violent crimes (murder, rape, robbery, aggravated assault) and three property crimes (burglary, larceny, and motor vehicle theft).[18] For purposes of the UCR, a criminal case may be cleared by arrest or by "exceptional means." The vast majority of offenses that are solved are cleared by an arrest, where the offender has be arrested, charged with the commission of the offense, and turned over to the court for prosecution. Cases are cleared by exceptional means in limited situations where elements beyond the control of the law enforcement agency prevent the agency from arresting and formally charging an offender that has been identified by the police. This might occur when, for example, the offender dies before he can be arrested.

In addition to making it easier for the police to solve crimes (or perhaps in part because of this fact), higher levels of cooperation with the police are also often thought to reduce crime rates.[19] If the level of immigrant cooperation with the police declined in the wake of Secure Communities' rollout, therefore, one might predict that crime rates would rise. In other work, we find that Secure Communities produced no meaningful change in

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[15] In addition to fingerprint submissions and detentions, ICE also provided us with the number of database hits, as well as the number of persons deported, in each county and month.

[16] This presumes, of course, that community members have information about the policy's activation in their community. If they learn about it only because they begin to notice that an increase in the number of local criminal arrestees who are getting turned over to federal immigration authorities, then the ICE detention figures may be more appropriate even from the procedural justice perspective.

[17] See, e.g., Reiss, 1971; Schulhofer et al 2011.

[18] Many of these crimes are the types that critics of cooperative immigration federalism contend will be harder to solve when local police are involved in some way in federal immigration enforcement. See, e.g., Harris, supra note __, at 392 (murder, robbery); Idilbi, supra note __, at 1731 (attempted murder, rape)

[19] See, e.g., Rob Sampson et all 1997.

the FBI Index crime rate. For that reason, we do not use crime rates as an alternative measure of police efficacy.

That said, our findings regarding crime rates helps alleviate one potential concern about the use of clearance rates as a measure of police efficacy. The concern is that if immigrant community members begin not to trust the police, they might shy away not just from cooperating with the police, but also from reporting crimes in the first place.[20] If Secure Communities caused more crimes to go unreported, the clearance rate might artificially rise, as the police would be able to devote their resources to solving a fewer number of offenses.[21] In light of our earlier findings, however, this possibility seems remote. We found that Secure Communities did not cause any decline in the FBI index crime rate. But the index crime rate, which is calculated on the basis of reported offenses, would have declined had the program actually led many people to stop reporting crimes. (Or, rather, the only way it would not have done so is if Secure Communities caused an increase in local crime, combined with a decrease in reporting that cancelled out the increase—a rather unlikely possibility.)

IV. Analysis and Results

Before diving into the core analysis of Secure Communities' impact on clearance rates, Table 1 provides some background, laying out summary statistics on the clearance rates of particular offenses. The patterns match the usual patterns you see for clearance rates in the criminology literature. Violent crimes are cleared much more often than property crimes. The gap in their clearance rates is nearly thirty percentage points. There are also much bigger differences in clearance rates within the category of violent crime than within property crimes. Two violent crimes—murders and aggravated assaults—were cleared more than half the time. By contrast, fewer than a third of robberies resulted in a clearance. Murder and aggravated assault also had the highest variance in their clearance rates, reflecting wide differences across counties in the frequency with which their police clear these offenses. Among property crimes, clearance rates were much lower and occupied a narrower range. Larceny had highest clearance rate among property offenses at 19.2%, and burglary had the lowest at 12.8%. Variance across counties was also much lower for property offenses than for violent crimes.

[20] Given the structure of Secure Communities, which functions as a point-of-arrest immigration screen, this might be particular true in situations where a person's report will result in the arrest of a friend or loved one.

[21] To be sure, it could also drive down clearance rates if the crimes that were no longer reported were ones that previously had been the most likely to be solved.

Table 1. Summary Statistics for Clearance Rates

Variable	Mean	Standard Deviation	N
All Index Crimes	.2799	.1322	292,551
Violent Crimes	.4529	.2181	265,909
Murder	.5135	.3581	73,522
Rape	.3738	.2764	168,252
Robbery	.2884	.2196	152,140
Aggravated Assault	.5256	.5384	258,867
Property Crimes	.1730	.1009	291,073
Burglary	.1277	.1065	277,584
Larceny	.1919	.1094	287,354
Motor Vehicle Theft	.1617	.1784	249,728
Other Crimes			
Simple Assault	.5642	.1983	265,434

Notes: Observations are monthly, county-level data from 2004-2012. Means and standard deviations are weighted by population.

To control for the numerous influences on county's clearance rate, ordinary least square regressions were estimated. The estimating equation took the form:

$$C_{it} = g(Activate_{it})\,\delta + X_{it}\,\beta + \alpha_i + \alpha_t + \varepsilon_{it},$$

where C_{it} is the clearance rate in county i at calendar month t. The clearance rate is defined as the number of offenses cleared divided by the total number of offenses.[22] The term $Activate_{it}$ is a variable representing whether Secure Communities is active in county i on date t. Several different functional forms of g(•) are used to capture the activation of Secure Communities. The vector X_{it} contains a set of county- and date-varying control variables that are commonly included in studies of crime.[23] The terms α_i and α_t are fixed effects for county i and calendar date t, respectively. The term ε_{it} captures the error. The regressions are weighted by a county's population, and the standard errors are clustered by county.

Table 2 reports the results of these regressions. The regressions in the first two columns employ a series of binary variables to measure the activation of the Secure

[22] In the FBI's UCR reporting protocol, clearances are linked to offenses, eliminating the concern that a clearance reported in a particular month might be associated with a crime reported in an earlier month.

[23] These include: the fraction of the county population that is foreign born, the fraction that is Hispanic, the fraction that is Black, the fraction of female-headed households, population density, median household income, and the number of sworn police officers per 100,000 residents. Police employment, median income, and population density are expressed in logs in the regressions.

Communities program in a county. These results are the differences-in-differences estimates of the program's impact. The last two columns of the table show regressions that include a measure of the intensity of the Secure Communities intervention. As described above, we measure intensity as the cumulative number of persons detained under the program in the county since Secure Communities was activated.[24] For each of these two measures of Secure Communities, Table 2 reports two specifications. In the odd-numbered columns, the regressions do not include fixed effects for each county. That is, they pool the data across counties. In the even-numbered columns, the regressions include fixed effects for counties, and therefore, the coefficients in even-numbered columns are panel estimates.

A. Immigrants and Clearance Rates

Before turning to the estimates of Secure Communities' impact, the estimated coefficients for two control variables deserve discussion. Panel A reports the estimates for the fraction of a county that is Hispanic and the fraction that is foreign-born. All the regressions included these covariates, but in order to conserve space in the tables, they are reported only in Panel A. When the regressions do not include fixed effects for each county (as in columns (1) and (3)), the fraction of persons who are foreign born within a county correlates strongly and negatively with the clearance rate of index crimes. For example, the estimate in column (1) implies that a ten percentage point increase in the fraction foreign born in a county (or roughly one standard deviation) correlates with a reduction in the clearance rate of nearly a three percentage points. Relative to an average clearance rate of just below 30%, this is a sizable impact. Moreover, the pattern is consistent with the view that immigrants are less likely to cooperate with law enforcement authorities. But this correlation reverses sign and loses statistical significance when the regression includes fixed effects, as in columns (2) and (4). For example, the estimated coefficient for the fraction of persons foreign born in column (2) is .1884.

The striking contrast in the estimates in Table 2 between columns (1) and (2) is attributable to whether comparisons are drawn across counties, or within counties over time. In column (1), fixed effects for counties are not included. The estimates are pooled and largely reflect comparisons between counties. For example, consider relatively less populous counties (those below the 75th percentile of the distribution of county populations) and compare them to more populated counties (those at or above the 75th percentile). Less populous counties had higher clearance rates and proportionately smaller immigrant populations than more populous ones. The average clearance rate for all index crimes was five percentage points higher in these less populous counties (29.2% versus 24.4%). At the same time, these less populous counties had on average proportionately smaller foreign-born (8.7% versus 23.1%) and Hispanic populations (11.3% versus 29.2%) than the most populous counties. The estimates in column (1) without fixed effects reflect these cross-sectional differences.

[24] To parallel the standard measure of crime rates, we express this as the cumulative number of detainees per 100,000 persons. Accordingly, the regressions in columns (3) and (4) use a variable that in the months after a county's activation, takes the value of the natural log of this ratio, and in the months before a county's activation, this variable is assigned the value zero. In each month following activation within a county, this variable captures how many noncitizens have been incapacitated by the program through that month.

Table 2. Impact of Secure Communities on Clearance Rate of FBI Index Crimes
OLS Regression Estimates

Explanatory Variable	(1)	(2)	(3)	(4)
Regression Specification A				
Activated	.0275	.0024	.0026	-.0018
	(.0203)	(.0045)	(.0049)	(.0013)
Fraction Foreign Born	-.2762**	.1884	-.2824**	.2157
	(.0903)	(.2656)	(.0924)	(.2531)
Fraction Hispanic	.0046	-.0362	.0114	-.0181
	(.0465)	(.2050)	(.0468)	(.1995)
Regression Specification B				
Activated x 75th Percentile of Fraction Pop. Foreign Born	.0277	-.0003	.0024	-.0019
	(.0218)	(.0049)	(.0049)	(.0013)
Activated x Below 75th Percentile of Fraction Pop. Foreign Born	.0254*	.0108	.0077	.0028
	(.0149)	(.0039)	(.0053)	(.0017)
Regression Specification C				
Activated x 75th Percentile of Fraction Pop. Hispanic	.0227	-.0024	.0018	-.0021
	(.0214)	(.0051)	(.0047)	(.0013)
Activated x Below 75th Percentile of Fraction Pop. Hispanic	.0357*	.0111*	.0105	-.0029*
	(.0183)	(.0041)	(.0066)	(.0015)
Regression Specification D				
Activated x Border County	.0462*	.0191**	.0074	.0029
	(.0249)	(.0063)	(.0048)	(.0012)
Activated x Not Border County	.0262	.0018	.0020	-.0023
	(.0199)	(.0044)	(.0050)	(.0013)
Regression Specification E				
Activated x First Year	.0369	-.0063	.0046	-.0026
	(.0245)	(.0084)	(.0053)	(.0021)
Activated x Second Year	.0154	.0041	-.0009	-.0012
	(.0206)	(.0052)	(.0052)	(.0014)
Activated x Third Year	.0344*	.0097*	.0102*	.0009
	(.0164)	(.0049)	(.0057)	(.0020)
Activated x Fourth Year	.0113	-.0060	-.0043	-.0012
	(.0085)	(.0051)	(.0044)	(.0026)
Measure of Secure Communities?	Indicator Variables	Indicator Variables	Indicator Variables x Persons in ICE Custody	Indicator Variables x Persons in ICE Custody
Includes County-level Fixed Effects?	N	Y	N	Y

Notes: ** $p < 0.05$, * $p < 0.1$. The dependent variable is the rate of clearance of the monthly index crimes. The table reports regression coefficients, with standard errors in parentheses. N = 292,551. Number of counties in sample = 2,985.

When the regressions include fixed effects for each county, the fixed effects help control for unobserved differences between counties, and the estimates in essence reflect comparisons drawn within a county over time. Here, the correlations between clearance rates and the Hispanic and foreign born populations disappear. Over this period, clearance rates in the most populous counties were flat, while they rose elsewhere. Between 2004 and 2012, the average clearance rate in counties in the 75th percentile of the population distribution fell by 0.3 percentage points, while in other counties, it rose by 2.7 percentage points. Over this period, the Hispanic population rose by 2.0 percentage points in these most populous counties, a change that was nearly identical to the 1.8 percentage point increase in other counties. For share of foreign-born persons, the changes were similar: an increase of 1.9 percentage points in the most populous counties, and 1.7 percentage points elsewhere. Accordingly, the movements in clearance rates within counties did not closely relate to the movements in the share of the immigrant population. This finding itself runs contrary to much of the (largely cross-sectional) literature on clearance rate, in which a persistent finding is that larger shares of immigrants in a community are associated with lower clearance rates.

B. Secure Communities' Impact

Panel A of Table 2 shows a set of baseline specifications. The estimates indicate that Secure Communities did not reduce clearance rates for FBI Index crimes. All but one of the estimates for Secure Communities are positive rather than negatively signed. All are small in magnitude, the largest is less than three percentage points (but again positively signed). None are statistically significant. The differences-in-differences estimate in column (2), for example, implies that Secure Communities raised the clearance rate of index crimes by about one quarter of one percentage point. Even if this estimate were statistically significant (which again it is not), it is miniscule relative to an average clearance rate for all index crimes of nearly 28%. The coefficient in column (4) is negatively signed. But this log-log specification in can be read as an elasticity, and its magnitude implies that a 10% increase in detentions under Secure Communities would lower the clearance rate by .018 percent. This implied response is very small, and again the estimate is not statistically significant.

DHS's stated objective for Secure Communities is the detention and removal of immigrant criminals. As we showed in earlier work, crime rates do not predict program rollout.[25] Instead, the pattern of rollout tracked general immigration enforcement priorities: DHS rolled out Secure Communities earlier in the counties closer to the southern border and in those with proportionately larger Hispanic populations. Moreover, federal detentions under the program have been concentrated in counties with the largest foreign-born populations.[26] The variation across activated counties in detention rates under the program implies that a single binary variable for activation measures with error the program's intensity, and estimates of the program's impact may suffer from attenuation bias. In addition, when a county's response to the program varies with its characteristics, such as the

[25] See Cox and Miles, Policing Immigration, Univ. Chi. Law Rev. (2013)

[26] See Miles and Cox, Does Immigration Enforcement Reduce Crime? J. Law and Econ. (forthcoming 2014).

proportion of immigrants in its population, the treatment effect will be heterogeneous. Each of the subsequent panels in Table 2 reports a different specification of the Secure Communities variables to overcome any measurement error and to test for heterogeneous effects.

Specification B decomposes the basic estimate into two components: one for counties that are likely to have high immigrant concentrations (measured as having shares of the foreign-born population at or above the 75th percentile) and one for counties likely to have low concentrations (below the 75th percentile). Specification C makes a similar comparison for counties with high and low proportions of their populations who are Hispanic, and Specification D makes a comparison between counties that are on and not on the southern border. The final specification decomposes the treatment variables by the year in which Secure Communities was activated in a county because DHS may have sought to introduce the program earlier in places where it would have the greatest impact.

None of these specifications changes the inference drawn from the baseline estimates: Secure Communities did not affect the clearance rate of FBI index crimes. Most of the estimates are statistically insignificant and small. The estimates that are largest in absolute value—such as an estimate of .0462 for border counties in column (1) and .0369 for counties activated in the first year of the program also in column (1)—are all positively signed, precisely the opposite of the prediction that the program may lower clearance rates. The largest negatively signed estimates are for counties with proportionately small Hispanic populations in column (4) and counties activated in the fourth year of the program in column (3). But again these (insignificant) estimates contradict the prediction that the impact of the program in these counties should be modest or even zero because the size of the affected population is proportionately small. On the whole, the results in Table 2 suggest that Secure Communities had no effect on the overall rate at which police clear FBI index crimes.

C. Sensitivity of the Estimates

Table 3 probes the sensitivity of the estimates to alternative specifications. It takes as a baseline the equation in column (3) of Specification (A) in Table 2, which employs county-level trends. Each column in Table 3 shows the coefficient on the (log of) the detention rates under Secure Communities or, where noted, a variation of it.

The first column in Table 3 reports a regression that includes county-specific trends. These specifications eliminate variation in clearance rates caused by factors that vary linearly over time and that are specific to individual counties. Identification of the impact of Secure Communities in these equations comes from within-county variation after netting out county-specific trends. The estimate in column (1) shows that county trends virtually no effect on the estimate.

In modeling the frequency with which police clear crimes, the size of the police force is likely to be an important factor, and the regressions include a measure of police employment per capita as a control variable. But the data for police employment are missing for several counties in some years. The missing values cause the observations to be dropped from the sample. The estimate in column (2) tests whether the estimates is sensitive to the

removal of these observations from the same cause the panel to be unbalanced. Although the estimate changes sign, it remains small and statistically insignificant.

Table 3. Testing the Sensitivity of our Clearance Rate Estimates

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Persons in ICE Custody	-.0013 (.0015)	.0022 (.0045)		-.0019 (.0065)	-.0028 (.0019)			.0053** (.0024)
Sets of Fingerprints Submitted						.0033 (.0027)		.0010 (.0074)
Persons Deported							.0016 (.0042)	-.0062 (.0065)
Persons in ICE Custody x L1 Persons in ICE Custody			-.0007 (.0067)					
Persons in ICE Custody x L2/L3 Persons in ICE Custody			-.0016 (.0069)					
Persons in ICE Custody x Noncriminal Persons in ICE Custody			.0036 (.0071)					
Change to Baseline Regression Specification	Include County-level Trends	Exclude Police per Capita		Persons in ICE Custody Measured as Flow	Persons in ICE Custody Measured per Foreign-born Person			

Notes: ** $p < 0.05$, * $p < 0.1$. The dependent variable is the rate of clearance of the monthly index crimes. The table reports regression coefficients, with standard errors in parentheses. Number of counties in sample = 2,985. In all columns but column (2), N = 325,462, and the number of counties in sample = 2,985. In column (1), N=306,244, and the number of counties = 3,113.

The equation in column (3) decomposes the detainees by their ICE classifications. A possibility is that the nearly zero baseline estimate results from aggregating the immigrant offenders with severe criminal histories together with others who have no criminal history. This might occur if the biometric identification of Secure Communities facilitated the arrest of serious offenders, thus increasing the clearance rate for violent crimes, while an unfavorable popular perception of the program reduced public cooperation, thus reducing the clearance rate for less serious crimes. In this circumstance, detentions of immigrants in the criminal categories of L1 and L2/L3 would correlate positively with the clearance rate, while detentions of immigrants without criminal histories would correlated negatively with it.

The estimates in column (3) do not bear out this prediction. All of the estimates are close to zero and statistically significant. Also, their signs are contrary to the prediction. The coefficients for the criminal categories of L1 and L2/L3 are negative, while that on noncriminal detainees is positive.

The regression in column (4) replaces the cumulative number of immigrants taken into federal custody under Secure Communities with the number taken into custody during that specific month. In effect, it measures the monthly "flow" of immigrants into custody under the program rather than the "stock." If the program shapes clearance rates principally by changing the probability of ICE apprehension, then this flow measure would be a more appropriate measure of the policy intervention. But this coefficient is almost identical to the baseline estimate. Column (8) changes the denominator of the detention rate measure. Instead of a county's total population, it measures detentions as ratio of the foreign-born population. This measure may more accurately reflect the relevant risk of detention because only immigrants are targets of the Secure Communities program. But the estimate in column (5) shows that this does not change the conclusion about the program's impact.

The regression in column (6) replaces the measure of Secure Communities detentions with an analogous measure of the rate of submission of fingerprints. If the program prompted police to alter their enforcement practices, such as by engaging in racial profiling of Hispanics, then the relevant measure of the program's intervention would be the rate at which police stop or arrest people rather than the rate at which ICE detains them. The measure in column (6) more closely proxies arrests by local police. The regression in column (7) employs the cumulative stock of deported immigrants rather than detained immigrants. This measure would be appropriate if Secure Communities increased only deportations rather than detentions, but as described above, it increases both. Column (8) includes all three metrics of the program, the cumulative submission, detention, and deportation measures. A possible theoretical justification for including all three measures is that it permits one to disentangle the effects of shorter- and longer-term incapacitation. Yet, such fine theoretical distinctions may not be possible in practice. ICE detains immigrants before deporting them, making these measures highly correlated. None of these specifications suggests a different conclusion about Secure Communities' impact.

All of the estimates in the last three columns of Table 6 imply relatively small effects on the clearance rate. Four of the five coefficients of interest in these regressions are positively signed, including the only one of them that is statistically significant. On the whole, the results in Table 3 suggest that the conclusion that Secure Communities has no impact on clearance rates is robust to different ways of measuring the program's intervention.

D. Estimates for Individual Crimes

Table 1 showed that the clearance rates of the individual offenses that comprise the FBI crime index vary widely, from under 15% to over 50%. These differences suggest that the processes that lead to an offense's clearance may differ substantially by the type of offense, and they raise the possibility that any impact of Secure Communities on clearance rates might also vary by offense category. Table 4 explores this possibility by presenting

regressions on clearance rates for each type of offense. For each category of offense, the table shows two regression estimates. The odd-numbered columns show the estimated coefficients on an indicator variable for program activation or the differences-in-differences estimate. The even numbered columns show the estimates when the detention rate measure is instead used to measure the program's intensity.

Table 4. Impact of Secure Communities on Clearance Rates of Specific Crimes
OLS Regression Estimates

	(1)	(2)		(3)	(4)
Violent Crimes			Property Crimes		
Murder	.0180**	.0071**	Burglary	.0045**	.0004
	(.0092)	(.0026)		(.0019)	(.0007)
Rape	-.0034	.0005	Larceny	.0027	-.0013
	(.0077)	(.0025)		(.0026)	(.0010)
Robbery	.0121**	.0019	Motor Vehicle	-.0094**	-.0030**
	(.0057)	(.0018)	Theft	(.0028)	(.0010)
Aggravated	.0141**	.0040*			
Assault	(.0065)	(.0022)			
			Other Crimes		
			Simple Assault	.0043	-.0007
				(.0092)	(.0028)
Measure of Secure Communities?	Indicator Variables	Indicator Variables x Persons in ICE Custody		Indicator Variables	Indicator Variables x Persons in ICE Custody

Notes: ** $p < 0.05$, * $p < 0.1$. The table reports regression coefficients, with standard errors in parentheses.

Larceny is the most common of the FBI index crimes, composing for over 60% the reported offenses in the index in 2012. It is therefore not surprising that the estimates for larceny in Table 4 are very close to the estimates for the overall index shown in Table 2. Yet, intriguing patterns emerge in the clearance rates for several of the less common (but still severe) offenses in the index. Five of the other offenses in the index show positive coefficients, several of which are statistically significant and sizable. For example, the differences-in-differences estimate for murder implies that Secure Communities raised its clearance rate by 1.8 percentage points.

The only offense category with negative and statistically significant estimates is motor vehicle theft. Yet, it is not clear that this result provides much support for the view that Secure Communities has impaired public trust. There are reasons to expect that Secure Communities should exert less of an influence on the clearance rate of this offense than on other offense categories. Motor vehicle theft is thought to suffer from less from under-reporting than other types of offenses because state registration and especially insurance requirements give motorists a strong incentive to report stolen vehicles. In addition, much

motor vehicle theft occurs through professional rings, and the sort of evidence that would facilitate the arrest of ring participants is likely different than the type of evidence garnered from public cooperation. Even if the estimates for motor vehicle theft are interpreted as support for the public cooperation hypotheses, when set against the broader set of results in Table 6, they offer very tepid support. The clearance rates of six of the seven index crimes correlate positively or not at all with Secure Communities.

The result for simple assault is also worth noting. Although it is not part of the FBI crime index, simple assault is a more common offense than any of the components of the FBI's index. It is also likely an offense that depends on public cooperate to clear through arrest. But the estimates for it are close to zero, inconsistently signed, and not statistically significant.

V. Conclusion

In short, we find no meaningful evidence that the largest integration of local police into federal immigration enforcement in the history of the United States undermined the efficacy of local law enforcement. This core finding calls into question many of the strong claims made by the literatures on cooperative immigration federalism and procedural justice. It also raises an obvious question: where did these claims go wrong? Sussing this out is beyond the scope of this project, but in closing we offer a few speculative thoughts.

First, theorists of cooperative immigration federalism may have been working with an excessively optimistic account of what immigrant-police relationships look like in the absence of local involvement in federal immigration enforcement. A longstanding finding in the procedural justice literature is that the communities most likely to have large numbers of immigrants--urban centers with large minority populations, higher rates of poverty, and so on--are places where there is already a considerable lack of trust in the police. If baseline levels of trust are low, there isn't much lower to go when a new program like Secure Communities is introduced. Relatedly, if immigrants (like many citizens) often view different "law enforcement" entities as a single undifferentiated mass--seeing local cops, federal investigative services like the FBI and DEA, and immigration enforcement arms like CBP and ICE as all of a piece—then changes in the extent of cooperation between these entities will actually have little effect on public attitudes.

Second, the procedural justice work on public attitudes about the police may have a reverse causation problem. As we described earlier, that literature makes quite detailed claims about how the public comes to hold particular beliefs about law enforcement officials. Beliefs about fair treatment are driven by actual police practices, and those beliefs in shape perceptions of police legitimacy, with legitimacy shaping willingness to comply with the law and cooperate with law enforcement. While there is no doubt some truth to this account, it also seem plausible that causation often runs the other way: that a person's perception of whether the police are legitimate shapes her beliefs about whether the police are likely to treat her fairly. To the extent causation runs in this direction, discrete policy interventions—even a widely publicized and highly salient one like Secure Communities—are extremely unlikely to have much of an effect on one's willingness to help out the police.

Third, accounts of law enforcement success that turn on the cooperation of the community at large, rather than on other law enforcement techniques may be overblown. The sort of cooperation contemplated by those accounts may be important for a limited set of offenses—perhaps paradigmatically for cases involving domestic abuse and other forms of violence among intimates. But other investigative techniques, including the very different sort of "cooperation" that is often obtained from co-conspirators or others involved in a criminal enterprise, may be much more important. Thus, even in a world where Secure Communities sows distrust of the police among immigrants, that distrust may not interfere with the bulk of what police do in order to solve most crimes.