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Legitimacy and Cooperation: Will Immigrants Cooperate with Local Police Who Enforce Federal Immigration Law?

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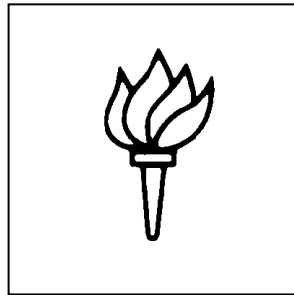
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LEGITIMACY AND COOPERATION: WILL IMMIGRANTS COOPERATE WITH LOCAL POLICE WHO ENFORCE FEDERAL IMMIGRATION LAW?

Adam B. Cox & Thomas J. Miles

THE LAW SCHOOL
THE UNIVERSITY OF CHICAGO

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LEGITIMACY AND COOPERATION:
WILL IMMIGRANTS COOPERATE WITH LOCAL POLICE
WHO ENFORCE FEDERAL IMMIGRATION LAW?

Adam B. Cox & Thomas J. Miles*

Solving crimes often requires community cooperation. Cooperation is thought by many scholars to depend critically on whether community members believe that law enforcement institutions are legitimate and trustworthy. Yet establishing an empirical link between legitimacy and cooperation has proven elusive, with most studies relying on surveys or lab experiments of people’s beliefs and attitudes, rather than on their behavior in the real world. This Article aims to overcome these shortcomings, capitalizing on a unique natural policy experiment to directly address a fundamental question about legitimacy, cooperation, and law enforcement success: do de-legitimizing policy interventions actually undermine community cooperation with the police? The policy experiment is a massive federal immigration enforcement program called Secure Communities. Secure Communities was widely criticized for undermining the legitimacy of local police in the eyes of immigrants, and it was rolled out nationwide over a four-year period in a way that approximates a natural experiment. Using the rate at which police solve crimes as a proxy for community cooperation, we find no evidence that the program reduced community cooperation—despite its massive size and broad scope. The results call into question optimistic claims that discrete policy interventions can, in the short run, meaningfully affect community perceptions of law enforcement legitimacy in ways that shape community cooperation with police.

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INTRODUCTION

Cooperation has long been a lynchpin of law enforcement success. Without the assistance of community members, the criminal justice system frequently fails to punish and prevent crimes.¹ Given cooperation's importance in securing public safety, it is critical to understand why (and when) community members choose to talk to the police, testify at trial, and otherwise help public officials combat crime.

Two leading theories seek to explain community cooperation with law enforcement. The first, developed by Wesley Skogan and others, is built on rational choice theory.² According to this view, individuals help the police when the material and psychic benefits of doing so outweigh the costs. A rival account, much discussed today, is the theory of procedural justice. Developed most prominently by Tom Tyler, this theory posits that a person's felt obligation to cooperate with police depends largely on her judgment about how police treat her and, ultimately, on her belief that the police are a legitimate institution.³ On this account, legitimacy becomes crucial to law enforcement success, because legitimate institutions more effectively secure the cooperation of community members.

Procedural justice theory dominates contemporary policy debates. Myriad law enforcement policies and practices—from stop-and-frisk tactics, to zero-tolerance policing, to drug courts, to the treatment of mentally ill offenders, to plea bargaining, to judicial performance, to sentencing—have all been attacked or defended by scholars, policymakers, and advocates on procedural justice grounds.⁴ But for all the theorizing, there is remarkably little evidence about the extent to which legitimacy shapes community cooperation with law enforcement. Nearly all existing efforts to test the theory suffer from two shortcomings. First, they typically measure public *beliefs* but tell us little about actual *behavior*. Second, they often are not well-designed to identify causal effects: they observe large differences in expressed beliefs about police legitimacy, but are unable to determine whether these beliefs can be meaningfully influenced by discrete policy interventions.

This Article aims to overcome these shortcomings, capitalizing on a unique natural policy experiment to directly address a fundamental question about

¹ See Albert J. Reiss, Jr., *The Police and the Public* (1971); *infra* Part I.

² See Wesley G. Skogan, *Citizen Reporting of Crime: Some National Panel Data*, 13 CRIMINOLOGY 535 (1976); *infra* text accompanying notes []-[].

³ See TOM R. TYLER, *WHY PEOPLE OBEY THE LAW* (2006); *infra* text accompanying notes []-[].

⁴ See K. Babe Howell, *Prosecutorial Discretion and the Duty to Seek Justice in an Overburdened Criminal Justice System*, 27 GEO. J. LEGAL ETHICS 285 (2014); Michael M. O'Hear, *Plea Bargaining and Procedural Justice*, 42 GA. L. REV. 407 (2008); Thomas L. Hafemeister, Sharon G. Garner, & Veronica E. Bath, *Forging Links and Renewing Ties: Applying the Principles of Restorative and Procedural Justice to Better Respond to Criminal Offenders with a Mental Disorder*, 60 BUFF. L. REV. 147 (2012); Michael M. O'Hear, *Rethinking Drug Courts: Restorative Justice as a Response to Racial Injustice*, 20 STAN. L. & POL'Y REV. 463 (2009); Adam Lamparello, *Incorporating the Procedural Justice Model into Federal Sentencing Jurisprudence in the Aftermath of United States v. Booker: Establishing United States Sentencing Courts*, 4 N.Y.U. J. L. & LIBERTY 112 (2009).

legitimacy, cooperation, and law enforcement success: do de-legitimizing policy interventions actually undermine community cooperation with the police? The policy experiment, known as “Secure Communities,” had the ambitious goal of ensuring that every single person arrested for a crime by local police anywhere in the country would be screened by the federal government for immigration violations.⁵ Immigration screening upon local arrest, virtually unheard of prior to the program’s launch in 2008, was universal in the United States by the end of 2012. The program led to the largest-ever integration of local law enforcement agencies into the process of enforcing federal immigration law.

Secure Communities has been widely attacked on procedural justice grounds. Immigrants’ rights activists and prominent law enforcement officials have both argued that the program threatens to undermine the legitimacy of local law enforcement officials in the eyes of the immigrant community. By entangling local law enforcement in federal immigration enforcement, critics worry, the program will erode immigrant trust in the police and lead immigrants to view the police as less legitimate. The loss of trust and legitimacy will make immigrants shy away from cooperating with law enforcement officials trying to solve crimes.

Recently (though after our study period), these concerns have sparked an explosion of local policies designed to bolster the legitimacy of local police in the eyes of immigrants. To date, well over a hundred cities and counties have adopted policies resisting Secure Communities. These “sanctuary city” policies cannot prevent the now-universal fingerprint-screening system at the heart of Secure Communities. That component is mandatory. Instead, New York, San Francisco, and other sanctuary cities have refused to honor requests from the federal government, known as detainers, that ask the local government to hold an immigrant for forty-eight hours so that federal agents can take custody of the immigrant.⁶ These anti-detainer ordinances are necessary, these cities have argued, because local law enforcement must disassociate itself with the federal immigration enforcement bureaucracy in order to secure the trust and cooperation of immigrants who otherwise would see local police as illegitimate.

The widespread view that Secure Communities corrodes trust and perceived legitimacy makes it an ideal vehicle for testing empirically the leading theory of cooperation’s foundation. Procedural justice theory predicts that a program like Secure Communities—one widely thought to undermine the legitimacy of local police—should reduce levels of cooperation by immigrants. Levels of cooperation are nearly impossible to observe directly—at least in any large-scale way—but

⁵ See Adam B. Cox & Thomas J. Miles, *Policing Immigration*, 80 CHI. L. REV. 87 (2012).

⁶ San Francisco’s policy has recently been in the national spotlight as the result of a local murder committed by a noncitizen whom the Sheriff’s Office had released rather than handing over to the federal government. See Editorial Board, *Lost in the Immigration Frenzy*, N.Y. TIMES, July 13, 2015. Sergio Garcia, *Sanctuary Laws Build Trust and Protect Working People*, N.Y. TIMES, July 9, 2015, <http://www.nytimes.com/roomfordebate/2015/07/09/should-immigrant-sanctuary-laws-be-repealed/sanctuary-laws-protect-working-people-and-build-trust>.

fortunately we have a powerful proxy available to us. If community cooperation is crucial for law enforcement success, then less cooperation will make it harder for the police to prevent and punish crimes. Thus, our key prediction is that any impact Secure Communities has on community cooperation should be detectable in “clearance rates,” the rates at which the police solve crimes.⁷

Two unique features of Secure Communities makes it doubly attractive for our inquiry. First, the program did not directly affect local police tactics or resources. This gives us greater confidence that any observed effects are due to changes in perceptions of police legitimacy rather than due to alterations in police tactics or effort.

Second, and perhaps more important, Secure Communities approximates a natural experiment. The program’s ambitious scope prevented the federal government from activating it everywhere at once. Instead, Secure Communities was rolled out around the country over a period of four years, from 2008 to 2012. The staggered activation makes it possible to separate causation from correlation, a foundational problem that has plagued procedural justice literature for years. In fact, so far as we are aware, this Article represents the first-ever use of a natural policy experiment to test procedural justice theory’s core predictions.

We find no empirical evidence that activating Secure Communities in a community impairs the ability of the police to solve crimes. We first estimate the effect of the program on the rate at which local police solve FBI index crimes—seven serious crimes that include murder, rape, robbery, aggravated assault, arson, burglary, and larceny. Our results are so precisely estimated that we should detect even relatively small declines in a county’s clearance rate for FBI index offenses, yet the estimates show no evidence of any decline in clearance rates. Nor do the crime rates themselves appear affected by activation. Moreover, these findings remain unchanged if we shift from a simple binary measure of program activation to a direct measure of the program’s intensity in each community.

It is possible, of course, that the program’s effects are isolated in particular counties. Counties with large foreign-born and Hispanic populations are likely to contain proportionately more immigrants, and these locations are where the program is most likely to have the largest effect. But even when the analysis focuses on these locations relative to others, no effect is found.

When the crimes that make up the FBI index are considered individually, the results remain largely unchanged. A few crimes, like murder and aggravated assault, actually have slightly higher clearance rates 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 this change is less than one percent—smaller even than the tiny increases seen for murder and aggravated assault, and dwarfed by the generally high levels of volatility in clearance rates across

⁷ As we explain more fully below, we also look for any evidence that the program reduces the rate of at which community members report crimes.

communities. On the whole, the estimates suggest that Secure Communities, a program frequently alleged to erode trust in the police, did not affect the rate at which police cleared crimes.

These striking finding might be interpreted in two ways. One possibility is that perceptions of legitimacy do not powerfully affect people's level of cooperation with the police. Perhaps people decide whether to cooperate for reasons—such as self-interest, or a sense of personal morality—that have little to do with whether they trust the police or perceive the police as legitimate. This conclusion would have profound implications for a number of debates about policing in diverse societies and about the role of procedural justice in securing cooperation with law enforcement.

The other possibility, of course, is that the oft-asserted claims about Secure Communities' de-legitimizing effects were incorrect. Perhaps integrating local police into federal immigration enforcement did not meaningfully undermine immigrant communities' trust and confidence in the local law enforcement. This conclusion would cast doubt on the legitimacy-based arguments against cooperative immigration federalism and in favor of sanctuary city policies.⁸ More importantly, it would be a serious blow to the practical relevance of procedural justice theory for public policy. It would call into question the theory's optimism about the ability of discrete policy interventions to significantly change public attitudes about the police. Over the long run, of course, one hopes that well-designed public policies can promote the legitimacy of law enforcement institutions and the cooperation engendered by that legitimacy. But public beliefs about the legitimacy and trustworthiness of the police may be far too sticky for an individual law enforcement program, even one as massive as Secure Communities, to significantly alter those beliefs over the short run.

The Article proceeds in three parts. Part I unpacks the dominant theories of why people cooperate with the police. Part II explains the how the unique policy experiment of Secure Communities permits us to get empirical purchase on the theoretical claim that trust-eroding policing policies will undercut community cooperation with the police. Part III discusses our results and their implications—both for debates about the connection between procedural justice and cooperation, as well as for current controversies over cooperative immigration federalism.

⁸ Together with other work we have done, this would suggest that both sides in the debate over Secure Communities were wrong. Supporters of the program have been wrong to claim that it was a critical crime control policy. For, as we have shown, Secure Communities had no effect on crime rates. See Thomas J. Miles & Adam B. Cox, *Does Immigration Enforcement Reduce Crime? Evidence from Secure Communities*, 79 J.L. ECON. (2014).; see also Kirk Semple, *Deportations Have 'No Observable Effect' on Crime Rate, Study Concludes*, N.Y. TIMES, Sept. 3, 2014 (discussing the findings of our study of Secure Communities' effect on crime). But on this interpretation of our clearance rate finding, critics of the program would also have been wrong to claim that the program undermines law enforcement by destroying immigrant trust in the police.

I. POLICE LEGITIMACY AND COMMUNITY COOPERATION

The social production of community safety is crucial to understanding crime. Much crime control is due to voluntary compliance and informal social sanctions for law breaking.⁹ Even when one focuses in on public enforcement as a strategy for preventing and punishing crimes, community involvement remains important. Community members report crimes to the police, assist investigations by providing tips and eyewitness accounts, testify at criminal trials, and cooperate in myriad other ways.¹⁰ Because this sort of cooperation is widely seen as crucial to law enforcement success, persuading community members to undertake these activities has long been a central goal of law enforcement agencies.

The community policing revolution, which began in the 1960s and 1970s, is perhaps the most salient historical example of these efforts to build police-community relations and promote cooperation with the police.¹¹ But the antecedents to this movement can be traced back much earlier in the twentieth century, to reforms begun in the progressive era to professionalize police forces.¹² And today the movement's progeny are everywhere, in discussions about order-maintenance policing, community-based prosecution, and countless other law enforcement initiatives.¹³ This history raises two crucial questions, one theoretical and the other empirical. First, what theories explain why individuals cooperate with the police and how this cooperation is affected by law enforcement policies? Second, how do we test these theories of cooperation in a world where the behavior at issue—cooperation—is extremely difficult to measure directly in the real world?

A. Mechanisms of Cooperation

An enormous literature investigates why people cooperate with the police. The theories around which this literature is organized can be lumped into two broad categories. The first focuses on instrumental mechanisms; the second emphasizes mechanisms that turn on the legitimacy of legal institutions.

1. Instrumental Mechanisms

Early research on community-police relations focused on a fundamental form of cooperation: the individual victim's decision to report the incident to the police. Wesley Skogan's influential 1976 article drew attention to three factors influencing

⁹ See ROBERT J. BURSIK, JR. & HAROLD G. GRASMICK, *NEIGHBORHOODS AND CRIME* (1993); CLIFFORD R. SHAW & HENRY D. MCKAY, *JUVENILE DELINQUENCY AND URBAN AREAS* (1942); Robert J. Sampson & W. Byron Groves, *Community Structure and Crime: Testing Social-Disorganization Theory*, 94 AM. J. SOC. 774 (1989).

¹⁰ See REISS, *supra* note 1, at []-[].

¹¹ See DAVID SKLANSKY, *DEMOCRACY AND THE POLICE* (2008).

¹² See *id.* at [].

¹³ See sources cited *infra* at notes []-[].

this decision: the characteristics of the victim, the relationship between the victim and offender, and the gravity of the crime.¹⁴ Skogan's framework broke sharply with prior approaches, which characterized the failure of victims to report crime as a "social pathology" and "individual failure." In place of this moralizing, Skogan proposed a framework of rational decisionmaking in which "[p]eople report or not for good reason."¹⁵ Much of the subsequent research on crime reporting has continued to operate either explicitly or implicitly within this rational choice framework.

The potential benefits of reporting range from the self-interested to the altruistic. For property crimes, reporting may be a first step in securing return of stolen property, and insurers may require it before indemnifying a loss. For all crimes, reporting the offense may be a pre-condition to obtaining compensation from offenders for the harm imposed. Where compensation is not possible—perhaps because the offender lacks resources—a victim may report in order to help the police catch and convict the offender. Conviction and punishment may offer the victim the emotional satisfaction or psychological consolation that the wrong has been corrected and justice meted out to the wrongdoer. Punishment may also serve the victim's altruistic desire to prevent the offender from inflicting similar harms on others in the community. All of these benefits may increase with the seriousness of the offense, and empirical studies have consistently found that more serious offenses are reported at higher rates.¹⁶

Yet crime victims can obtain these benefits only if they are also willing to bear the costs of reporting crimes. In many cases these costs may be relatively minor—such as the hassle of traveling to a police station, explaining the incident to an officer, and filing out the paperwork. But even these "minor" costs can disrupt work and family obligations, especially if repeated interviews with police and multiple court appearances are necessary. And for some crimes, particularly sex offenses, the act of reporting to the police and testifying can be substantial burdens. It can cause embarrassment, shame, and emotional harm for the victim—harms some victims' rights advocates describe as a second victimization.¹⁷ The victim's dignity may also suffer if the police fail to respect the victim.¹⁸ Worse, these burdens may come with

¹⁴ See Wesley G. Skogan, *Citizen Reporting of Crime: Some National Panel Data*, 13 CRIMINOLOGY 535 (1976) [hereinafter Skogan, *Citizen Reporting of Crime*].

¹⁵ *Id.* at 536.

¹⁶ See Skogan, *Citizen Reporting of Crime*, *supra* note [], at 544-47; Michael R. Gottfredson & Michael J. Hindelang, *A Study of the Behavior of Law*, 44 AM. SOCIOLOGICAL REV. 3 (1979).

¹⁷ Alan J. Lizotte, *The Uniqueness of Rape: Reporting Assaultive Violence to the Police*, 31 CRIME & DELINQUENCY 169 (1985).

¹⁸ Fisher et al. found that college women who were victims of sexual assault were more likely to report the offense to the police when its nature made the victim more credible such as the presence of a weapon, the offender was a stranger, and the victim had not consumed alcohol. Bonnie S. Fisher et al. *Reporting Sexual Victimization to the Police and Others: Results from a National-Level Study of College Women*, 30 CRIMINAL JUSTICE & BEHAVIOR 6, 27-30 (2003).

no offsetting benefits if the police are unable to locate and convict the offender.¹⁹ This may induce victims to report only when they possess information likely to help police apprehend the offender. The failure of police to arrest and convict the wrongdoer might, in turn, further aggravate and emotionally harm the victim.

Victims may also worry about retaliation. Social networks, or the police investigation itself, may inform the offender that the victim has reported the offense and perhaps incriminated the offender specifically. The fear of violent reprisal from the offender or his confederates may further discourage reporting.²⁰ Finally, victims may fail to report a crime because the context of the offense may reveal to the police the victim's own law-breaking.²¹ A standard example is that a drug dealer may fail to report a theft of his drug inventory or its cash proceeds. Immigrants without legal status may also be reluctant to report crimes because identifying themselves to police may expose them or their companions to immigration enforcement actions and ultimately deportation.²²

2. *Legitimacy and Procedural Justice*

A competing theory of cooperation is that people cooperate with legal authorities because they feel obligated to do so—not simply because doing so serves their own interests. This sense of obligation arises when people perceive legal authorities to be *legitimate*. On this psychological account, therefore, people's beliefs

¹⁹ Police may not “solve” a crime when they fail to exert effort or when information necessary for apprehension and conviction is not available. See Skogan, *Citizen Reporting of Crime*, *supra* note [], at 548-49; Wesley G. Skogan & George E. Antunes, *Information, Apprehension, and Deterrence: Exploring the Limits of Police Productivity*, 7 J. CRIM. JUSTICE 217 (1979) (arguing that if police rely on the information readily available to them rather than acquiring information from witnesses and surveillance, apprehension rates will not rise).

²⁰ Simon I. Singer, *The Fear of Reprisal and the Failure of Victims to Report a Personal Crime*, 4 J. QUANT. CRIMINOLOGY 289 (1988).

²¹ Mark T. Berg et al., *Illegal Behavior, Neighborhood Context, and Police Reporting by Victims of Violence*, 50 J. RESEARCH CRIME & DELINQUENCY 75, 87-92 (2013) (finding in a survey of youth that self-reported involvement in criminality reduces the willingness to report being a victim of robbery or assault).

²² Elizabeth Fussell, *The Deportation Threat Dynamic and Victimization of Latino Migrants: Wage Theft and Robbery*, 52 SOCIOLOGICAL QUARTERLY 593 (2011) (presenting survey evidence that that Latinos working construction jobs in New Orleans after Hurricane Katrina were more likely to be victims of wage theft because employers believed the risk of deportation dissuaded their reporting it); Dane Hautala et al., *Predictors of Police Reporting among Hispanic Immigrant Victims of Violence*, RACE & JUSTICE (forthcoming 2015) (reporting a 32% reporting rate in this sample and that it rises with the seriousness of the offense). *But see* Callie Marie Rennison, *Reporting to the Police by Hispanic Victims of Violence*, 22 VICTIMS & VIOLENCE 754, 759 & 761-63 (2007) (finding modest differences in rates at which Hispanics report assaults relative to non-Hispanics, but the data do not include measures of immigrant status); *see also infra* text accompanying notes []-[].

and attitudes about the legitimacy of a legal institution—not just their narrow self-interest—affects the extent of their cooperation with that institution.²³

What shapes public beliefs about whether the police are legitimate? There are any number of possibilities, but today the dominant explanation is supplied by procedural justice theory. Developed by Tom Tyler, the theory is a legitimacy-based account of why people obey the law and cooperate with the police.²⁴ As Tyler himself has described the theory:

The procedural justice model involves two stages. The first involves the argument that public behavior is rooted in evaluations of the legitimacy of the police and courts. People's social values—in this case, their feelings of obligation and responsibility to obey legitimate authorities—are viewed as key antecedents of public behavior. In other words, people cooperate with the police and courts in their everyday lives when they view those authorities as legitimate and entitled to be obeyed.

The second involves the antecedents of legitimacy. The procedural justice argument is that process-based assessments are the key antecedent of legitimacy (Tyler 1990). In this analysis, four indicators—summary judgments of procedural justice, inferences of motive-based trust, judgments about the fairness of decision making, and judgments about the fairness of interpersonal treatment—are treated as indices of an overall assessment of procedural justice in the exercise of authority.²⁵

Procedural justice theory's key innovation, therefore, is to provide a psychological explanation of what drives people's beliefs about legitimacy, as well as a prediction about how these beliefs shape legal compliance and cooperation. The claim is that people's beliefs are shaped mostly by criminal justice procedures—by the way in which law enforcement officials behave—rather than by criminal justice outcomes.²⁶ If police procedures are viewed by the public as just, community members will be more likely to see the police as legitimate. Procedures viewed as unjust will have de-legitimizing effects.

²³ It is helpful to notice that these competing psychological accounts of legal compliance and cooperation—one grounded in rational choice, the other in legitimacy and obligation—parallel the dominant competing accounts of the very nature of law within legal philosophy. One account of the nature of law, developed most prominently by Jeremy Bentham and John Austin, argues that law is nothing more than commands backed by sanctions. See JEREMY BENTHAM, *AN INTRODUCTION TO THE PRINCIPLES AND MORALS OF LEGISLATION* (1823). The competing account, developed by H.L.A. Hart, argues that the nature of law lies in the internalization of an obligation to obey legal rules. See H.L.A. HART, *THE CONCEPT OF LAW* (1961). For a recent effort to connect these analytic debates about the “nature” of law to psychological debates about why people comply with the law, see FRED SCHAUER, *THE FORCE OF LAW* (2015).

²⁴ See TOM R. TYLER, *WHY PEOPLE COOPERATE: THE ROLE OF SOCIAL MOTIVATIONS* (2013); TOM R. TYLER, *WHY PEOPLE OBEY THE LAW* (2006).

²⁵ Tom Tyler (2006).

²⁶ See TYLER, *supra* note [], at [].

This raises, of course, the question of which sorts of procedures are likely to be viewed by the public as procedurally just. The enormous procedural justice literature that has developed over the last two decades provides literally hundreds of possibilities. From a theoretical perspective, however, proponents of the theory typically argue that a person's procedural justice assessments are driven primarily by whether the person feels that the legal authority (1) uses fair procedures, (2) treats her fairly, and (3) acts in a way that shows that the authority is concerned about her and will behave in ways that serve her interests.²⁷ So fair procedures, fair treatment, and the trustworthiness of police motives are key determinants of the public's procedural justice judgments.²⁸

While procedural justice theory was first deployed to explain compliance with the law, Tyler and others have argued that it is even more important for explaining cooperation: "cooperation is a more fragile commodity than compliance, because it is easy for people not to cooperate."²⁹ For this reason, they argue that the theory can explain myriad types of community cooperation with the police—and do so better than alternative theories grounded in incentives and self-interest.³⁰ Under the theory, attitudes translate into actions in the following fashion:

²⁷ Procedural justice theorists take the position that that these attitudes can, but need not be, based on a person's personal interactions with the police. While one strand of procedural justice scholarship considers on people's personal interactions with the police, another strand considers the general public's overall evaluation of police rather than personal experiences with legal authorities. See Tom R. Tyler, *Procedural Justice, Legitimacy, and the Effective Rule of Law*, 30 CRIME & JUSTICE 283, 317-18 (2003); Tom R. Tyler, *Public Trust and Confidence in Legal Authorities: What Do Majority and Minority Group Members Want from Legal Authorities?*, 19 BEHAVIORAL SCI. & L. 215 (2001). Procedural justice research on immigrants in particular often focuses on beliefs that are not connected directly to personal interactions with the police. See David S. Kirk, Andrew Papachristos, Jeffrey Fagan, & Tom Tyler, *The Paradox of Law Enforcement in Immigrant Communities: Does Tough Immigration Enforcement Undermine Public Safety?*, 641 ANNALS, AAPSS 79 (2012)

²⁸ See David DeCremer & Tom R. Tyler, *The Effects of Trust in Authority and Procedural Fairness in Cooperation*, 92 J. APPLIED PSYCHOLOGY 639, 641 (2007). Trust can be decomposed into two types: (1) institutional, which refers to the perception that police are honest institutional players who care for members of the communities they police; and (2) motivation-based, which is the belief that police officers have good intentions when they interact with the public and will make a good faith effort to respond to their needs. See Tom R. Tyler, *Policing in Black and White: Ethnic Group Differences in Trust and Confidence in the Police*, 8 POLICE QUARTERLY 322, 325-26 (2005).

²⁹ Stephen J. Schulhofer, Tom R. Tyler, & Aziz Z. Huq, *American Policing as a Crossroads: Unsustainable Policies and the Procedural Justice Alternative*, 101 J. CRIM. L. & CRIMINOLOGY 335, 348 (2011).

³⁰ See, e.g., Tom R. Tyler & Jeffrey Fagan, *Legitimacy and Cooperation: Why Do People Help the Police Fight Crime in their Communities*, 6 OHIO ST. J. CRIM. L. 231, 238-40, 252-55 (2008); Schulhofer et al, *supra* note []. For example, Jason Sunshine and Tom Tyler measure cooperation with the police by creating an index from responses on a 6-point scale to the following ten questions: would you call police to report a crime occurring in your neighborhood; to report an accident; to help police find someone suspected of committing a crime; give info to police to help solve a crime; report dangerous or suspicious activities; voluntarily work as a liaison officer at night/weekends; show police around your neighborhood; volunteer to attend community meeting to discuss crime in neighborhood; work with neighborhood watch; serve on neighborhood committee. Jason Sunshine & Tom R. Tyler, *The*



Procedural justice theory contrasts sharply with other theories of legal compliance and cooperation. Deterrence theory, which Gary Becker most prominently articulated, predicts that compliance with the law is determined by the anticipated gain from offending relative to the size of the expected punishment for violations, which is a product of the probability of apprehension and magnitude of the sanction.³¹ Procedural justice theory discounts the role sanctions play in securing legal compliance. It also downplays the importance of visible police efficacy. Others—most prominently George Kelling and James Q. Wilson in their “broken windows” theory of crime—have contended that demonstrations of police effectiveness in controlling crime are crucial to securing compliance, because small disorders left unchecked escalate into crimes that are greater in number and more serious in nature.³² But these ideas are rejected by procedural justice theory. Finally, the theory deemphasizes the relevance of the distributive fairness of law enforcement. Whether police services are fairly distributed across communities has little effect, from a procedural justice perspective, on levels of legal compliance and community cooperation.³³

The theory’s distinctive conception of human motivations also leads to different recommendations for policing strategy. Tyler advocates “process-based regulation” in which “police can best regulate public behavior by focusing on engaging the social values, such as legitimacy, that lead to self-regulation on the part of most of the public, most of the time.”³⁴ Practices and policies characterized by procedural legitimacy will build police legitimacy, and when police must exercise their discretion,

Role of Procedural Justice and Legitimacy in Shaping Public Support for Policing, 37 LAW & SOC’Y REV. 513, 541-42 (2003).

³¹ See Gary S. Becker, *Crime and Punishment: An Economic Approach*, 76 J. POL. ECON. 169, 174-76 (1968). See Sunshine & Tyler, *supra* note [], at 514 (describing credible threats for rule-breakers as a “risk” approach). In this 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 than Holmesian.

³² George L. Kelling & James Q. Wilson, *Broken Windows: The Police and Neighborhood Safety*, 249 ATLANTIC MONTHLY 29 (1982). See Sunshine & Tyler, *supra* note [], at 514 (describing a police “performance” approach).

³³ See Sunshine & Tyler, *supra* note [], at 514.

³⁴ See Sunshine & Tyler, *supra* note [], at 515, 535; see also TOM R. TYLER & YUEN J. HOU, TRUST IN THE LAW: ENCOURAGING PUBLIC COOPERATION WITH POLICE AND COURTS 198-208 (2002).

the public will be more likely to support the decision.³⁵ Ultimately, police policies that the public perceives as procedurally legitimate will be those which are most effective in fighting crime.

B. Existing Empirical Evidence

Procedural justice theory purports to explain why people cooperate with the police. How can the theory be tested empirically? As it turns out, demonstrating that legitimacy-altering policies actually affect the extent of community cooperation with the police has proven to be an incredibly difficult task.

Existing empirical efforts suffer from two important limitations. First, the procedural justice literature does not seek to measure actual cooperation with the police. Instead, it has focused almost exclusively on an input in or an intermediate step in the causal chain of the procedural justice model: the effect of perceived legitimacy on *attitudes* about cooperation. Early studies relied on classroom simulations with college students.³⁶ Subsequent work examined actual litigants, members of the public, and members of heavily-policed groups.³⁷ Most procedural justice scholarship involves 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 actual cooperative behavior is presumed.³⁸ Thus, even if

³⁵ See Sunshine & Tyler, *supra* note [], at 535.

³⁶ See, e.g., Laurens Walker, Stephen LaTour, E. Allan Lind, & John Thibaut, *Reactions of Participants and Observers to Modes of Adjudication*, 4 J. APPLIED SOC. PSYCH. 295, 298 (1974); Ruth Kanfer, John Sawyer, Christopher P. Earley, E. Allan Lind, *Participation in Task Evaluation Procedures: The Effects of Influential Opinion Expression and Knowledge of Evaluative Criteria on Attitudes and Performance*, 1 SOC. JUSTICE RESEARCH 235 (1987); E. Allan Lind, Ruth Kanfer, Christopher P. Earley, *Voice, Control, and Procedural Justice: Instrumental and Noninstrumental Concerns in Fairness Judgments*, 59 J. PERSONALITY & SOC. PSYCH. 952, 954 (1990).

³⁷ See, e.g., Jonathan D. Casper, Tom Tyler, & Bonnie Fisher, *Procedural Justice in Felony Case*, 22 LAW & SOC. REV. 483, 487-89 (1988) (felony defendants); TYLER, *supra* note [], at 327 (telephone survey of New York City residents); Tyler & Fagan (a two-wave telephone survey of New York City residents); Sunshine & Tyler, *supra* note [] (a two-wave telephone survey of New York City residents); Aziz Z. Huq, Tom R. Tyler, & Stephen J. Schulhofer, *Mechanisms for Eliciting Cooperation in Counterterrorism Policing: Evidence from the United Kingdom*, 8 J. EMPIRICAL LEG. STUD. 728, 736-38 (2011) (two surveys of Muslims in London); Aziz Z. Huq, Tom R. Tyler, & Stephen J. Schulhofer, *Why Does the Public Cooperate with Law Enforcement? The Influence of the Purposes and Targets of Policing*, 17 PSYCHOLOGY, PUBLIC POLICY, & LAW 419, 426 (2011) (multiple waves of Muslims and general residents of New York City); Andrew V. Papachristos, Tracey L. Meares, & Jeffrey Fagan, *Why Do Criminals Obey the Law? The Influence of Legitimacy and Social Networks on Active Gun Offenders*, 102 J. CRIM. LAW & CRIMINOLOGY 387, 412-16 (2012) (adults on probation or parole living in high crime Chicago neighborhoods).

³⁸ 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.” Sunshine & Tyler, *supra* note [], at 535 (quoting Sampson, Raudenbush, & Earls 1997).

surveys accurately measure attitudes, those attitudes may not necessarily reflect actual behavior.

Second, studies of procedural justice typically do not employ the empirical designs commonly found in the policy evaluation literature, designs that are intended to estimate the causal impacts of specific policy interventions. Many procedural justice studies conduct surveys of residents within a single jurisdiction. These studies therefore do not capture policy changes that might support credible inferences about the impact of specific policies. Even when these studies encompass multiple jurisdictions, numerous other unobserved differences across jurisdictions impair the ability of these cross-sectional comparisons to support causal claims.³⁹ A small number of procedural justice researchers have conducted experiments rather than surveys. In a recent meta-analysis of nearly 1,000 empirical studies investigating police interactions with individuals that implicated procedural justice, the authors found that fewer than 30 studies employed the strategy of randomizing subjects into treatment and control groups.⁴⁰ A standard limitation of experiments is that the treatment that the researcher administers may reflect only part of the wider public policy of interest. Although inspired by actual policies and practices, experiments can employ only laboratory proxies for them. Another conventional uncertainty surrounding experiments is whether any findings generalize beyond the experimental setting. In view of these limitations, additional methods should be deployed to assess procedural justice theory.

II. A COOPERATION EXPERIMENT IN IMMIGRATION FEDERALISM

The ideal experiment testing legitimacy's importance for community cooperation would be a randomized policy intervention that altered public attitudes about the police. The intervention would cause community members in some locations, but not others, to see the police as less legitimate than they did prior to the experiment. If residents in the communities that received the policy "treatment" started to cooperate less with the police, but residents of the "control" communities did not, we would have strong evidence that the de-legitimizing policy affected levels of community cooperation in the real world.

To approach this ideal experiment and solve the problems that have plagued existing attempts to measure legitimacy's effects on cooperation, therefore, one would like to have: (1) a policy that undermines the trustworthiness and legitimacy of the police in the eyes of immigrants, (2) a mechanism to randomly apply that policy

³⁹ Two studies that used longitudinal data are: Tyler & Fagan, *supra* note [], at 244-45; and Tom R. Tyler, Lawrence Sherman, Heather Strang, Geoffrey C. Barnes, & Daniel Woods, *Reintegrative Shaming, Procedural Justice, and Recidivism: The Engagement of Offenders' Psychological Mechanisms in the Canberra RISE Drinking-and-Driving Experiment*, 41 L. & SOC. REV. 553, 560-63 (2007)

⁴⁰ Lorraine Mazerolle, et al., *Procedural Justice and Police Legitimacy: A Systemic Review of the Research Evidence*, 9 J. EXP. CRIMINOLOGY 245, 255-56 (2013).

to some communities but not others, and (3) a measure of actual cooperative behavior.

Our core innovation is to identify a real-world policy—Secure Communities—that provides the first two items on this checklist. First, the information-sharing program was widely seen as undermining the trustworthiness and legitimacy of local police in the eyes of immigrants—particularly immigrants living in the United States unlawfully. Importantly, it did so without simultaneously changing local police tactics or resources in a way that might have altered levels of community cooperation irrespective of any effect caused by the loss of trust and legitimacy.

Second, the way in which Secure Communities was activated around the country on a county-by-county basis approximates a natural experiment. This makes it possible to make comparisons of counties with and without the controversial program, as well as to make comparisons before and after the program was activated in a county. Moreover, because community cooperation was not an explicit objective of the program, making it likely that that timing of the program’s rollout was likely unrelated (exogenous, economists would say) to the degree of community cooperation within each county. These features allow us to identify the program’s causal effect on cooperation.

This leaves the challenge of measuring cooperative behavior rather than just beliefs. Our second innovation solves this problem by focusing directly on the *outputs* of the procedural justice model, rather than its *inputs*. We capitalize on the fact that the ultimate output—the ability of police to solve and reduce crime as a result of community cooperation—is in principle observable. In fact, a large literature in criminology examines the frequency with which police “clear” crimes—that is, solve them by arresting offenders or by other means.⁴¹ Clearance rates thus permit us to measure how likely the police are to solve any particular category of crime. Changes in clearance rates thus furnish a proxy for changes in cooperative behavior that result from Secure Communities.

A. Trust, Legitimacy and Secure Communities

1. The Program

The American immigration enforcement system has long been characterized by high levels of both unauthorized migration and deportation.⁴² In this system, locating unauthorized immigrants is crucial to the rational functioning of the immigrant screening system.⁴³ But information on the identity and whereabouts of unauthorized immigrants has proven hard for the federal government to come by.

⁴¹ See Marc Riedel, *Homicide Arrest Clearances: A Review of the Literature*, 2 SOCIOLOGY COMPASS 1145, 1153-55 (2008).

⁴² See generally Adam B. Cox & Eric A. Posner, *The Second-Order Structure of Immigration Law*, 59 STAN. L. REV. 809 (2007) (conceptualizing the immigration system as a screening system, and emphasizing the information asymmetry at the heart of the system).

⁴³ See *id.*; Cox & Miles, *supra* note [], at 131-35.

Despite the importance of information in immigration enforcement, there has historically been relatively little information-sharing between local and federal officials about immigrant status. This is true even with respect to a long-standing federal enforcement priority: the desire to deport immigrants who engage in crime. For decades, when local law enforcement agencies arrested immigrants or convicted them of crimes, federal immigration officials were only sporadically informed. Often the local officials had no way of knowing whether an arrestee was a potentially deportable noncitizen. And even if they happened to know the noncitizen's status (or the fact that a conviction might render the noncitizen deportable), the likelihood that they would alert federal officials turned entirely on informal cooperative relationships among local and federal law enforcement agencies.⁴⁴

Beginning in the late 1980s, the federal government launched several initiatives to change this pattern by arranging for individual inmates to be interviewed in local jails and prisons. These interviews were conducted by federal officials pursuant to the Criminal Alien Program (CAP),⁴⁵ and by deputized local law enforcement officials under so called "287(g)" agreements—agreements named for the provision of the Immigration and Nationality Act that authorized them.⁴⁶ 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.⁴⁷

This all changed in 2008. Dissatisfied with the modest progress of the CAP and 287(g) programs, the Department of Homeland Security launched "Secure Communities."⁴⁸ The program was no less than an information revolution. It established 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.⁴⁹ Traditionally, when a person is arrested and booked by a state or local law enforcement agency, his fingerprints are taken and

⁴⁴ See ALEINIKOFF ET AL., *supra* note [], at 944.

⁴⁵ The first incarnation of CAP was the Alien Criminal Apprehension Program, launched in 1986 as a pilot project in four cities. See Mark R. Rosenblum & William A. Kandel, Interior Immigration Enforcement: Programs Targeting Criminal Aliens, Congressional Research Service (December 20, 2012); Andrea Guttin, The Criminal Alien Program, American Immigration Council (2010).

⁴⁶ 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.

⁴⁷ See Cox & Miles, *supra* note [], at 92-93.

⁴⁸ See Immigration and Customs Enforcement, 1st Quarterly Status Report (April–June 2008) for Secure Communities: A Comprehensive Plan to Identify and Remove Criminal Aliens 7 (DHS Aug 2008).

⁴⁹ The account of the program draws on our description in Thomas J. Miles & Adam B. Cox, *Does Immigration Enforcement Reduce Crime? Evidence From Secure Communities*, 57 J. L. & ECON. 1 (2014).

forwarded electronically to the Federal Bureau of Investigation (FBI), which conducts a criminal background check and 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.⁵⁰

If the fingerprints matched a set in the DHS database, DHS personnel evaluated the person's immigration status and decide whether to place a "detainer" (sometimes referred to as an "immigration hold") on the person. The detainer requested that the local law enforcement agency hold the person for forty-eight hours beyond the scheduled release, in order to permit Immigration and Customs Enforcement (ICE) to transfer the person to federal custody for the initiation of deportation proceedings.⁵¹ The detainer thus allowed the federal government to readily apprehend and place in deportation proceedings a noncitizen whom the local criminal justice system would otherwise release. This included 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 increased 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 made it the largest expansion of local involvement in immigration enforcement in the nation's history.

2. *De-Legitimizing Local Police*

Procedural justice theory predicts that involving local police in the enforcement of federal immigration law will undermine the legitimacy of local law enforcement institutions. This prediction, widely subscribed to by procedural justice scholars (including Tom Tyler himself) is that the

assignment of immigration enforcement to local police . . . may have complications that its proponents have not anticipated. One potentially significant byproduct of such enforcement is the changing of attitudes toward

⁵⁰ *See id.*

⁵¹ *See* Christopher Lash, *Federal Immigration Detainers After Arizona v. United States*, 46 LOYOLA L.A. L. REV. 629 (2013).

the law, legal authority, and especially the police in immigrant communities. . . . Thus a *paradox* arises: harsh legal sanctions against immigrants are often framed as a means to keep communities “safe,” yet they may in fact have the opposite effect by decreasing cooperation with police.⁵²

On this account, the integration of local police activates several of the psychological mechanisms described by procedural justice theory. Immigrants are more likely (1) to see police procedures as unfair when those procedures incorporate elements of immigration enforcement, rather than focusing exclusively on public safety; (2) to believe that they will be treated unfairly by the police in comparison to those who do not appear to be immigrants,⁵³ and (3) to see local police involvement as evidence that police are less concerned about immigrants and will not always behave in ways that serve their interests.⁵⁴ In other words, the theory predicts that local police involvement in immigration enforcement will affect immigrants’ procedural justice judgments about fair procedures, about fair treatment, and about the trustworthiness of the police.⁵⁵

Secure Communities was the largest integration of local police into federal immigration enforcement in the nation’s history. The scale of the policy intervention was vast. According to operational data we obtained about the program through extensive FOIA requests, Secure Communities led to the federal detention of over a quarter of a million immigrants during its first four years of operation. As a percentage of noncitizens in the United States, this figure is larger than the percentage of the entire U.S. population that is currently incarcerated in the criminal

⁵² David S. Kirk, Andrew Papachristos, Jeffrey Fagan, & Tom Tyler, *The Paradox of Law Enforcement in Immigrant Communities: Does Tough Immigration Enforcement Undermine Public Safety?*, 641 ANNALS, AAPSS 79 (2012) (emphasis in original).

⁵³ As procedural justice scholars have emphasized, this perception may take two forms. First, immigrants may believe that the police treat citizens better than immigrants. Second, local immigration enforcement may also lead immigrants to believe that police make race-based decisions about them, also undermining immigrants’ perceptions of procedural justice: “[W]e have shown . . . mounting evidence that this policy [of immigration enforcement by local police] would create the perception (if not the reality) of racial (anti-Latino) bias. This perceived bias in cross-deputization policy would taint the legitimacy of the police in the eyes of the community, a grave development given that illegitimacy has been shown consistently to undermine the relationship between the community and the police, rendering the police less effective.” Liana Maris Epstein & Phillip Atiba Goff, *Safety or Liberty? The Bogus Trade-off of Cross-Deputization Policy*, 11 ANALYSES OF SOCIAL ISSUES & PUB. POL’Y, 314 (2011). See generally Trevor Gardner II & Aarti Kohli, *The C.A.P. Effect: Racial Profiling in the ICE Criminal Alien Program*, Warren Institute (2009).

⁵⁴ See, e.g., Kirk et al., *supra* note []; Ramiro Martinez, Jr. & Janice Iwama, *The Reality of the Secure Communities Program: Are Our Communities Really Becoming More Secure?*, 13 CRIMINOLOGY & PUBLIC POLICY 339 (2014); Charles E. Kubrin, *Secure Or Insecure Communities?*, 13 CRIMINOLOGY & PUBLIC POLICY 1 (2014); 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); Jason G. Idilbi, *Local Enforcement of Federal Immigration Law: Should North Carolina Communities Implement 287(g) Authority*, 86 N.C. L. Rev. 1710 (2008).

⁵⁵ See *id.*

justice system.⁵⁶ Given the size of the intervention and the theoretical prediction of procedural justice theory, it should come as little surprise that Secure Communities was widely seen as a serious threat to immigrant trust in the local criminal justice system—perhaps the most serious threat in decades.⁵⁷

This concern was far from limited to procedural justice scholars. Indeed, the claim that Secure Communities would undermine immigrant cooperation with local police became *the dominant* critique leveled against the program by advocates and public officials around the country. Leading immigrants' rights groups organizing against Secure Communities in communities where it was being activated.⁵⁸ The National Day Laborers Organizing Network, the American Civil Liberties Union, and others argued that the the program would undermine relations between local police and immigrant communities. Immigrants would no longer trust that the police were working to serve their interests, and this distrust would make immigrants reluctant to interact with the police.⁵⁹

These organizations backed up their claims with survey evidence suggesting that these de-legitimizing effects would affect immigrant attitudes about cooperation. One widely cited study—involving a randomized survey of over 2,000 Latinos living in Chicago, Houston, Los Angeles, and Phoenix—documented the way that the increasing integration of local police into federal immigration enforcement was shaping perceptions of the police.⁶⁰ According to the survey, nearly half of foreign-

⁵⁶ See Miles & Cox, *supra* note [], at []. In a way this impact is unsurprising. Some 40 million immigrants live in the United States today—more than ten percent of the nation's population—and roughly 25 million of those immigrants are still noncitizens (the remainder have naturalized). Moreover, of the 25 million noncitizens, *nearly half* are currently living in the country in violation of immigration law. And because of migration patterns and residential segregation, the numbers are often much more stark 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.

⁵⁷ See, e.g., Martinez & Iwama, *supra* note [], at [] (deploying procedural justice theory to argue that Secure Communities threatens to “produce mistrust of local police, create conflict with the goals of community policing, and provoke suspicion on the part of newcomers”); Kubrin, *supra* note [] (same).

⁵⁸ See, e.g., Kate Linthicum, *Obama Ends Secure Communities Program as Part of Immigration Action*, L.A. TIMES, Nov. 21, 2014 (surveying the history of critiques by immigration advocates).

⁵⁹ See Rachel Zoghlin, *Insecure Communities: How Increased Localization of Immigration Enforcement Under President Obama through the Secure Communities Program Makes Us Less Safe, and May Violate the Constitution*, 6 MOD. AM. 20 (2010) (collecting opposition from immigrant advocacy groups and law enforcement officials); Alex Stepick, *False Promises: The Failure of Secure Communities in Miami-Dade County*, Americans for Immigrant Justice (Apr. 2013) (same), <http://d3n8a8pro7vhmx.cloudfront.net/aijustice/pages/283/attachments/original/1390429692/False-Promises-The-Failure-of-Secure-Communities-in-Miami-Dade-County.pdf?1390429692>.

⁶⁰ See Nik Theodore, *Insecure Communities: Latino Perceptions of Police Involvement in Immigration Enforcement* (May 2013), http://www.policylink.org/sites/default/files/INSECURE_COMMUNITIES_REPORT_FINAL.PDF. For similar survey evidence, see Phillip Atiba Goff et al., *Crossing the Line of Legitimacy: The Impact of Cross-Deputization Policy on Crime Reporting*, 19 PSYCHOL. PUB. POL'Y & L. 250 (2013).

born respondents reported that increased local involvement in immigration enforcement made them less likely to report crimes or provide information to the police—even in situations where they were themselves victims.⁶¹ The results were even more stark among respondents who identified themselves as undocumented: in that group, more than two-thirds said that they would be less likely to go to the police.⁶²

The concern that Secure Communities undermines cooperation has also been raised by law enforcement officials around the country. The sentiment was well-captured by Michael Hennessey, the sheriff of San Francisco:

Maintaining public safety requires earning community trust. We rely heavily on the trust and cooperation of all community members—including immigrants—to come forward and report crimes, either as victims or as witnesses. Otherwise, crimes go unreported—and this affects everyone, citizens and noncitizens alike. . . . Immigration and Customs Enforcement’s controversial Secure Communities program violates this hard-earned trust with immigrant residents.⁶³

Many other prominent public officials, including New York’s famous former district attorney Robert Morgenthau,⁶⁴ as well as former Los Angeles and current New York City police chief William Bratton,⁶⁵ have echoed this view.⁶⁶ It is shared by many

⁶¹ See Theodore, *supra* note [], at 5-6.

⁶² See *id.*

⁶³ Michael Hennessey, *Secure Communities Destroys Public Trust*, SFGATE, May 1, 2011, <http://www.sfgate.com/opinion/article/Secure-Communities-destroys-public-trust-2373213.php>.

⁶⁴ See Robert Morgenthau, *Opinion: The Police and Immigration: New York’s Experience*, WALL ST. J., May 18, 2010.

⁶⁵ See Chief William J. Bratton, Comments on Immigration, Los Angeles Police Department Newsroom (last visited Feb. 22, 2015), http://www.lapdonline.org/newsroom/content_basic_view/43388.

⁶⁶ See, e.g., Joseph A. Curtatone, *Ending Secure Communities Strengthens Police, Public Cooperation*, SOMERVILLE TIMES, Dec. 11, 2014, <http://www.thesomervilletimes.com/archives/54592> (quoting the mayor of Somerville, Massachusetts: “Truly keeping a community safe requires the police and residents to work together. Secure Communities undermined the trust needed for that kind of collaboration to happen between law enforcement and residents . . . It discouraged innocent immigrant victims and witnesses of crimes from reporting or cooperating with law enforcement due to fear of deportation. Police officers who rely on public cooperation to solve crimes and maintain public safety found their jobs harder, not easier.”); *Immigrants Say Chicago Sanctuary Policy Being Violated*, FOX NEWS LATINO, Aug. 9, 2011 (quoting Cook County (Chicago) Sheriff Tom Dart’s concern that cooperation was “erod[ing] the immigrant communities’ confidence in the role of law enforcement”), <http://latino.foxnews.com/latino/news/2011/08/09/immigrants-say-chicago-sanctuary-policy-being-violated/>. See generally Chris Burbank, Phillip Atiba Goff & Dr. Tracie L. Keese, *Policing Immigration: A Job We Do Not Want*, HUFFINGTON POST, June 7, 2010, http://www.huffingtonpost.com/chief-chris-burbank/policing-immigration-a-jo_b_602439.html (expressing belief of Salt Lake City Chief of Police Chris Burbank that “[s]heriffs and chiefs have long voiced their concerns that asking officers to be immigration agents will scare undocumented community members out of calling on law enforcement for help.”). See generally Scott H. Decker et al., *On the Frontier of Local Law Enforcement: Local Police and Federal Immigration Law*, in IMMIGRATION, CRIME, AND JUSTICE (2009) (surveying law enforcement officials); A. Elena Lacayo, *The Impact Of*

organizations that study policing as well. They have expressed the same concern that “[l]ocal police involvement in immigration enforcement could have a chilling effect on immigrant cooperation Without this cooperation, law enforcement will have difficulty apprehending and successfully prosecuting criminals, thereby reducing overall public safety for the larger community.”⁶⁷

Recently, fears like these fueled the passage of local policies designed to limit state and local cooperation with Secure Communities. Local law enforcement agencies are powerless to resist the rollout of Secure Communities in their jurisdictions. But they can refuse to help Immigration and Customs Enforcement (ICE) take custody of immigrants identified through the program. And this is exactly what they did. In the past two years, nearly 300 localities have adopted “anti-detainer” policies that restrict the extent to which local officials may continue to detain individuals in order to hand them over to ICE.⁶⁸ Because these policies nearly all came into existence after the end of our study period, they do not undermine the extent to which the program can be understood an exogenous shock to local police policy, which is useful for our research design.⁶⁹ Nonetheless, the policies highlight the intensity of concerns that Secure Communities will undermine the trustworthiness and legitimacy of local police. In fact, in California, the legislation ultimately adopted was aptly named the “Trust Act”—a term that then quickly became the label applied to pretty much all efforts to resist Secure Communities at the state or local level.⁷⁰

The criticism and growing resistance to the program ultimately made the moniker “Secure Communities” politically toxic. On November 20, 2014, the

Section 287(G) Of The Immigration And Nationality Act On The Latino Community, National Council Of La Raza (2010), 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 [cooperative enforcement] agreements often severely hinder the ability of police to earn trust required to implement effective community policing strategies to fight criminal activity.”).

⁶⁷ Anita Khashu, *The Role Of Local Police: Striking A Balance Between Immigration Enforcement*, Police Foundation, (2009); *Major Cities Chiefs Immigration Committee Recommendations for Enforcement of Immigration Laws by Local Police Agencies* (2006), http://www.houstontx.gov/police/pdfs/mcc_position.pdf. See generally Pradine Saint-Fort et al., *Engaging Police in Immigrant Communities: Promising Practices from the Field*, Vera Institute for Justice (2012).

⁶⁸ See Catholic Immigration Legal Network, *States and Localities That Limit Compliance with ICE Detainer Requests* (July 2015), <https://cliniclegal.org/resources/articles-clinic/states-and-localities-limit-compliance-ice-detainer-requests-jan-2014>.

⁶⁹ See Miles & Cox, *supra* note [], at [] (documenting the policies and their timing).

⁷⁰ The Acronym TRUST stands for Transparency and Responsibility Using State Tools.” CAL. GOV’T CODE §§ 7282-7282.5 (West, Supp. 2014); see also *Recent Legislation—Trust Act*, 127 Harv. L. Rev. 2593 (2014). Explaining the purpose of the Trust Act, Representative Zoe Lofgren said that “[w]hat this will do for law enforcement in California is that it will ensure that immigrants collaborate with law enforcement.” Elise Foley and Roque Planas, *Trust Act Signed in California to Limit Deportation Program*, HUFFINGTON POST (Oct. 5, 2014), http://www.huffingtonpost.com/2013/10/05/trust-act-signed_n_4050168.html.

Secretary of Homeland Security, Jeh Johnson, announced that “the Secure Communities program, as we know it, will be discontinued.”⁷¹ Even his announcement sounded in procedural justice, conceding that any program of cooperative immigration enforcement “must be implemented in a way that supports community policing and sustains the trust of all elements of the community in working with local law enforcement.”⁷²

Embedded in these public critiques of Secure Communities are two related but distinct claims about the effect of the program on immigrant communities. The first is an instrumental claim about perceived risk. The idea is that turning every local arrest into a point of immigration screening will shape the behavior of an immigrant whenever (a) a police encounter increases the risk of arrest, and (b) the screening accompanying arrest increases the risk of federal apprehension or deportation for either the immigrant or someone she cares about. On this account, immigrants have no reason to avoid police encounters that have no chance of resulting in an arrest for themselves or someone they care about. Relatedly, even when arrest is a possibility, the effects of Secure Communities on immigrant trust will be greater for unauthorized migrants than for those immigrants who have legal status.⁷³ For a person without legal status, any arrest—even one for minor conduct that leads to no prosecution or conviction—could result in deportation, because the person is deportable by virtue of her status.⁷⁴ For a green card holder, however, an arrest poses a threat only under circumstances where it might result in a conviction for an offense that would make the noncitizen deportable under the immigration code—a code that makes certain convictions, but not others, grounds for deportation.⁷⁵

⁷¹ See Memorandum from Jeh Johnson, Department of Homeland Security, Nov. 20, 2014, http://www.dhs.gov/sites/default/files/publications/14_1120_memo_secure_communities.pdf, at 1.

⁷² *Id.* at 1-2. While Secure Communities has formally been discontinued, it has been replaced with a similar program known as the “Priority Enforcement Program,” or PEP. PEP continues, without change, the core technological component of Secure Communities: the mandatory, biometric immigration screening of every person arrested by local law enforcement officials. *See id.* But the program differs from Secure Communities in two respects. First, PEP narrows the grounds on which ICE will seek to detain a noncitizen identified through the biometric screening system. While nearly one-third of the immigrants detained under Secure Communities between 2008 and 2012 had no criminal conviction at all, PEP’s authorizing memorandum states that, subject to limited exceptions, “enforcement actions through the new program will only be taken against aliens who are *convicted* of specifically enumerated crimes.” *Id.* Second, PEP narrows considerably the use of immigration detainers. Instead of issuing detainers, ICE is now required in most cases to issue a simple request for notification—that is, a request that the local government inform the federal officials of the time when they plan to release the noncitizen. The idea is that, with this information, federal immigration agents can show up at the appointed time and effectuate a transfer of custody, without requiring local officials to hold the immigrant beyond the time when they otherwise would be released from criminal custody. *See id.*

⁷³ See Theodore, *supra* note [], at [] (documenting, in survey results, greater concern about police contact among unauthorized migrants than other foreign born persons).

⁷⁴ See INA § 212(a)(6), 8 U.S.C. § 1182(a)(6); INA § 237(a)(1), 8 U.S.C. § 1227(a)(1).

⁷⁵ See INA § 237(a)(2), 8 U.S.C. § 1227(a)(2) (laying out criminal grounds of deportability).

While this instrumental claim has played some role in public debate, the critique of Secure Communities has far more commonly been grounded in perceptions of legitimacy. The concern is that once immigrants see local police as immigration enforcers, they will come to doubt the motives of the police—no longer believing that local police are really concerned about their well being and committed to serving their interests. In the language of procedural justice, inferences of motive-based trust and judgments about the fairness of interpersonal treatment lead immigrants to view local police as less legitimate.⁷⁶ This loss of legitimacy 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.⁷⁷ Risk assessments are not central to this account, because cooperation is based more on beliefs about whether it is just for local police to assist in federal immigration enforcement, about whether such involvement will lead immigrants to be treated unfairly by the police, and ultimately about whether involvement undermines the legitimacy of the police.⁷⁸

Interestingly, the instrumental and legitimacy-based mechanisms lead to different predictions about *who* will cooperate less with police and about *how much less* they will cooperate. Thus, not only does Secure Communities provide a potential means of testing theories of legitimacy's importance for securing community cooperation, in theory it makes it possible to distinguish legitimacy-based effects from more instrumental effects.

3. Resource Neutrality

A second feature of Secure Community that makes it an ideal policy to study is that it does not directly affect the resources or tactics of the local police. As we explained above, the program piggybacks on existing arrest and booking practices. It was carefully designed by the federal government to require no additional effort on the part of local police: they would continue to make the same arrests they had always made, and the federal government would simply use those arrests as point of immigration screening, as a way to pluck noncitizens out of the back end of the local criminal justice system.

This unique structure helps isolate the legitimacy-based effects of the policy. Most other law enforcement policies that have been studied by procedural justice scholars do much more than simply shock perceptions of legitimacy. The policies typically change police tactics in significant ways, often at great cost. Community policing strategies that emphasize putting more officers out on foot patrol, for example, reflect a significant tactical change that diverts resources towards cops walking the beat and away from other crime fighting strategies. These changes in

⁷⁶ See Tyler, *supra* note [], at [].

⁷⁷ See, e.g., Khashu, *supra* note [] (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”).

⁷⁸ See *supra* text accompanying notes []-[].

tactics and resources might themselves change the level of community cooperation, making it hard to tell whether any observed change is the result of perceptions of legitimacy or instead something more mundane—like the increased opportunities for conversations with police officers who find themselves out on foot more frequently. The fact that Secure Communities does not entail these confounding effects is valuable, and it distinguishes the program not just from other law enforcement programs generally but from previous cooperative immigration federalism initiatives in particular. The 287(g) program, for example, often involved local police spending time investigating immigration violations and making immigration arrests, which might have had negative consequences from crime fighting.⁷⁹

That said, it is important to note that there are two indirect ways in which—at least in theory—Secure Communities have affected local policing tactics or resources. First, it is possible that local police changed 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. In other work we are exploring this possibility.⁸⁰ The second possibility is that Secure Communities could chew up local law enforcement resources if local agencies had to spend large sums of money detaining immigrants on behalf of the federal government. As we noted above, the federal government regularly placed detainees on noncitizens identified through the program. Detainers requested that the local law enforcement agency hold the immigrant for up to forty-eight hours after any otherwise-scheduled release, in order to give ICE time to take custody of the immigrant.⁸¹ And detainers often resulted in more than forty-eight hours of additional detention, because many local governments adopted policies of refusing to grant pre-trial release to noncitizens against whom a detainer had been lodged.⁸² Nonetheless, while a number of local governments have complained about the costs of complying with detainer requests,⁸³ there is little evidence that these costs were anywhere near large enough to have had a meaningful effect on the functioning of local law enforcement agencies.⁸⁴

⁷⁹ See, e.g., Maria Fernanda Parra-Chico, *An Up-Close Perspective: The Enforcement of Federal Immigration Laws by State and Local Police*, 7 SEATTLE J. SOCIAL JUSTICE 321 (2008).

⁸⁰ See Adam B. Cox & Thomas J. Miles, *Do Local Police Profile Immigrants?* (work-in-progress).

⁸¹ See 8 C.F.R. § 287.7.

⁸² See Ingrid Eagley, *Criminal Justice for Noncitizens: An Analysis of Variation in Local Enforcement*, 88 N.Y.U. L. REV. 1126 (2013) (documenting local policies in Los Angeles and elsewhere).

⁸³ See, e.g., Letter from Toni Preckwinkle, Board of Commissioners of Cook County, to John Morton, Director of Immigration and Customs Enforcement (Jan. 19, 2012), <http://www.immigrantjustice.org/sites/immigrantjustice.org/files/Preckwinkle%20Response%20to%20Morton%20%2801%2019%2012%209.pdf>.

⁸⁴ Calculating the local cost of complying with detainers is extremely difficult for three reasons. First, it is hard because one must decide which costs to include: for example, should the costs of pretrial detention be included if that detention is the result of a local policy relating to criminal defendants with detainers, rather than the direct result of the detainer request? Second, it is often challenging to calculate the marginal (as opposed to average) cost of these additional days of

B. Nationwide Rollout as a Natural 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.⁸⁵ 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.⁸⁶ Figure 1 provides a visual representation of the pattern of rollout.

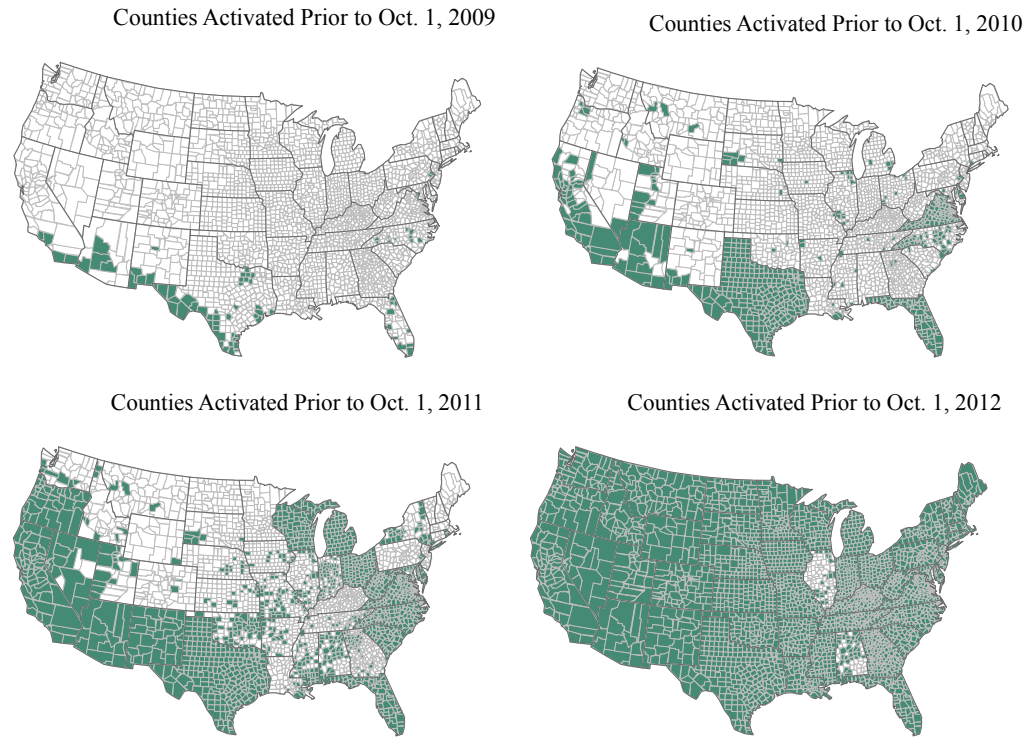
detention—and those marginal costs may depend on factors that fluctuate, like how close to capacity the jail is at any given time. Third, cost assessments are complicated by the fact that the federal government provides at least partial reimbursement for some of the costs local governments incur detaining unauthorized migrants. *See* State Criminal Alien Assistance Program, https://www.bja.gov/ProgramDetails.aspx?Program_ID=86.

Perhaps the best study available of detainer compliance costs comes from King County, Washington. Researchers there estimated that noncitizens subject to detainers stayed in jail, on average, 29.2 days longer than others—with the vast majority of this time attributable to a local policy of denying bail to those against whom detainers had been lodged. *See* Katherine Beckett & Heather Evans, *Immigration Detainer Requests in King County, Washington* (2013). They also estimated that refusing to comply with any detainer requests would have saved King County \$1.8 million in 2011, *see id.* at 21, which amounts to a bit more than 1% of the county's detention budget. *See* King County Budget FY 2011, *Adult and Juvenile Detention F-13*.

⁸⁵ *See* Cox & Miles, *supra* note [], at 95-102 (explaining the reasons for, and the pattern of, program rollout).

⁸⁶ *See* ICE, *Secure Communities Activated Jurisdictions* (last visited Feb. 26, 2015), <http://www.ice.gov/doclib/secure-communities/pdf/sc-activated.pdf>.

Figure 1. The Pattern of Secure Communities Rollout



This staggered sequence of rollout creates a quasi-natural experiment in the consequences of immigrant trust and perceived police legitimacy on law enforcement effectiveness. The program was applied uniformly to over 3,000 counties, generating many experiments rather than just one.⁸⁷ These experiments occurred over time, and the timing of each experiment was determined by the federal government, which dictated 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.⁸⁸ Moreover, the program's structure made informal

⁸⁷ Though the screening system operates identically in all jurisdictions, the intensity of the treatment does turn on local policing intensity. We discuss this possibility later in the paper and provide direct measures of the program's intensity.

⁸⁸ 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. *See* Office of Inspector General, Department of Homeland Security, Communication Regarding Participation in Secure Communities 4 (2012), http://www.oig.dhs.gov/assets/Mgmt/2012/OIG_12-66_Mar12.pdf

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—an extremely unlikely possibility.

In earlier work, we explored in detail the determinants of Secure Communities activation using proportional hazard analysis.⁸⁹ 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.⁹⁰ 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.⁹¹

C. Clearance Rates as a Measure of Cooperation

That leaves the question of how to measure changes in cooperation produced by Secure Communities' de-legitimizing effects. As noted above, procedural justice studies have focused almost exclusively on measuring beliefs—likely because it is so difficult to measure cooperation directly. But there is an alternative to measuring cooperation directly: measuring cooperation's consequences. Criminologists and other scholars have long argued that police are more likely to solve crimes when there are higher levels of community engagement and cooperation with law enforcement.⁹² The rate at which police “solve” crimes, therefore, provides a proxy for community cooperation.

The crime-solving rate, also known as a “clearance rate,” is widely recorded by law enforcement agencies. The FBI collects these data from every law enforcement agency in the country and publishes them annually in the FBI's Uniform Crime Reports (UCR). The UCR reports clearances for seven crimes that make up what are known as the FBI Index Crimes.⁹³ Four of the crimes are violent offenses: murder, rape, robbery, and aggravated assault. Three are property crimes: burglary, larceny, and motor vehicle theft.⁹⁴ A crime is “cleared” for purposes of the UCR when the

⁸⁹ See Adam B. Cox & Thomas J. Miles, *Policing Immigration*, 80 CHI. L. REV. 87 (2012).

⁹⁰ See *id.* at 118-122.

⁹¹ See *id.*

⁹² See, e.g., REISS, *supra* note []; Schulhofer et al., *supra* note [], at []; Robert J. Sampson, S. W. Raudenbush & F. Earls, *Neighborhoods and Violent Crime: A Multilevel Study of Collective Efficacy*, 277 SCIENCE 918 (1997).

⁹³ We obtained data on “clearance rates” from the FBI's Uniform Crime Reports (UCR). See Federal Bureau of Investigation, Uniform Crime Reports, <http://www.fbi.gov/about-us/cjis/ucr/ucr>.

⁹⁴ 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

offender is arrested, charged with the offense, and turned over to the court for prosecution.⁹⁵ (Crimes can also be cleared by “exceptional means” in rare cases where arrest is impossible for one reason or another.)⁹⁶ The “clearance rate” is thus defined as the ratio of cleared offenses to the total number of reported offenses.⁹⁷

To get a sense of how widely clearance rates vary by crime, Table 1 provides summary statistics for our clearance rate data. (Part III describes the construction of our dataset in more detail.) The patterns match the usual patterns seen 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, robberies were cleared less than a third of the time. 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.

enforcement. See, e.g., Harris, *supra* note ___, at 392 (murder, robbery); Idilbi, *supra* note ___, at 1731 (attempted murder, rape)

⁹⁵ See FBI, Uniform Crime Report: Offenses Cleared (2013), http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2013/crime-in-the-u.s.-2013/offenses-known-to-law-enforcement/clearances/clearancetopic_final.pdf. The vast majority of cleared offenses are cleared by arrest.

⁹⁶ Cases are cleared by exceptional means in limited situations where elements beyond the control of the law enforcement agency prevent it from arresting and formally charging an offender who has been identified by the police. This might occur when, for example, the offender dies before he can be arrested. *See id.*

⁹⁷ The quality of clearance data is subject to well known criticisms. *See, e.g.*, John J. Donohue III, *Does Miranda Diminish Police Effectiveness?* 50 STAN. L. REV. 1147, 1151-55 (1998). As long as any mis-measurement in clearances is uncorrelated with Secure Communities, it should affect not bias our estimates. Rather it should only reduce the precision of our estimates. We have no reason to believe that Secure Community influenced the reporting of clearances.

Table 1. Summary Statistics for Clearance Rates of FBI Index Crimes

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.

Clearance rates have long been studied by criminologists, many of whom seek to test sociologist Donald Black's theory of law.⁹⁸ Black hypothesized that society was stratified in multiple ways and that persons occupying lower social status receive "less law" than those occupying higher ones.⁹⁹ An implication of this theory is when police exercise discretion in choosing which cases to investigate, they are more likely to pursue and make arrests in offenses with high status victims. Thus, discretionary or extra-legal factors primarily determine which cases police are more likely to clear.

Criminologists have attempted to test this prediction by looking for correlations between the probability that a crime is cleared and measures of the victim's social disadvantage. Most of these studies examine homicide cases because it is the offense for which the most details about victim characteristics are available. Their primary measures of social disadvantage are the victim's demographic characteristics. The studies commonly identify youth, women, and racial minorities as socially disadvantaged and predict that their clearance rates will be lower than those of older

⁹⁸ See Donald Black, *THE BEHAVIOR OF LAW* (1976); see also, e.g., Kenneth J. Litwin, *A Multilevel Multivariate Analysis of Factors Affecting Homicide Clearance*, 41 J. RES. CRIME & DELINQUENCY 327, 328-30 (2004); Aki Roberts, *Predictors of Homicide Clearance by Arrest: An Event History Analysis of NIBRS Incidents*, 11 HOMICIDE STUDIES 82, 83 (2007); Tanya Tussler, *Explaining the Changing Nature of Homicide Clearance in Canada*, 20 INT'L CRIM. JUSTICE REV. 366, 368-369 (2010); Graham C. Ousey & Matthew R. Lee, *To Know the Unknown: The Decline in Homicide Clearance Rates, 1980-2000*, 35 CRIM. JUSTICE REV. 41, 43-44 (2010).

⁹⁹ See BLACK, *supra* note [], at [].

white males. Yet many of these predictions are not borne out consistently in empirical studies. Most research finds that police clear homicides with white victims more often than those with non-white victims.¹⁰⁰ But many studies also find, contrary to the predictions, that police are more likely to clear homicides involving women than men, and involving younger rather than older victims.¹⁰¹

A rival theory to Black's idea of victim devaluation is that the probability of clearance depends mainly on the nature of the offense rather than the identity of the victim. In this account, police face strong incentives to secure arrests in all homicide cases and a victim's characteristics do not influence the amount of effort police are willing to invest in investigating homicides.¹⁰² Clearance rates are instead primarily a function of the evidence, and the nature of the offense largely determines the amount and type of evidence available to police. Thus, clearance rates are the product of factors over which police exercise little discretion.

Empirical studies have found greater support for the non-discretionary account. This vein of research uses aspects of the criminal incident as proxies for the availability of evidence and tests whether these proxies correlate with the likelihood of clearance. For example, police clear homicides involving weapons other than guns at higher rates, a pattern that is consistent with shootings leaving less forensic evidence behind for police than stabbings or other forms of physical attack. Similarly, homicides in private residences or involving family members have higher clearance probabilities.¹⁰³ Most recent studies conclude that these situational characteristics better explain homicide clearances than the discretionary theory.¹⁰⁴

Our focus, of course, is on neither victims nor offense characteristics. Instead we are interested in the connection between levels of community cooperation and clearance rates. The literature on clearance rates has long argued that community cooperation is a crucial input in producing arrests. As one scholar summarized, "a theme that runs through many of the previous studies was stated by Reiss several

¹⁰⁰ See Riedel, *supra* note at 1153-55.

¹⁰¹ See *id.*

¹⁰² See, e.g., Litwin, *supra* note [], at 331 (describing organizational pressure within police departments to solve all homicides).

¹⁰³ See *id.* at 1157-59.

¹⁰⁴ See Litwin, *supra* note [], at 345 ("Overwhelming support exists for the importance of nondiscretionary factors in understanding homicide clearances in a given time period"); Roberts, *supra* note [], at 89 ("The findings of the current analysis support the second perspective's claim that police discretion based on victim and offender characteristics is minimized in homicide cases, with availability of physical evidence and information being the decisive factors"); Tussler, *supra* note [], at 379-80 ("This research showed little support for Black's (1976) theory of law . . . Nondiscretionary factors substantially affect clearance rates"); Ousey and Lee, *supra* note [], at 52 (reporting that empirical "models offer little support for the discretionary perspective" and are "somewhat consistent with the nondiscretionary perspective").

decades ago, “There is no feasible way to solve most crimes except by securing the cooperation of citizens to link a person to the crime.”¹⁰⁵

This theme is reflected in studies of all perspectives. One researcher, working within Black’s victim-centric theory, described four statistically significant results for variables as diverse as home ownership rates within a locality and the victim’s ethnicity as reflecting “some aspect of witness willingness to provide useful information to police.”¹⁰⁶ Clearance scholars observing correlations inconsistent with Black’s victim-centric theory have also suggested that the patterns they find reflect differences in the degree of community cooperation with police. For example, one study found that homicides with Latino victims were much less likely to be cleared relative to those with white victims. The author conjectured that this pattern may result from community members’ reluctance to provide information to the police, which could arise from Latinos’ fear of revealing their immigrant status to authorities.¹⁰⁷ Similarly, another study considered the possibility that the lower clearance rates of black homicide victims may not indicate that police assign a lower priority to investigating such crimes. Rather, the lower clearance rates may indicate the “level of witness cooperation with investigators” and specifically that community members are “too frightened or alienated to talk to the police.”¹⁰⁸

Thus, criminologists studying the determinants of clearance rates have often alluded to a central claim of procedural justice theory. But they have not engaged that literature directly, and the procedural justice literature has paid little attention to the clearance literature. Consequently, the two literatures have remained entirely disconnected.¹⁰⁹ And despite the claims in clearance rate scholarship about the effect community cooperation has on crime clearance, existing scholarship has yet to test those claims directly.¹¹⁰

¹⁰⁵ See Riedel, *supra* note [], at 1159. See also Ousey & Lee, *supra* note [], at 45 (describing how the public’s willingness to provide information to the police influences their ability to gather evidence that leads to arrests).

¹⁰⁶ Litwin, *supra* note [], at 347. This author adds, “In Chicago, nearly two thirds of police officers agreed with the statement, ‘without citizen cooperation, the majority of crimes would never be solved.’” *Id.* at [] (quoting Wesley G. Skogan et al., ON THE BEAT: POLICE AND COMMUNITY PROBLEM SOLVING 235 (1999)).

¹⁰⁷ See Litwin, *supra* note [], at 339-40.

¹⁰⁸ Edward R. Maguire et al., *Why Homicide Clearance Rates Decrease: Evidence from the Caribbean*, 20 POLICING & SOCIETY 373, 377, 386-87 (quoting police, citing survey evidence on residents’ fear of reprisals, and discussing police mistreatment of the public). See also Janice L. Puckett & Richard J. Lundman, *Factors Affecting Homicide Clearances: Multivariate Analysis of a More Complete Conceptual Framework*, J. RES. CRIME & DELINQUENCY 171, 183 (2003) (reporting that “census-tract race composition measures matter in ways that suggest less information and cooperation from citizens in African American communities”).

¹⁰⁹ Ousey & Lee, *supra* note [], at 53 (referring to the “trust people have in the police”).

¹¹⁰ One reason for this failure is likely, as we observed above, the simple fact that cooperation is difficult for researchers to observe directly. See *id.* (“Most importantly, the degree of police-citizen integration is an inherently difficult concept to operationalize with macrosocial data . . .”). Standard

Our innovation is to combine and build on these two literatures—one concerned primarily with the reasons why people cooperate with law enforcement, the other concerned with the conditions that promote or impair effective law enforcement. For our immediate purposes, this provides us with a way to plausibly measure community cooperation with law enforcement, without having to rely on surveys about attitudes or laboratory experiments. Where a community is willing to cooperate with police in responding to crime, the rate at which police secure arrests for reported offenses should be higher. This metric has shortcomings, which we discuss below. But a key virtue is that it allows theories of cooperation with police to be connected to actual law enforcement outcomes, something that the procedural justice literature has heretofore not done.¹¹¹

In addition to employing clearance rates as a proxy for the degree of community cooperation, this Article makes an additional contribution to the study of clearance rates themselves. It is one of the very first (if not the first) to utilize variation in a specific law enforcement policy to identify the impact of community cooperation on clearance rates. Research on clearance rates has for the most part not assessed the impact of specific law enforcement programs.¹¹² A focus on public policy is almost entirely missing from the clearance rate literature. Its concern has been identifying the sociological factors that influence clearance, rather than evaluating the effects of discrete legal policies. Some studies include control variables for the number of reported offenses per police officer, which proxies for the workload of officers, but they often, but not always, find that lower workload associates with a higher

datasets of offending and law enforcement activity do not typically have quantitative measures of the specific types of evidence available to police in a criminal incident, whether it is physical evidence or a witness's testimony. Even if an effort were made to collect such details, it would likely encounter the difficulties due to the confidentiality of some cooperation and perhaps subjective judgments of whether a witness was truly or fully cooperating. These reasons make it unsurprising that clearance scholars have not tested directly the importance of community cooperation and that procedural justice scholars have relied primarily on survey evidence.

¹¹¹ Our approach does, of course, have its own empirical limitations, as does any empirical analysis. By focusing on geographic aggregates and a broader range of offenses, detailed information about the circumstances of the offense and the victims are not available. While we give up this fine-grained information, we gain considerable breadth: our analysis studies clearance rates for the full set of FBI Index I offenses, for example, while nearly all existing work focuses exclusively on homicide. See sources cited *supra* notes []-[]]. But see Jang et al., *supra* note [], at 532 (considering all Part I offenses); Aki Roberts, *The Influences of Incident and Contextual Characteristics on Crime Clearance of Nonlethal Violence: A Multilevel Event History Analysis*, 36 J. CRIM. JUSTICE 61, (2008) (examining violent crimes other than homicide). We believe this and the other advantages described above outweigh this limitation.

¹¹² An exception is Hyunseok Jang et al., *Effect of Broken Windows Enforcement on Clearance Rates*, 36 J. CRIM. JUSTICE 529 (2008). This article tests the impact of broken windows policing using fifteen years of data from 35 Texas police departments, and it finds that the estimated effect is not consistent across offense categories. Its measure of broken windows policing is not whether a police department officially embraced such a strategy, but instead the share of arrests for certain categories of crime thought be priorities under the broken windows approach. See *id.* at 533.

clearance rate.¹¹³ But again, these results are correlational, leaving the correct causal inference unclear. It could be that policies that lower workloads increase clearance rates because officers can invest more effort in each case. Or, policies that clear more crimes may lower criminal activity because clearances signal to potential offenders the detection probability.

Before proceeding, we should note an important aspect of the way in which we are using clearance rates. Some prior research has treated clearance rates as a metric of police efficacy.¹¹⁴ This interpretation is problematic. As many have pointed out, the efficacy of police depends not only the ability to solve crimes when they occur, but also to prevent crimes in the first instance.¹¹⁵ It is possible that an enforcement effort which is effective in reducing offending may also cause the clearance rate to fall.¹¹⁶ This is so because clearance rates are the ratio of crimes cleared to crimes reported and, therefore, are endogenous to enforcement policies that influence the rate of offending.¹¹⁷ While this criticism is important, our approach does *not* assume that clearance rates are a measure of, or sufficient statistic for, police efficacy. Rather, we seek to test whether policies that shape public attitudes toward the police can influence cooperation and thereby alter the frequency with which police clear crimes. In addition, as we show below, Secure Communities had no effect on crime rates. Accordingly, it is unlikely to have caused a change in criminal opportunities that would impair the ability to detect an impact on clearances.

III. SECURE COMMUNITIES' IMPACT ON COOPERATION

Procedural justice theory predicts that activating Secure Communities in a jurisdiction should undermine the legitimacy of local police and, consequently, reduce cooperation by immigrants. Reduced cooperation, criminologists and other scholars have long argued, makes it difficult for the police to solve crimes.¹¹⁸ Thus, our main prediction is that activating Secure Communities in a county will reduce the rate at which crimes are solved in the county.

¹¹³ See *id.* at 531 (collecting citations).

¹¹⁴ See, e.g., Alexandre Mas, *Pay, Reference Points, and Police Performance*, 121 QUART. J. ECON. 783 (2006); Paul-Philippe Paré, Richard B. Felson, & Marc Ouimet, *Community Variation in Crime Clearance: A Multilevel Analysis with Comments on Assessing Police Performance*, 23 J. QUANT. CRIMINOLOGY 243 (2007); Paul G. Cassell & Richard Fowles, *Handcuffing the Cops? A Thirty Year Perspective on Miranda's Harmful Effects on Law Enforcement*, 50 STAN. L. REV. 1055 (1998).

¹¹⁵ See Daniel S. Nagin, Robert M. Solow, & Cynthia Lum, *Deterrence, Criminal Opportunities, and Police*, 53 CRIMINOLOGY 74, 84 (2015).

¹¹⁶ See Steven N. Durlauf & Daniel S. Nagin, *Imprisonment and Crime: Can Both Be Reduced?*, 10 CRIMINOLOGY & PUBLIC POLICY 13, 19-21 (2011) (presenting an example in which an enforcement program deters marginal offenses and thereby lowers both the offending and clearance rates).

¹¹⁷ See Philip J. Cook, *The Clearance Rate as a Measure of Criminal Justice Effectiveness*, 11 J. PUBLIC ECON. 135, 136 (1979) (offering a theoretical model showing that if offenders adapt to improvements in the efficacy of police, clearance rates may remain unchanged).

¹¹⁸ See, e.g., REISS, *supra* note []; Schulhofer et al., *supra* note [], at [].

This Part evaluates that prediction by exploiting Secure Communities' staggered rollout. The staggered rollout permits us to use a statistical technique, known as differences-in-differences, which mirrors the randomized control trials widely used in medical research. The goal of the typical medical trial is to measure whether a particular treatment improves health outcomes. To determine this, medical researchers randomize subjects into two groups: a treatment group that receives the intervention and a control group that does not. For each group, health outcomes are measured on two dates: before and after the date the treatment is administered. The treatment is then estimated by comparing the change in the health outcomes of the treatment group before and after the administration of treatment, relative to a similar before-after change in the health outcomes of the control group.

Such estimates are called differences-in-differences because they rely on two sets of before-after comparisons: one for the treatment group and another for the control group. The before-after comparison for the control group (the first "difference") is subtracted from the before-after comparison for the treatment group (the second "difference"). This means that any changes in the treatment group are measured *relative to* any changes in the control group, which excludes the possibility that some omitted third factor—like the simple passage of time—caused any changes observed in the health of the treatment group. Randomization of subjects into treatment and control groups is also important. It assures that systematic differences in the treatment and control groups, such as preexisting trends in their health, do not account for any of the estimated changes in outcomes.¹¹⁹

Social scientists typically cannot conduct randomized control trials on social policies. Instead, they must rely on naturally occurring policy variations that, after controlling for other factors, are plausibly random. Part II demonstrated that Secure Communities is just such a natural experiment. This permits us to use differences-in-differences estimation to identify the causal effect of lower levels of legitimacy on community cooperation.¹²⁰

A. The Effect on Clearance Rates

We operationalize the differences-in-differences approach in a multivariate regression framework that controls simultaneously for a number of factors, beyond just perceived legitimacy, that may influence levels of community cooperation.¹²¹

¹¹⁹ JOSHUA D. ANGRIST & JÖRN STEFFEN PISCHKE, *MOSTLY HARMLESS ECONOMETRICS: AN EMPIRICIST'S COMPANION* 227-43 (2009); David Card & Alan B. Krueger, *Minimum Wages and Employment: A Case Study of the Fast Food Industry in New Jersey and Pennsylvania*, 84 AM. ECON. REV. 772 (1994).

¹²⁰ Mark R. Rosenzweig & Kenneth I. Wolpin, "Natural Experiments" in *Economics*, 38 J. ECON. LIT. 827 (2000).

¹²¹ We estimated ordinary least squares regressions, with the estimating equation taking the form $C_{it} = g(\text{Activate}_{it})\delta + X_{it}\beta + a_i + a_t + \varepsilon_{it}$. The dependent variable 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. The independent variable Activate_{it} represents whether Secure Communities is active in county i on date t . Several different functional forms of $g(\bullet)$ are used to capture different

Thus, in addition to information on our treatment (Secure Communities) and predicted effect (cooperation as proxied by clearance rates), we collected a large amount of demographic information about each county, as well as information on each county's median income, from the Census Bureau.¹²² It is worth noting that the decennial census does not determine the number of noncitizens in each county.¹²³ But it does determine for each county the number of foreign-born and Hispanic persons, and these groups are closely correlated with noncitizen status.

Because Secure Communities was activated month-by-month at the county level, the unit of observation in the regression models is a county-month. Thus, the assembled data is a panel of monthly, county-level observations. The observation period runs from 2004 to 2012: it ends in 2012 because that is the most recent year for which nationwide clearance rate data is available, and it begins in 2004 to balance the number of years before and after Secure Communities launch in late 2008. The full dataset thus contains nearly 300,000 observations.

Table 2 reports the results of the clearance rate regressions. Each of the four columns in the table represents a different regression model. These different models capture two refinements to our basic differences-in-differences approach.

The first refinement relates to how we measure the “treatment” of Secure Communities in each county. Our basic measure of the treatment is binary: the program is either “off” or “on” in a particular county.¹²⁴ Yet even though the program was applied uniformly to every activated county, the intensity of the program's treatment may have varied across counties. To account for this possibility, we obtained detailed operational data on Secure Communities through a series of FOIA requests. These data, provided by the Department of Homeland Security, include the precise number of persons taken into custody by ICE under the Secure

ways of conceptualizing activation. The vector X_{it} contains a set of county- and date-varying control variables that are commonly included in studies of crime, including 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.) 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.

¹²² See USA Counties (Census Bureau), <http://censtats.census.gov/usa/usa.shtml>.

¹²³ The decennial census last collected information on citizenship status in 2000. After the 2000 census, the census “long form”—which contained the citizenship status question—was discontinued and replaced by the American Community Survey (ACS). The ACS uses monthly surveys to sample the population characteristics previously surveyed by the long form. See U.S. Census Bureau, American Community Survey: Design and Methodology (2006), <https://www.census.gov/history/pdf/ACSHistory.pdf>.

¹²⁴ This binary measure is operationalized using a series of binary indicator variables that take the value of “1” when Secure Communities is active in a county during a particular month and the value “0” otherwise.

Communities in each county and month.¹²⁵ The data reveal widely varying rates of ICE detention across counties. Unsurprisingly, detentions under the program have been concentrated in counties with the largest foreign-born populations.¹²⁶ This suggests that relying exclusively on a binary measure of activation might introduce error into our measurement of the program's treatment and lead us to underestimate its effect (a problem economists describe as attenuation bias). For this reason, we use ICE detention rates as a continuous measure of the program's intensity in a county.¹²⁷

In addition to ameliorating the problem of attenuation bias, this direct measure of the program's intervention in theory permits us to distinguish between the instrumental and legitimacy-based accounts of immigrant cooperation. If immigrants are attentive to the actual risk that a police interaction will lead to negative immigration consequences, then the detention rate under the program may be the most theoretically appropriate measure of the program's intervention. But if, on the other hand, immigrants are focused primarily on the perceived legitimacy of local police, the simple fact of activation may be the most appropriate treatment measure.¹²⁸

The second refinement provides alternate ways of controlling for differences in clearance rates across counties. The baseline regression includes "fixed effects" (a dummy variable) for each county. These control variables remove any pre-treatment differences in clearance rates across counties prior to testing the program's impact. It is possible, however, that counties differ not just in their pre-treatment clearance rates but in the trend of those rates prior to the activation of Secure Communities. If clearances were trending upward in some counties, but trending downward in others, there is a risk of mistaking these general trends in clearance rates for the program's impact. In our research design, this would occur if counties that had declining clearance rates also happened to be more likely to be activated earlier under Secure Communities.¹²⁹ To avoid this possibility, we ran alternate regression specifications

¹²⁵ In addition to data on detentions, the Department of Homeland Security also provided, for each county-month, the number of database submissions and "hits," as well as the number of persons deported pursuant to the program. This gives us a snapshot of the enforcement pipeline from the point of initial local arrest to ultimate federal deportation.

¹²⁶ See Miles and Cox, *Does Immigration Enforcement Reduce Crime?*, *supra* note [], at [].

¹²⁷ In the regressions, this measure is implemented by interacting the dummy variable for activation with the ICE detention rate. The ICE detention rate is defined, for each county-month observation, as the *cumulative* number of persons detained under the program in the county through that month. In Part III.A.3 below we also evaluate the effect of defining detention rates in different ways.

¹²⁸ 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.

¹²⁹ In related work, we show that crime rates fell steadily over this time period, especially in counties with high foreign-born populations. See Miles & Cox, *supra* note [], at [].

that include county-level trends rather than fixed effects. In effect, these specifications remove a linear trend from each county before testing for the impact of the program.

Table 2. Impact of Secure Communities on the Clearance Rate of FBI Index Crimes

Explanatory Variable	(1)	(2)	(3)	(4)
<u>Regression Specification A</u>				
Activated	.0024 (.0045)	.0028 (.0049)	-.0018 (.0013)	-.0013 (.0015)
<u>Regression Specification B</u>				
Activated x 75 th Percentile of Fraction Pop. Foreign Born	-.0003 (.0049)	-.0001 (.0040)	-.0019 (.0013)	-.0016 (.0016)
Activated x Below 75 th Percentile of Fraction Pop. Foreign Born	.0108** (.0039)	.0088** (.0040)	.0028 (.0017)	.0013 (.0018)
<u>Regression Specification C</u>				
Activated x 75 th Percentile of Fraction Pop. Hispanic	-.0024 (.0051)	.0003 (.0063)	-.0021 (.0013)	-.0016 (.0017)
Activated x Below 75 th Percentile of Fraction Pop. Hispanic	.0111* (.0041)	.0060 (.0040)	-.0029* (.0015)	-.0002 (.0017)
<u>Regression Specification D</u>				
Activated x Border County	.0191** (.0063)	.0186* (.0099)	.0029 (.0012)	.0042* (.0025)
Activated x Not Border County	.0018 (.0044)	.0024 (.0048)	-.0023 (.0013)	-.0017 (.0016)
<u>Regression Specification E</u>				
Activated x First Year	-.0063 (.0084)	.0019 (.0084)	-.0026 (.0021)	-.0001 (.0026)
Activated x Second Year	.0041 (.0052)	-.0022 (.0056)	-.0012 (.0014)	-.0028 (.0020)
Activated x Third Year	.0097* (.0049)	.0047 (.0051)	.0009 (.0020)	-.0019 (.0020)
Activated x Fourth Year	-.0060 (.0051)	-.0001 (.0053)	-.0012 (.0026)	.0035 (.0026)
Measure of Secure Communities?	Activated	Activated	Persons in ICE custody x Activated	Persons in ICE custody x Activated
Includes County-level Trends?	N	Y	N	Y

Notes: OLS regression estimates, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the monthly clearance rate of FBI index crimes. The table reports regression coefficients for the listed explanatory variable, with standard errors in parentheses. Coefficients for control variables are omitted in order to conserve space. N = 267,010. Number of counties in sample = 2,864.

1. *Baseline Estimates for FBI Index Offenses*

Table 2 begins with Regression Specification A, which provides a set of baseline estimates for whether Secure Communities' activation influenced clearance rates. The regressions in the first two columns use our binary measure of program activation. The regressions in the second two columns replace the binary measure of program activation with our continuous measure of the program's intensity within a county. Labels at the bottom of each column indicate whether that column's model includes county-level fixed effects or, instead, county-level trends.

Specification A suggests that Secure Communities had little effect on the ability of local law enforcement to solve crimes. In fact, the first two estimates for this specification—the ones utilizing the binary measure of activation—are *positive*. If taken at face value, this would suggest that clearance rates rose as a result of the program's activation, precisely the opposite of the predictions of procedural justice theory.

While the estimates change signs when we use detention rates as the treatment measure, all of the baseline estimates are very small in magnitude, and none of them are statistically significant. For example, the differences-in-differences estimate in column (2)—which is the largest in magnitude—implies that Secure Communities *raised* the clearance rate of index crimes by about one quarter of one percentage point (0.25%). 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 magnitude of the negatively signed coefficients are even smaller. The magnitude of the estimate in column (4), for example, implies that a 10% increase in detentions under Secure Communities would lower the clearance rate by .013%. This implied response is very small.

These estimated effects are not just close to zero; they are also very precisely estimated. To give the most generous interpretation of the estimates for any clearance-reducing effect, consider the lower bound of the 95% confidence interval. For the baseline estimate in column (1) of Specification A, the bottom of the confidence interval is -.0070. The implied effect of this lower bound is very small: it amounts to a less than one percentage point reduction in clearance rates, which is tiny when compared to the sample mean clearance rate of about 28%. Moreover, the manner in which Secure Communities is measured does not influence this conclusion. When the program's intensity is measured using the rate of federal detention (in column (3)) rather than mere activation, for example, the lower bound of the 95% confidence interval remains close to zero: -.0045. The precision of the estimates is such that if activation of Secure Communities caused even a one percentage point reduction in clearance rates, it would be statistically significant. Thus, the failure to detect an effect of Secure Communities on clearance rates is not due to a lack of precision; it is due to the absence of any actual effects.

2. *Heterogeneous Treatment Effects*

In addition to the possibility that the policy's treatment varied from county to county, a county's response to that treatment might vary with its characteristics—such as the proportion of immigrants in its population. This raises the possibility that the treatment effect will be heterogeneous. To account for this, 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. Specification D makes a comparison between counties that are on and not on the southern border. The final specification, Specification E, 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 .0191 for border counties 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 foreign-born populations 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.

3. *Sanctuary Cities and Anti-Detainer Policies*

Another important way in which counties might differ is in the efforts they make to combat the de-legitimizing effects of Secure Communities. As we explained earlier, counties cannot avoid participating in the mandatory fingerprint screening system at the core of Secure Communities. But they could try to interfere with the federal government's ability to take custody of immigrants identified through the program. They have attempted to do this in two ways. First, some local governments have adopted sanctuary policies.¹³⁰ These policies restrict local police and other government actors from cooperating with federal immigration enforcement. The specific nature of these policies varies across jurisdictions, but the common element of these policies is that by preventing local officials from cooperating with federal immigration efforts, they created a zone of safety or sanctuary for immigrants. The

¹³⁰ See Rose Cuison Villazor, *What is a "Sanctuary"?*, 61 S.M.U. L. REV. 133 (2008) (describing the evolution of sanctuary policies over time).

adoption of sanctuary policies occurred mostly before the launch of Secure Communities.¹³¹ The second way in which communities may constrain the ability of the federal government to take custody of immigrants is through so-called anti-detainer policies, which are a direct response to Secure Communities. Under such a policy, local authorities refuse to honor requests from the federal government that immigrants be held for up to forty-eight hours after they ordinarily would be released.¹³² Refusing to honor a detainer request makes it more likely that an immigrant will be released from local custody before federal officials arrive to take custody of the person. Anti-detainer policies were rare prior to 2013, and they have since proliferated—a testament to the view that Secure Communities undermines immigrant trust in local police, as well as to the belief that public actions by local law enforcement agencies can help rebuild that trust.¹³³

The fact that a local jurisdiction adopted one of these policies may indicate concern, within the community, that local immigration involvement is especially likely to impede community cooperation with police in that locality. That prediction cannot be tested directly, because of the timing of these policies. Sanctuary policies were mostly adopted before our sample period, and anti-detainer policies mostly arrived after it, which precludes the possibility of making comparisons within a county before and after the adoption of such a policy. That is, their timing does not permit us to develop difference-in-difference estimates of their impact on clearance rates. Nevertheless, it is possible to test whether counties that had already adopted sanctuary policies, or that would later adopt anti-detainer policies, had a more pronounced or differential reaction to Secure Communities than other counties.

¹³¹ See Trevor Gardner II, *The Case for Immigrant Sanctuary: Federalism, Local Penology, and the Ideological Challenge to the Homeland Security Model* (working paper); Villazor, *supra* note [], at []. Trevor Gardner generously shared with us his data on the adoption of sanctuary city policies. Those data are used as the measure of sanctuary policies in the Table 3 regressions.

¹³² See Christopher N. Lasch, *Federal Immigration Detainers after Arizona v. United States*, 46 *LOY. L.A.L. REV.* 629 (2013).

¹³³ See Catholic Immigration Legal Network (CLINIC), *States and Localities That Limit Compliance with ICE Detainer Requests* (July 2015), <https://cliniclegal.org/resources/articles-clinic/states-and-localities-limit-compliance-ice-detainer-requests-jan-2014> (listing local anti-detainer policies by jurisdiction and date of adoption). CLINIC is the source of data on anti-detainer policies for the Table 3 regressions.

Table 3. Sensitivity of Estimates to Local Immigration Policies

Explanatory Variable	(1)	(2)	(3)	(4)
Persons in ICE Custody	-0.0013 (.0015)			
Persons in ICE Custody x Local Immigrant Policy		-.0027 (.0021)	-.0008 (.0020)	-.0023 (.0021)
Persons in ICE Custody x No Local Immigrant Policy		-.0003 (.0016)	-.0015 (.0017)	-.0005 (.0017)
Local Immigrant Policy		Anti- detrainer	Sanctuary	Either anti-detrainer or sanctuary

Notes: OLS regression estimates, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the monthly clearance rate of FBI index crimes. The table reports regression coefficients for the listed explanatory variable, with standard errors in parentheses. Coefficients for control variables are omitted in order to conserve space. $N = 267,010$. Number of counties in sample = 2,864.

Table 3 shows the results of interacting the variables for the detention rate under Secure Communities with dummies for the (eventual) presence of one these local policies. For comparison purposes, the regression in column (1) repeats the baseline estimate from Table 2. The remaining columns show estimates in which the detention variable is split into two, one for counties with the local policy and a second for other counties. Column (2) shows the interaction for anti-detainer policies, and column (3) shows it for sanctuary policies. Only about 10% of counties have adopted either of these two policies, and they are correlated: 55% of counties with sanctuary policies later adopted anti-detainer policies. Column (4) reports estimates for counties with either of the two policies. The results do not support the hypothesis that counties adopting these policies were ones especially likely to suffer a loss of cooperation as a result of Secure Communities. None of the estimates in Table 3 are statistically significant and, like the baseline estimate, all are close to zero. On the whole, a thorough testing of Secure Communities, including an examination of the intensity of the program’s intervention and a study of locations where its impact is likely to be greatest, shows that the program had no effect on the overall rate at which police clear FBI index crimes.

4. Robustness Checks

To probe the sensitivity of the estimates, Table 4 provides a series of robustness checks. The baseline regression for these checks is the equation in column (3) of Specification (A) in Table 2—the equation in which we utilized, as a direct measure of Secure Communities’ intensity, the number of persons detained by ICE under the program. We repeat this baseline regression in the column (1) of Table 4 in order to make it easier to compare the findings in this table to that baseline finding from Table 2. Each subsequent column of this new table reflects a variation on that baseline equation, and each column shows the coefficient on the (log of) the detention rates under Secure Communities or, where noted, a variation of it.

In modeling the frequency with which police clear crimes, the size of the police force is likely to be an important factor. For that reason, all of the regressions in Table 2 include a measure of police employment per capita as a control variable.¹³⁴ But the data for police employment are missing for several counties in some years. The missing values cause the observations for those county-months to be dropped from the sample, which leads the panel to be unbalanced. In column (2) of Table 4, we test whether our estimates are sensitive to the removal of these observations. They are not: although the estimate changes sign, it remains both small in absolute value and statistically insignificant.

Under Secure Communities, ICE apprehended immigrants with widely varying criminal histories. Nearly one-third of all immigrants apprehended had no criminal record at all; less than one-third had been convicted of a crime that ICE deemed the most serious.¹³⁵ This raises the possibility that our nearly zero baseline estimate results from aggregating the effect of detaining immigrants who have severe criminal histories together with the effect of detaining immigrants 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 at the same time 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 with criminal histories (and perhaps particularly those with the most serious convictions) would correlate positively with the clearance rate, while detentions of immigrants without criminal histories would correlated negatively with it.

¹³⁴ See *supra* note [], (describing the control variables included in all of the models).

¹³⁵ See Miles & Cox, *supra* note [], at [] (showing graphically the composition of the detainee pool under Secure Communities over time). It is important to note that even the crimes ICE deems the most serious offenses include a fair number of nonviolent offenses and even some misdemeanors. See *id.* at [] (describing the way in which ICE defines the criminal history categories that it uses to classify immigrants).

Table 4. Testing the Sensitivity of the Clearance Rate Estimates

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Persons in ICE Custody	-0.0018 (.0013)	.0022 (.0045)		-0.0019 (.0065)	-0.0028 (.0019)			.0053** (.0024)
Sets of Fingerprints Submitted						.0033 (.0027)		.0010 (.0074)
Persons Deported							.0016 (.0042)	-.0062 (.0065)
L1 Persons in ICE Custody			-.0007 (.0067)					
L2/L3 Persons in ICE Custody			-.0016 (.0069)					
Noncriminal Persons in ICE Custody			.0036 (.0071)					
Change to Baseline Regression Specification		Exclude Police per Capita		Custody Measured as Flow	Custody Measured per Foreign-born			

Notes: OLS regression estimates, ** $p < 0.05$, * $p < 0.1$. The dependent variable is the monthly clearance rate for FBI index crimes. The regressions include the same control variables used in the regressions in column (2) of Table 2. The table reports regression coefficients, with standard errors in parentheses. In all columns but column (2), $N = 325,462$, and the number of counties in sample = 2,985. In column (2), $N = 306,244$, and the number of counties = 3,113.

To test this possibility, the equation in column (3) decomposes the detainees by the criminal history classifications assigned to them by ICE. Detainees in category L1 have the most serious criminal histories, those in categories L2 and L3 have less serious criminal records, and “noncriminal” detainees have no criminal records when they are taken into custody by ICE under Secure Communities.¹³⁶ The estimates in column (3) do not bear out the prediction. All of the estimates are close to zero and statistically insignificant. 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

¹³⁶ See *id.* at [].

shapes clearance rates principally by changing the probability of ICE apprehension, then this flow measure would provide a more accurate measure of the risk faced by immigrants in a particular county in a particular month—and thus potentially be a superior measure of the policy intervention. Yet this alternative way of measuring the program’s intensity has no meaningful effect: the coefficient is almost identical to the baseline estimate. To try yet another alternative measure, column (5) 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. Again, however, 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 fingerprint submissions under the program. If the program prompted local police to alter their enforcement practices, such as by engaging in racial profiling of Hispanics, then the relevant measure of the program’s intervention might be better conceptualized as the rate at which local 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 on immigrants’ perceptions of the risks created by the program. 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 3 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 point to the conclusion that Secure Communities has no impact on clearance rates, a conclusion that appears robust to different ways of measuring the program’s intervention.

5. *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—raising the possibility that any impact of Secure Communities on clearance rates might also vary by offense category. Table 5 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—that is, the baseline differences-in-differences estimates. The even numbered columns show the estimates when the detention rate measure is instead used to measure the program’s intensity.

Table 5. Impact of Secure Communities on Clearance Rates of Specific Crimes

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

Notes: OLS regression estimates, ** $p < 0.05$, * $p < 0.1$. The table reports regression coefficients, with standard errors in parentheses. Each regression coefficient represents a separate regression for which the dependent variable is the listed crime, and the explanatory variable is the measure of Secure Communities indicated at the bottom of the column. The regressions include the same control variables used in the regressions in column (2) of Table 2.

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 5 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 insurance requirements give motorists a strong incentive to report stolen vehicles.¹³⁷ In addition, many motor vehicle thefts occur 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.¹³⁸ 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 2, 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 results for simple assault are 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 cooperation—often in the form of testimony by the victim herself—to clear through arrest.¹³⁹ Simple assault thus provides perhaps the best way to capture the concern that Secure Communities will suppress victim participation in cases involving domestic violence or other forms of violence among intimates, where victims may often be concerned not only about their own immigration consequences, but also about the immigration consequences for the perpetrator. Here again, estimates for simple assault are close to zero, inconsistently signed, and not statistically significant.

B. The Effect on Rates of Reported Crime

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.¹⁴⁰ Thus, crime rates could be interpreted as a second measure of community cooperation. If the level of immigrant cooperation with the

¹³⁷ See Callie M. Rennison, *Criminal Victimization 2000: Changes 1999-2000 with Trends 1993-2000*, BUREAU OF JUSTICE STATISTICS (2001).

¹³⁸ See Christopher T. McDonald, *The Changing Face of Vehicle Theft*, 78 POLICE CHIEF (2011); Ian Ayres & Steven D. Levitt, *Measuring Positive Externalities from Unobservable Victim Precaution: An Empirical Analysis of LoJack*, 113 Q. J. ECON. 43, 60-61 (1998).

¹³⁹ See, e.g., Barbara E. Smith, *Evaluation of Efforts to Implement No-Drop Policies: Two Central Values in Conflict*, Department of Justice, National Institute of Justice (March 2001), <http://www.ncjrs.gov/App/Publications/abstract.aspx?ID=187772>.

¹⁴⁰ See, e.g., Robert J. Sampson et al., *Neighborhoods and violent crime: A multilevel study of collective efficacy*, 277 SCIENCE 918 (1997).

police declined in the wake of Secure Communities' rollout, therefore, one might predict that crime rates would rise. In fact, this claim has been made explicitly by many opponents of the program, who have argued that Secure Communities will increase local crime and decrease community safety.¹⁴¹

In related work, we have explored the relationship between Secure Communities and crime rates. Our analysis in *Does Immigration Enforcement Effect Crime?* found no evidence that Secure Communities reduced the overall rate of FBI index crimes.¹⁴² Crime rates did indeed fall around the nation during the period of Secure Communities' rollout, and they fell especially fast in counties with larger shares of foreign-born persons. But after controlling for these trends, our analysis detected no impact of the program on aggregate rates of crime.¹⁴³ The failure to find an impact on crime rates reinforces the interpretation that the program was, in the main, an immigration enforcement effort.¹⁴⁴

To make it easier to compare these crime rate findings to the clearance rate findings above, Table 6 (contained in the Appendix) presents a set of estimates for crime rates that parallel those for clearance rates in Table 2. These results confirm the findings in our earlier article. The most prominent feature of the estimates in Table 5 is the importance of controlling for county-level trends. In the absence of these trends, the various measures of the program correlate with declines in crime rates, and in some instances, substantial declines. But when the regression removes the trend of crime within each county, the point estimates uniformly fall to zero or close to it. None of them remain statistically significant above the .05 level. This pattern is consistent both in the baseline specification in Regression A and in the four alternatives that consider counties with proportionately high foreign-born and Hispanic populations, counties on the southern border, and counties by year of activation. The results in Table 6 confirm that Secure Communities had no meaningful effect on crime rates.

The lack of evidence that Secure Communities had any impact on crime rates is important for three reasons. First, it is consistent with our clearance rate finding in

¹⁴¹ See *supra* text accompanying notes []-[].

¹⁴² See Miles & Cox, *Does Immigration Enforcement Reduce Crime?*, *supra* note 8, at []. Relying on our findings, the New York Times argued for the abolition of Secure Communities. See Editorial Board, *The "Secure Communities" Illusion*, N.Y. Times, Sept. 5, 2015, <http://www.nytimes.com/2014/09/06/opinion/the-secure-communities-illusion.html>. On November 20, 2014, the Secretary of Homeland Security Jeh Johnson announced publicly that "The Secure Communities program, as we know it, will be discontinued." Memorandum from Jeh Charles Johnson, Department of Homeland Security, Nov. 20, 2014, http://www.dhs.gov/sites/default/files/publications/14_1120_memo_secure_communities.pdf. Nonetheless, the core aspect of the program—mandatory immigration screening for all local arrestees—continues under the new label of the "Priority Enforcement Program." See *id.*

¹⁴³ See *id.*

¹⁴⁴ See Cox and Miles, *supra* note [] (exploiting the program's rollout timing to test competing hypotheses: first, that the program was thought of as implemented as a crime control strategy; second, that it was designed as an immigration control measure).

rejecting the prediction of the cooperation theories that we laid out in Part I.¹⁴⁵ Moreover, while evaluations of public programs often consider only one outcome, which simplifies drawing normative conclusions, an evaluation that considers two outcomes permits us to make more complex normative judgments about the value of a program like Secure Communities. For example, if clearance rates remained unchanged while crime rates fell, it would suggest that the program succeeded at reducing the frequency of offending while not reducing the rate at which police “solved” crimes. This would suggest that the program succeeded along both dimensions. But the results in Table 6 demonstrate that this possibility can be excluded.

Second, our finding that the program did not reduce crime rates ameliorates the concern that Secure Communities might have affected clearance rates through an alternative causal pathway to the one we focus on in this Article. Had Secure Communities affected the crime rate, that change itself could have altered clearance rates. The reason is that clearance rates are defined as the number of offenses solved divided by the total number of offenses. The denominator of this fraction measures crimes, and changes in the number of offenses will mechanically change the clearance rate unless there is also a change in the number of offenses solved by the police. Now, of course, if there are more crimes there will be more opportunities to solve crimes, so there is no deterministic relationship between crime rates and clearance rates. But it is easy to see why changes in the former might affect the latter. If crime rates declined significantly as a result of Secure Communities, for example, clearance rates might have gone up simply because the police were able to devote their resources to solving a smaller number of crimes—increasing their success rate. Thus, the fact that we find no change in crime rates gives us greater confidence in the robustness of our clearance rate finding.

Third, the crime rate finding alleviates concerns one might have about the fact that the FBI’s UCR data, like almost all crime data, cannot measure the *actual* number of criminal offenses and so is restricted to measuring the *reported* number.¹⁴⁶ The fact that we measure reported offenses suggests that there are actually two competing predictions one might have about the effect of community cooperation on crime rates. On the one hand, crime rates might go up as cooperation goes down. On the other hand, *reported* crime rates might actually go down if immigrant community members lose trust in the police and shy away from reporting crimes.¹⁴⁷ 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

¹⁴⁵ See *supra* text accompanying notes []-[] (discussing limitations with clearance rates).

¹⁴⁶ See FBI, Uniform Crime Report: Offenses Cleared (2013), http://www.fbi.gov/about-us/cjis/ucr/crime-in-the-u.s/2013/crime-in-the-u.s.-2013/offenses-known-to-law-enforcement/clearances/clearancetopic_final.pdf.

¹⁴⁷ 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.

fewer number of reported offenses.¹⁴⁸ But we found no decline in reported crime rates. This is possible under the under-reporting theory only if Secure Communities led to a rise in actual crime rates that was somehow magically offset by an equal decline in reporting. This possibility seems farfetched.

C. Implications

In short, we find no evidence that Secure Communities caused a reduction in the rate at which FBI index crimes are cleared. Our evaluation of individual crimes points to the same overall conclusion. While there is suggestive evidence that Secure Communities may have caused a small decrease in the clearance rate of one property offense (motor vehicle theft), seven of the eight individual crimes we examined have clearance rates that correlate either positively or not at all with Secure Communities. In fact, if the estimates for individual crimes were taken at face value, they imply that the program actually *increased* the rate at which murder and aggravated assault are cleared. In our judgment those individual estimates should be taken with a grain of salt. Nonetheless, they reinforce the overall pattern of our empirical analysis, in which basically every estimate of the program's effect is both close to zero and so precisely estimated that we would be able to detect very small movements in clearance rates. Combining that clearance rate finding with the lack of evidence that the program had any effect of reported crime rates provides strong evidence supporting the conclusion that Secure Communities did not reduce community cooperation with the police.

To consider the implications of this finding, recall the causal structure of procedural justice theory:



Given this two-step structure, there are two principal ways our finding might be interpreted, each of which has significant implications for theories about legitimacy and cooperation. The first possibility is that the perceived legitimacy of law enforcement does not significantly affect levels of community cooperation with the police. Perhaps people decide whether to cooperate largely for other reasons—such as self-interest, or a sense of personal morality. This conclusion would call into question a long line of procedural justice scholarship, which has argued on the basis of surveys and lab experiments that the perception of legitimacy is the most powerful

¹⁴⁸ 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.

predictor of cooperative behavior.¹⁴⁹ Shifting the focus from surveyed beliefs to real-world behavior yields a starkly different conclusion about whether policies designed to promote procedural justice will reap immediate crime-fighting dividends.

That does not mean, of course, that we should necessarily reject reform efforts designed to enhance procedural justice. The discrete policies often discussed in the procedural justice literature—policies aimed at enhancing the procedural regularity of the criminal justice system, reducing the unequal treatment of marginalized communities, and so on—have much else to recommend them. Our findings simply suggest that these policies might be better defended on more traditional grounds of due process and equal protection than on the ground that they are critical to law enforcement success.¹⁵⁰ Moreover, our findings are focused on seven relatively serious crimes. Serious crime is, of course, an important domain, but that does not diminish the goal of determining whether our findings hold in other contexts—such as with respect to minor, “quality of life” offenses that are the focus of some procedural justice studies.¹⁵¹ For this reason, it would be valuable to complement existing procedural justice scholarship with more studies measuring real-world behavior rather than just surveyed beliefs, building on this Article’s first step in that effort.

The second possible interpretation of our findings is that Secure Communities did not lead immigrants to lose trust in local police or to see them as less legitimate. (In other words, perhaps the first stage of the causal process was not satisfied.) As we explained in Part II, procedural justice theory predicts that an intervention like Secure Communities should de-legitimize local police—by causing immigrants to doubt the fairness of police procedures, the equality of their treatment in police encounters, and the trustworthiness of police motives.¹⁵² This implication of the theory has been advanced by leading procedural justice scholars. Moreover, the belief that Secure Communities would have these effects has been far from limited to academics; it also became the leading critique of Secure Communities lodged by public officials, policy organizations, and community activists. Fear of the program’s de-legitimizing effects may even have helped precipitate the program’s ultimate demise.

If this widely-held belief was mistaken, and Secure Communities had no effect on police legitimacy, that conclusion would cast doubt on the legitimacy-based

¹⁴⁹ See *supra* Part I.B.

¹⁵⁰ In this sense, our findings suggest some skepticism about the happy story told by much procedural justice scholarship. In that story, reforms that enhance participation, process, and equality in policing represent win-win policies—protecting the rights of community members while simultaneously promoting the public safety goals of law enforcement officials. While this account has considerable political appeal, in reality it may not often be the case that we can have our cake and eat it too.

¹⁵¹ See, e.g., TYLER, *supra* note [] (focusing on mostly minor law breaking, such as driving over 55 miles per hour on the highways, parking in violation of the law, and littering in violation of the law).

¹⁵² See *supra* text accompanying notes []-[].

arguments frequently advanced against cooperative immigration enforcement.¹⁵³ More importantly, if procedural justice theory predicts these de-legitimizing effects, but if in fact they did not occur, then we would have to ask: Where did the theory go wrong?¹⁵⁴ First, perhaps critics of Secure Communities were working with an excessively optimistic account of how immigrant-police relationships operate 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.¹⁵⁵ If baseline levels of trust are already low, a new program like Secure Communities may not reduce them further.¹⁵⁶ Relatedly, if immigrants (like many citizens) 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.

More theoretically, perhaps procedural justice theory is overly optimistic about the ability of discrete policy interventions to significantly change public attitudes about the police. The theory argues that actual police practices shape a person’s perceptions about whether those practices are procedurally just, and those perceptions in turn alter a person’s beliefs about the legitimacy of the police. But causation may often run in the opposite direction: a person’s beliefs about whether the police are legitimate may instead shape her perceptions about how she is treated by the police.¹⁵⁷ To put it slightly differently, a person’s beliefs about the police may

¹⁵³ See *supra* sources cited in notes []-[].

¹⁵⁴ From time to time, there is a suggestion in procedural justice scholarship that only direct empirical evidence about the beliefs of individuals can tell us anything about the procedural justice implications of particular policies. On such a view, our question might seem misplaced: the theory did not go astray, one might argue, because the theory simply does not tell us anything definite until we go out and measure beliefs. Of course, the implication of this view is that procedural justice theory provides no testable predictions about the way in which government policies shape public beliefs about government institutions.

¹⁵⁵ See, e.g., Robert J. Sampson & Dawn Jeglum Bartusch, *Legal Cynicism and (Subcultural) Tolerance of Deviance: The Neighborhood Context of Racial Differences*, 32 LAW & SOCIETY REV. 777(1998); Tyler & Fagan, *supra* note []. That said, there is some evidence that immigrant communities have *less* cynicism than similarly situated native communities, see Kirk et al., *supra* note [] at 92, and that “residents of immigrant neighborhoods are actually more cooperative with the police than are residents of native-born neighborhoods.” *Id.* at 93. While this evidence is limited—based on correlations in survey responses—it suggests that it is at least not obvious that immigrants, in general, start from a place of deep distrust of local law enforcement.

¹⁵⁶ This conclusion would have implications far beyond Secure Communities, because many police policies criticized on procedural justice grounds are policies that impact communities that already harbor low levels of trust in law enforcement.

¹⁵⁷ See David J. Smith, *The Foundations of Legitimacy*, in LEGITIMACY AND CRIMINAL JUSTICE: INTERNATIONAL PERSPECTIVES 29, 32–33 (Tom Tyler et al. eds. 2007) (exploring this possibility).

often be far too sticky to be affected significantly by any individual policy reform—even a reform as far-reaching and widely publicized as Secure Communities. Again, this does not necessarily mean that constructing just institutions is beside the point. One hopes that, over the long run, well-designed public policies can promote the legitimacy of law enforcement and promote cooperation with the police. But to the extent perceptions of legitimacy are the product of long-term social processes, it is a mistake to believe that discrete procedural justice interventions will significantly alter the beliefs of community members over the short run.

Before concluding, we turn to one final way in which one might attempt to interpret our core finding. In theory, it is possible that Secure Communities both delegitimized local police *and* undermined voluntary immigrant cooperation, but did so without affecting the rate at the police were able to solve crimes. This could have occurred if it turned out that the voluntary cooperation contemplated by procedural justice scholars—calling the police, providing leads, and so forth—was simply not all that important for crime-solving. Maybe other investigative techniques, including the less-than-fully-voluntary “cooperation” often obtained from co-conspirators or others involved in a criminal enterprise, are much more important in most criminal contexts. If involuntary cooperation is a much more significant crime-fighting tool than is acknowledged by the procedural justice literature, then many criminal justice interventions that implicate legitimacy could in theory be double-edged swords from the perspective of cooperation—increasing involuntary cooperation even as they undercut the more voluntary sort. One might even tell such a story about Secure Communities: perhaps the program increased rates of involuntary cooperation by providing local police with an additional threat they could use to coerce assistance from otherwise uncooperative immigrants.

Ultimately, we are somewhat skeptical of this final possibility. But the larger point is this: Every possible interpretation of our core finding has significant implications for how we think about the connection between legitimacy and cooperation, as well as for how we evaluate the role of procedural justice theory in mediating that relationship.

IV. CONCLUSION

The question why people cooperate with the police is as important as it is old. Recently, answering the question for one group of people—immigrants—has become all the more pressing. Many are worried that immigrant cooperation with the police is threatened by the growing role local police play in the enforcement of federal immigration law. Procedural justice scholars, policymakers, and advocates all argue that this role for local law enforcement threatens to undermine the trustworthiness and legitimacy of local police in the eyes of immigrants. The loss of legitimacy, they contend, will curtail immigrant cooperation and threaten public safety.

This Article capitalizes on the natural experiment provided by Secure Communities—the largest integration of local police into federal immigration

enforcement in the nation's history—to test empirically whether de-legitimizing police policies actually undermine cooperation with law enforcement. We find no evidence that the activation of Secure Communities reduced cooperative behavior by immigrants. This finding has important implications for how we evaluate increasingly widespread policies of cooperative immigration federalism. It also raises critical questions about the power of procedural justice theory to predict the real-world consequences of police policy interventions.

APPENDIX

Table 6. Impact of Secure Communities on Crime Rate of FBI Index Crimes

Explanatory Variable	(1)	(2)	(3)	(4)
<u>Regression Specification A</u>				
Activated	-.0400** (.0173)	.0025 (.0118)	-.0163** (.0051)	-.0006 (.0045)
<u>Regression Specification B</u>				
Activated x 75 th Percentile of Fraction Pop. Foreign Born	-.0545** (.0191)	-.0024 (.0144)	-.0165** (.0052)	-.0006 (.0048)
Activated x Below 75 th Percentile of Fraction Pop. Foreign Born	.0067 (.0154)	.0120 (.0095)	-.0067 (.0063)	-.0007 (.0045)
<u>Regression Specification C</u>				
Activated x 75 th Percentile of Fraction Pop. Hispanic	-.0522** (.0212)	.0067 (.0166)	-.0052** (.0212)	.0014 (.0051)
Activated x Below 75 th Percentile of Fraction Pop. Hispanic	-.0152 (.0139)	-.0027 (.0088)	-.0152 (.0139)	-.0080* (.0042)
<u>Regression Specification D</u>				
Activated x Border County	-.1887** (.0282)	-.0067 (.0569)	.0410** (.0042)	-.0013 (.0112)
Activated x Not Border County	-.0312 (.0170)	.0028 (.0114)	-.0134** (.0054)	-.0006 (.0047)
<u>Regression Specification E</u>				
Activated x First Year	-.0778** (.0291)	.0088 (.0313)	-.0213** (.0063)	-.0016 (.0067)
Activated x Second Year	-.0331* (.0190)	-.0097 (.0121)	-.0109** (.0053)	-.0011 (.0050)
Activated x Third Year	-.0021 (.0144)	.0053 (.0104)	-.0060 (.0048)	.0019 (.0045)
Activated x Fourth Year	-.0031 (.0144)	.0040 (.0129)	-.0197 (.0067)	-.0007 (.0060)
Measure of Secure Communities?	Activated	Activated	Persons in ICE Custody x Activated	Persons in ICE Custody x Activated
Includes County-level Trends?	N	Y	N	Y

Notes: OLS regression estimates, ** $p < 0.05$, * $p < 0.1$. 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.

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