Does Immigration Enforcement Reduce Crime?
Evidence from “Secure Communities”

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Abstract. Does immigration enforcement actually reduce crime? Surprisingly, little evidence exists either way—despite the fact that deporting noncitizens who commit crimes has been a central feature of American immigration law since the early twentieth century. We capitalize on a natural policy experiment to address the question and, in the process, provide the first empirical analysis of the most important deportation initiative to be rolled out in decades. The policy initiative we study is “Secure Communities,” a program designed to enable the federal government to check the immigration status of every person arrested for a crime by local police. Before this program, the government checked the immigration status of only a small fraction of arrestees. Since its launch, the program has led to over a quarter of a million detentions. We exploit the slow rollout of the program across more than 3,000 U.S. counties to obtain differences-in-differences estimates of the impact of Secure Communities on local crime rates. We also use rich data on the number of immigrants detained under the program in each county and month—data obtained from the federal government through extensive FOIA requests—to estimate the elasticity of crime with respect to incapacitated immigrants. Our results show that Secure Communities led to no meaningful reductions in the FBI index crime rate. Nor has it reduced rates of violent crime—homicide, rape, robbery, or aggravated assault. This evidence shows that the program has not served its central objective of making communities safer.

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Introduction

The belief that immigrants – or at least certain immigrants – commit more crime than native-born people is persistent and widely held. Popular concern with immigrant criminality has risen with each succeeding wave of immigrant groups: first Irish and Chinese immigrants, then Italians and others from southern and eastern Europe, and today Mexicans and others from Latin America. This belief has translated directly into public policy and has made crime control a leading objective of immigration regulation.

Deportation, the physical removal of an immigrant from the country, has long been a key tool in pursuing this objective. Well before the United States became a nation, other states regularly used "transportation" – the deportation of large numbers of criminals – as a means of crime control. Early efforts at immigration restriction in America built on these ideas, excluding those who were serving criminal sentences elsewhere, as well as anyone whose sentence was commuted in exchange for passage to the United States.[1] Moreover, in recent decades immigration policy has been reshaped as an instrument of criminal justice policy on a breathtaking scale. New grounds of criminal deportation have proliferated; immigration status violations have increasingly been used to remove suspected criminals who would be difficult or impossible to prosecute under criminal law; and the immigration and criminal enforcement bureaucracies have become deeply entwined (Cox and Miles 2013; Sklansky 2012).

The belief in immigrant criminality that produced these policies has persisted even though academic research generally finds that immigrants are no more prone (and may be less prone) to engage in crime than the native born. Yet given the longstanding focus on

[1] See section 3 of the Page Act, March 3, 1875, 18 Stat. 477.

whether immigration increases crime, it is remarkable that the policy at the heart of the immigration-crime debate—the detention and deportation of immigrants who commit crimes—has received almost no attention by empiricists. A central goal of the immigration system is to screen out undesirable immigrants and shape the behavior of immigrants living in the host country (Cox and Posner 2007). Thus, even if immigrants have lower *average* offending rates than citizens, the selectivity of the immigrant screening system might produce meaningful crime reductions. Moreover, this screening system today represents a massive social intervention. Policies requiring the detention and deportation of noncitizens who commit crimes have grown explosively in recent years. Yet, little is known about whether detention and deportation of these immigrants makes communities safer.

This Article fills this gaping hole by examining a policy innovation specifically intended to increase the detention and deportation of immigrants who commit crimes, whom we will refer to as "immigrant offenders."[2] The program, called Secure Communities, has a simple goal: to ensure that every person arrested for a crime by local law enforcement anywhere in the country is screened by the federal government for immigration violations. Before this program, a criminal arrestee's immigration status was rarely investigated because it was a labor-intensive task, typically requiring the physical presence of a federal officer in a local lock-up. Secure Communities revolutionized the screening process by relying instead on fingerprint information about arrestees that was already being collected by local police and forwarded electronically to

[2] Given the relatively common confusion about the civil nature of most immigration law, we should emphasize that we use the term "immigrant offenders" throughout this paper to refer to noncitizens who commit crimes, not noncitizens who are simply in violation of immigration law. In popular discourse immigrant offenders are often referred to as "criminal aliens."

the FBI. Under Secure Communities, these fingerprints are now also forwarded to the Department of Homeland Security (DHS). This relay is electronic, instantaneous, and routine. Local police can avoid submitting fingerprints to DHS only by refusing to conduct a criminal background check on an arrestee, which would be contrary to standard arrest practices.

DHS checks the fingerprints against its database of foreign-born persons. If it identifies a putatively deportable noncitizen whom it wishes to remove, the enforcement arm of DHS, known as Immigration and Customs Enforcement (ICE), notifies the local police and takes custody of the noncitizen when she is released from criminal confinement. ICE typically retains custody of the person until the completion of removal proceedings, which leads, in the vast majority of cases, to the person's deportation. Secure Communities thus increases the probability that immigrant offenders who would otherwise be released are subject to immediate federal detention and ultimate deportation. In its first four years, ICE has detained over 250,000 immigrants through the program and has deported over 200,000. As a percentage of the number of noncitizens in the United States, this figure exceeds the percentage of the entire U.S. population currently incarcerated in the criminal justice system.

Technological constraints prevented the simultaneous nationwide implementation of Secure Communities. Instead, DHS rolled out the program on a county-by-county basis over four years. This staggered implementation created variation across counties in the rates at which immigrant offenders were detained. We argue that the timing of Secure Communities activations was exogenous to county crime rates and provides an ideal experiment to identify the impact on crime of detaining and deporting noncitizen offenders. It also allows us to evaluate whether Secure Communities met its objective of reducing violent crime – a critical question for a program that represents the largest effort in U.S. history to integrate local police into federal immigration enforcement.

We estimate the impact of Secure Communities on crime rates using a monthly panel of nearly 3,000 counties over nine years. Exploiting the quasi-experiment created by the staggered timing in the program's implementation, we develop differences-in-differences estimates of the impact of the program on crime. In addition, we use unique data obtained from DHS through a series of Freedom of Information Act requests – data on the number of persons detained by month and county under the program – to estimate directly the elasticity of crime with respect to immigrant detention.

Our results indicate that Secure Communities has had no impact on the overall rate of crime. This result is especially important given the large number of people detained under the program, as well as the fact that the program is designed to prioritize the removal of immigrants with offending rates much higher than that of the average immigrant. The rollout of the program coincided with a period of declining crime rates in counties with high immigrant concentrations, and accordingly, the estimates are sensitive to controlling for county-specific trends. When accounting for county-level trends, the estimates indicate that Secure Communities had no effect on the FBI index crime rate, and this zero estimate is fairly precise. Nor did the program reduce rates of violent crimes—of murder, rape, arson, or aggravated assault. Some specifications suggest modest declines in the rates of two property crimes—burglary and motor vehicle theft—but even these results imply an elasticity of crime with respect to immigrant detention that is far smaller than existing estimates of the elasticity of crime with respect to prison populations.

The Article proceeds in five parts. Section 1 reviews the existing empirical literature on immigration and crime, and Section 2 develops a simple theoretical framework for understanding how immigration enforcement may influence crime. Section 3 describes the details of the policy intervention, explaining both the operation and rollout of Secure Communities. Section 4 explains the empirical strategy and data, and Section 5 presents the results.

1. Literature on Immigration and Crime

Crime and immigration have long been linked by two corollary questions. First, does immigration increase crime? Second, can detention and deportation reduce crime? There is substantial prior work on the first question yet very little on the second.

Scholars have examined the first question using both micro-data and geographic aggregates. Individual-level studies relate self-reports of criminal activity to immigrant status. Butcher and Piehl (1998b) examined the 1980 wave of the National Longitudinal Survey of Youth and found immigrant youth were less likely to report offending. This pattern was evident in unconditioned comparisons and robust to controlling for a large set of youth and family characteristics. Sampson, Morenoff, and Raudenbush (2005) surveyed over 3,000 individuals in Chicago neighborhoods and found that violence rose with a generation's distance from immigration. The odds of violence for a first-generation immigrant was three-fourths that of a second-generation immigrant and half that of a third-generation immigrant. Desmond and Kubrin (2009) examined the National

Longitudinal Survey of Adolescent Health, a survey that contains a rich set of individual and community characteristics. They estimated that youths living in neighborhoods with more foreign-born residents have lower risks for violence, and this reduction in risk is greater for foreign-born youths in these communities.

Other studies with individual-level data infer offending rates from institutionalization rates. Butcher and Piehl (1998, 2007) examined multiple decennial censuses and found that even without conditioning on age or education, immigrant institutionalization rates were less than half those of the native born. Moreover, this gap widened over time, and deportation does not appear to account for it (Butcher and Piehl 2000). To the contrary, immigration enforcement policies may cause an over-representation of immigrants in jail populations. Hagan and Palloni (1999) found that in El Paso, Texas, and San Diego, California, immigrants were more likely to be detained before trial, a pattern that Hagan and Palloni speculated may be due to detainer requests (also known as immigration holds) placed on immigrants by federal authorities. They observed that defendants detained pre-trial faced higher rates of conviction and incarceration, and after adjusting for the effects of pre-trial detention on adjudication and sentencing, Hagan and Palloni estimated that immigrants offended less often than the native born. Similarly, Butcher and Piehl (2007) concluded that the self-selection of immigrants who either have lower criminal propensities or greater responsiveness to punishments than the native born explained their lower institutionalization rates.

The second approach to studying the effect of immigration on crime focuses on geographic aggregates rather than individuals. This approach asks whether crime rates differ in times when, or places where, there are higher concentrations of immigrants.

Some studies make cross-sectional comparisons either across cities or across neighborhoods within a city. Reid, Weiss, and Adelman (2005) examined a cross-section of metropolitan areas in the year 2000. They found that after controlling for the characteristics of each area, the share of immigrants in the population had either a negative correlation or no correlation with the rates of four categories of crimes. Graif and Sampson (2009) applied a variety of regression techniques to a cross-section of Chicago census tracts and similarly found that, after controlling for other neighborhood characteristics, immigrant concentration had either no relationship or a negative relationship to homicide rates.

Cross-section estimates may be biased upwards because immigrants tend to locate in cities with higher rates of poverty and crime. To combat this selection bias, some authors have turned to panel data so that fixed effects can be used to control for time-invariant differences across locations. Butcher and Piehl (1998) analyzed a panel of metropolitan areas over the 1980s. They first found a positive association between the inflow of immigrants and a city's crime rate, consistent with larger cities having both more crime and more immigrants. But once they conditioned on a city's demographics and labor market conditions, this association disappeared. Similarly, Ousey and Kubrin (2009) examined a sample of large cities at three points in time between 1980 and 2000, and they found that an index of immigrant concentration correlated negatively with a city's crime rate. MacDonald, Hipp, and Gill (2013) instrumented for immigrant concentration in Los Angeles neighborhoods in 2000 with its level in 1990. They found that higher concentrations of immigrants led to substantial reductions in crime. Spenkuch (2014) took a similar approach but reached somewhat different conclusions. Analyzing counties at three decennial intervals, his ordinary least squares estimates imply that a 10% increase in a county's share of immigrants has no effect on violent crime but raises property crime by 1.2%. When the share of immigrants is instrumented with its own lag, the point estimates are stable but lose statistical significance. Spenkuch concludes that the social benefits of immigration outweigh the costs of these additional crimes.

Other panel studies considered immigration shocks, focusing on periods of rapid immigration that permit before-after comparisons within a location. Martinez, Stovell, and Lee (2010) analyzed homicides in census tracks in San Diego between 1980 and 2000, a period of rapid immigration. They found that declines in homicide accompanied increases in the share of the foreign-born population. Bell, Machin, and Fasani (2010) looked at two waves of immigration since the 1990s in the United Kingdom. They reported that the rate of property crimes increased in local areas experiencing the largest influx of immigrants during the first episode, but they observed no change in crime rates during the second episode. Chalfin (2014) observed that deviations in rainfall from its long-term mean in Mexican states raised the likelihood of migration to the U.S. Using these rainfall shocks as instruments, Chalfin estimated that Mexican immigration had no effect on rates of violent or property crime in major U.S. metropolitan areas.

Existing work largely ignores the corollary question whether immigration policy can be an instrument of crime control. This is puzzling, because a central goal of immigration law has long been to screen out undesirable immigrants and to shape the behavior of immigrants once they arrive in the host country. The explosive growth of these screening policies during the twentieth century—particularly of policies directly targeting immigrants who commit crimes—raise important questions about the effect of immigration policy on crime.

The first is whether granting immigrants legal status affects their offending behavior. Mastrobouni and Pinotti (2012) examined two exogenous variations in the legal status of Italian immigrants: a 2006 clemency and the 2007 enlargement in the European Union. They estimated that legal immigration status halved the odds of re-incarceration for a criminal offense, and this accounted for one- to two-thirds of the gap in offending between immigrants and the native-born. Two papers study the 1986 immigration reform that granted legal resident status to some immigrants and imposed penalties on employers that hire immigrants without legal status. Baker (2011) examined a nationwide panel of counties and estimated that for every 1% of a county's population that received legal status, crime rates, especially property crime rates, fell by 1- 4%, a drop he attributes to increased labor market access by immigrants who gained legal status. By contrast, Freedman, Owens, and Bohn (2013) found that in San Antonio, Texas, felony charges, especially for economically motivated crimes, filed against Hispanics rose by nearly 60% after the 1986 reforms. Because the neighborhood characteristics of these defendants suggested that they were likely immigrants who did not gain legal status as part of the reforms, the authors attributed the increase in crime to the presence of new barriers to work imposed by the employer sanctions regime that accompanied the legalization program.[3]

[3] Freedman, Owens, and Bohn also showed that following the legal change, felony charges against Hispanics were less likely to end in conviction, which implied that policing behavior also responded to the legal change. But they conclude that even after adjusting for altered policing practices, barriers to legal work may increase immigrant offending

A second question—in some sense the opposite of the first—is whether detaining and deporting immigrant offenders can reduce crime. Over the past two decades, efforts to deport noncitizen offenders have increased and have in many ways effectively integrated the immigration and criminal justice systems. In particular, the practice of deporting noncitizens who commit crimes, unheard of in United States law prior to 1907, has become perhaps the most important tool for screening out immigrants (Cox and Posner 2007). These developments have led legal scholars such as Sklansky (2012) to see the immigration and criminal justice systems today as effectively a single governance system focused on deporting noncitizen offenders and worthy of its own name, "crimmigration."

We are aware of only one prior article attempting to study the effect of deportation on crime, by Stowell et al (2013).[4] It examined deportations in several metropolitan areas between 1994 and 2000 and concluded they had no relationship to the incidence of violent crime. Yet Stowell et al. did not specifically measure the deportation of immigrant offenders, and the vast majority of deportees during that period had no criminal convictions. It also did not consider the detention of immigrants prior to deportation, even though periods of detention may be lengthy. Moreover, Stowell et al.'s measure of deportations was at the level of an ICE administrative district, a unit that

[4] In work subsequent to ours, Chalfin et al. (2014) also investigate the impact of Secure Communities. Their analysis has several differences from that conducted here. They measure only a single cross-section of deportations measured on the last date of their sample. By contrast, we observe both detentions and deportations in each month since the program's launch. Also, DHS rolled-out Secure Communities on a county-by-county basis, but Chalfin et al.'s observations of crime data are at the city level, creating a mismatch between their outcome and treatment variables and causing the exclusion of a substantial number of jurisdictions affected by Secure Communities. As we describe blow, our data is structured as county-month observations.

typically encompasses multiple states, and this mismatch between the policy and the outcome introduces the risk of attenuation bias.[5]

2. Theoretical Framework

An obstacle in using geographic aggregates to examine the relationship between immigration and crime is that the offending rates of immigrants and the native born are not directly observed. Instead, the observed crime rate is a weighted average of the offending rates of immigrants and the native born. Specifically, letting β_k be population group *k*'s offending rate, permitting α_k be the group's share of the population, and allowing index *k* refer either to *i* (immigrants) or *n* (native born), the overall crime rate can be written as:

$$\beta = \alpha_i\beta_i + \alpha_n\beta_n \,. \qquad (1)$$

The primary intervention of Secure Communities is to detain and deport immigrant arrestees whom local law enforcement would otherwise release back into the community. This has two potential consequences for the observed offending rate. First, the program raises the probability of an arrestee's immediate and continued confinement. It increases the likelihood that an immigrant who is arrested for a local offense is placed

[5] A strand of the literature on cooperation with police has examined how immigrants perceive law enforcement. This work has focused almost entirely on New York and has tended to examine general policing rather than immigration enforcement specifically. Kirk et al. (2011) surveyed New York City residents and found that persons residing in neighborhoods with high concentrations of the foreign-born were less cynical about the law and more willing to cooperate with the police. Davies and Fagan (2012) analyzed crime and policing data for New York and found that neighborhoods with higher concentrations of immigrants were subjected to more intense police activity, as measured by arrests and street stops, even though these communities had lower rates of total and violent crime. Tyler, Schulhofer, and Huq (2010) showed that the risk of deportation reduced feelings of trust and police legitimacy among Muslims in New York.

in federal custody rather than released while her criminal case is adjudicated.[6] Once an immigrant is detained as a result of Secure Communities, federal custody will often continue until the completion of deportation proceedings. Second, Secure Communities raises the probability that the noncitizen will be deported following the completion of her criminal adjudication and any criminal sentence imposed. Removal from the country constitutes continuing incapacitation because reentry is difficult, both legally and practically. Immigration law currently prohibits deportees from seeking re-admission for at least ten years, and offenders convicted of certain offenses are barred for life.[7] While border enforcement is imperfect, avoiding detection at the border is costly, and re-entering without permission is a felony punishable by a term of incarceration of up to 20 years for those deported after being convicted of certain crimes.[8]

To capture the incapacitative effects of Secure Communities, let S represent the number of immigrants detained under the program. The immigrants' share of the population is a function of S, or $\alpha_i = \alpha_i(S)$. The removal of persons through detention and deportation implies a mechanical reduction in the size of immigrant population, or $\partial\alpha_i/\partial S < 0$.[9]

Secure Communities also alters the expected sanction for crimes committed by immigrants. The increased risk of immediate and continuing post-arrest confinement is surely seen by arrestees as a sanction, as are both the eventual removal from the country

[6] It also increases the likelihood that the immigrant will be denied bond in her criminal proceeding, as some local courts routinely deny bond to criminal defendants against whom immigration detainers have been lodged.

[7] 8 U.S.C. § 1182(a)(9).

[8] 8 U.S.C. § 1326.

[9] We do not directly consider the possibility of replacement because the focus of our empirical analysis is on relatively short-term effects.

and the long-term bar on re-entry. Importantly, Secure Communities supplies these additional sanctions without undercutting the underlying criminal sanction. Immigration law prohibits federal authorities from deporting an incarcerated noncitizen prior to the completion of his criminal sentence.[10] Deportation is thus imposed in addition to, rather than in lieu of, any criminal sentence imposed on the arrestee.

In the classic deterrence model of Becker (1968), an increase in the expected sanction should dissuade offending. Accordingly, it is assumed that the offending behavior of immigrants depends on detention, or $\beta_i = \beta_i(S)$, and that immigrant offending declines with the number of noncitizens detained under Secure Communities, or $\partial\beta_i/\partial S < 0$. Lastly, it is assumed that Secure Communities has no effect on β_n, the offending rate of citizens, because citizens cannot legally be deported, and thus the policy should have no incentive or incapacitating effects on citizens.[11]

The effect of increasing the number of incapacitated immigrants on the overall crime rate can be seen by differentiating equation (1) with respect to S. Using the definition $\alpha_i + \alpha_n = 1$, this can be written:

$$\partial\beta/\partial S = \alpha_i\partial\beta_i/\partial S + (\beta_i - \beta_n)\partial\alpha_i/\partial S. \quad (2)$$

[10] Subject to limited exceptions, "the Attorney General may not remove an alien who is sentenced to imprisonment until the alien is released from imprisonment." 8 U.S.C. § 1231(a)(4)(A). This does not mean, of course, that the availability of deportation *never* affects the criminal sanction received by a noncitizen. First, some federal criminal enforcement initiatives use immigration violations to remove gang members. Often this occurs in situations where there are obstacles to criminal prosecution (Chacón 2007; Sklansky 2012), but there might be instances where prosecutors would bring charges anyway were deportation not an option. Second, in practice there are almost certainly situations where a person charged with a crime receives a lighter sentence, or has her changes dropped altogether, when local officials know that she will be handed over to ICE following any criminal sentence.

[11] While enforcement errors do on occasion lead to the unlawful detention (or even deportation) of citizens, there is no evidence that such errors are common enough to produce a meaningful incentive or incapacitation effect among citizens.

The impact of the policy is the sum of two terms, the first of which represents the effect on the offending behavior of immigrants and the second is its effect on the size of the immigrant population. The first term on the right-hand side is negative because of the assumption of a deterrent response to the heightened sanction of immediate detention and eventual deportation.

The second term on the right-hand side consists of the change in the size of the immigrant population weighted by the difference in offending rates of immigrants and the native born. Before considering the sign of this term, it is worth noting that it may be zero in two circumstances. Trivially, it is zero when the policy intervention is too small to affect the size of the immigrant population, or $\partial\alpha_i/\partial S = 0$. We rule out this possibility. Section 3 below describes how Secure Communities has induced substantial movement in the share of the immigrant population, especially in counties with high concentrations of immigrants. Next, the term may be zero when immigrants and the native born offend at similar rates, or $\beta_i = \beta_n$.

Setting aside these two possibilities of zero values, the sign of the second-term on the right-hand side of equation (2) depends on the relative sizes of β_i and β_n. The persistent popular belief is that immigrants offend at higher rates than the native born, or $\beta_i > \beta_n$. If this were true, the effect of Secure Communities on the overall crime rate would be unambiguously negative. The policy would reduce observed crime rates through both a behavioral effect (deterrence) and an incapacitative effect (removing higher offending individuals from the population).

If instead the majority of the academic evidence is correct that immigrants offend at lower rates than the native born, or $\beta_i < \beta_n$, then the second term on the right-hand side of equation (2) would be positive. This term would represent a counterintuitive consequence of Secure Communities: that it would contribute to a higher observed crime rate. It would do so by removing persons who offend less frequently (immigrants) and would leave a population comprised of proportionately more high-offending persons (the native born). Whether the overall consequence of Secure Communities would be to increase the observed crime rate would depend on whether this compositional effect exceeded the deterrent effect.

The stylized framework above can be extended in two ways. First, the model assumes offending rates are uniform within groups. In reality, of course, individuals differ considerably in their propensity to commit crimes. If ICE prioritized the most serious immigrant offenders, the marginal immigrant detained under Secure Communities would have a higher offending frequency than the average immigrant. This is likely because the program targets only those who have been arrested by local criminal authorities. It implies that estimates of immigrant offending from Secure Communities may be biased upwards from their average rate.

Second, the framework assumes that there is just one type of crime, but there are many crimes of varying levels of seriousness. Serious offenders are more likely to be confined prior to adjudication and typically receive longer sentences than more minor offenders. This practice influences the types of immigrant offenders who are likely detained under Secure Communities and who are likely deterred by it. A person charged with a serious felony, such as murder, is likely to be detained by state authorities prior to her trial and, if convicted, to receive an extremely long prison sentence. She will face deportation only at the completion of the prison term. In such cases, Secure

Communities has no marginal incapacitative impact in the short term, and given the discounting of future utility, it is also unlikely to have much deterrent impact. By contrast, a person convicted of a less serious offense, such as simple drug possession, might typically be sentenced to time served during the pendency of the criminal proceeding. Before Secure Communities, time served would be the only sanction, but after Secure Communities, ICE may elect to take this defendant into federal custody and begin deportation proceedings. Thus, Secure Communities' effect on deterrence and incapacitation may be greatest when the offense of conviction is less serious.

Given the relatively short period during which Secure Communities has been in operation, violent offenders are unlikely to comprise a large fraction of immigrants detained under the program – even if DHS wishes to target such offenders. As shown below, the data demonstrate that the majority of the immigrants taken into federal custody as a result of Secure Communities are not violent offenders. Yet, the program may still reduce violent crime rates if offenders are not highly specialized, as others have found. For example, in a study of state prison populations, Kuziemko and Levitt (2003) found that incarcerating a drug offender reduced violent and property crime by about as much as confining other types of offenders.

3. The Secure Communities Program

3.A. Operation of the Program

Federal immigration law makes many crimes grounds for deportation.[12] Until recently, however, the task of identifying noncitizens among the pool of persons arrested

[12] 8 U.S.C. §§ 1182(a)(2), 1227(a)(2).

by state and local law enforcement authorities was costly. Before Secure Communities, immigrant arrestees were identified principally through individual inmate interviews in local jails and prisons. These interviews were conducted by federal officials pursuant to the Criminal Alien Program, and by deputized local law enforcement officials under so called "287(g)" agreements.[13] 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 (Cox and Miles 2013).

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

[13] 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.

U.S. in violation of immigration law, such as persons who were previously deported or overstayed their visas; (2) noncitizens who are lawfully in the United States but who might become deportable were they to be convicted of the crime for which they have been arrested; (3) citizens who naturalized at some date after their fingerprints were included in the database.

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

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

3.B. Rollout of Secure Communities

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

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

The federal government determined the sequence of rollout. It prohibited local governments from formally opting out of Secure Communities even though elected officials in some localities wished not to participate.[14] Moreover, the program's structure

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

made informal noncompliance with the screening system practically impossible. Once Secure Communities is activated in a county, local authorities have no way to share the fingerprints of arrestees with the FBI but not with DHS. The only way a local law enforcement agency could prevent DHS's immigration check from taking place would be to stop fingerprinting arrestees altogether, and we are aware of no local agency that has done so.[15]

Cox and Miles (2013) explored in detail the determinants of Secure Communities activation using proportional hazard analysis. They found that, while the timing of activation was not wholly random, it appeared to mirror federal enforcement priorities for *immigration* generally rather than for crime control. The strongest correlates of an early activation were a county's location on the southern border and the fraction of the county's population that was Hispanic. Although Hispanic and foreign-born populations correlate closely with each other, Cox and Miles (2013) found that, after controlling for other factors, only the Hispanic population fraction had a statistically significant relationship to activation timing. The federal government's decision to commence the program on the southern border, a flashpoint of popular debate over immigration policy, suggests that despite the allusion to public safety in the program's title, the federal government saw immigration regulation rather than crime control as Secure Communities' main purpose.

Figure 2 provides a visual summary of these findings. It shows the shares of various population groups that resided in jurisdictions covered by Secure Communities at

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

each calendar date since the program's launch. The solid line shows that Secure Communities expanded its coverage of the total population in a steady, at times linear, fashion. By contrast, the rapid activation of counties on the border resulted in over 90% of the population in counties on the southern border being covered by Secure Communities within its first year. The dotted line shows that the Hispanic population was covered more quickly than the overall population but at a substantially slower pace than counties directly on the border. The foreign-born population received coverage less rapidly than the Hispanic population but more quickly than the general population. The correlation of Hispanic and foreign-born populations with the speed of activation is far less dramatic than that of border counties. Although these population groups are associated with earlier activation, large portions of the Hispanic and foreign-born populations were not covered by Secure Communities until late in its rollout.

Cox and Miles (2013) also found that, after controlling for urban density, the timing of activation did not correlate with crime rates or the level of local policing. When measures of urban density were excluded from the analysis, higher crime rates implied only a modest increase in the speed of activation. Even in counties with very large Hispanic populations, higher crime rates implied only small movements in the timing of activation. Thus, the pattern of activation does not have a close relationship to the primary outcome of interest here, crime rates. Moreover, even if the federal government had activated sooner the counties where it expected the largest crime reductions, such targeting should bias our differences-in-differences estimates towards finding a crime-reducing effect, which makes our findings – of almost no meaningful reductions in crime – more compelling.

3.C. Program Intensity

The rates at which ICE detains immigrants under Secure Communities varies across counties and within a county over time. A simple classification of counties as "activated" or "not activated" under the program therefore measures with error the intensity of the program's intervention, and difference-in-difference estimates of the program's effect will suffer from attenuation bias. We overcome this problem by measuring the magnitude of the program's primary intervention directly. Through a series of Freedom of Information Act requests to the Department of Homeland Security, we obtained tallies of the persons detained by ICE under Secure Communities in each county and month.[16] The cumulative number of immigrants taken into federal custody under Secure Communities in each county by each month is our measure of the program's intensity.

Figure 3 highlights the importance of using this direct measure of program intensity. It shows the cumulative number of activated counties in bars corresponding to the left scale,[17] and the cumulative total number of persons detained under Secure Communities in lines corresponding to the right scale. The figure illustrates two

[16] We also obtained data from DHS on other potential dosage measures, including the monthly number of persons deported under Secure Communities in each county. Most deportees identified through Secure Communities are detained by the federal government during the pendency of their deportation proceedings, and a very high percentage of those who are detained are ultimately deported. For these reasons, the date of federal detention, rather than the date of deportation, is a more accurate measure of the program intervention. Moreover, the deportation counts are not necessarily a good (lagged) proxy for the detention counts, because the period of pre-removal federal detention varies widely from case to case: some noncitizens are detained only a few days because they agree to stipulated orders of removal and forgo deportation proceedings entirely, while others are detained for months or years during the pendency of their deportation proceedings in immigration court. That said, all of the results we find using federal detention as a dosage measure hold, largely unchanged, if the number of deportations is instead used.

[17] The number of activated counties whose share of foreign-born population was in the top quartile are indicated by darker (shorter) bars.

important differences between this direct measure of program intensity and a binary measure of program activation. First, the number of detentions grows in the months after activation. The final eight months of 2012 saw virtually no change in the number of activated counties, as all but a few locations had been activated, but the number of immigrants detained continued to increase steadily. Second, there is significant heterogeneity in the use of detention across counties. The dotted line indicates the total number of detentions occurring in counties with foreign-born population shares in the 75th percentile. The dotted line for these counties lies nearly on top of that for the nationwide total; that is, nearly all detentions under Secure Communities occurred in counties with the highest proportions of foreign-born population.

Figure 3 also shows the overall magnitude of Secure Communities. Between its launch and the end of 2012, Secure Communities led to the detention of just over 250,000 immigrants nationwide. This is more people than are currently in federal prison, which today incarcerates about 216,000 inmates (Bureau of Prisons 2014) The comparison to the entire U.S. criminal justice system is also remarkable: Secure Communities over this period detained 1.13% of all noncitizens in the U.S. (Acosta 2014),[18] while approximately .71% of the U.S. population is currently incarcerated in prison and jails (Glaze and Herberman 2013)—and the U.S. incarceration rate is one of the highest in the world (Walmsley 2013). Secure Communities is an enormous social intervention.

[18] Secure Community detentions are roughly 0.63% of the foreign-born population in the U.S. (Greico et al. 2012). Because the foreign born population includes naturalized citizens who are no longer at risk of deportation, noncitizen population is a more appropriate denominator—though this is a measure that the Census Bureau calculates only at the state and national level,.

4. Data and Empirical Strategy

The dataset is a panel of monthly, county-level observations from 2004 to 2012. The observation period terminates with the final year of available crime data, and the start date was chosen to balance roughly the number of years before and after the launch of Secure Communities in late 2008. Each county-month observation includes three types of information: (1) crime data, (2) demographic and other county-level characteristics, and (3) measures of the intensity of the program's treatment.

Crime data are the index offenses from the Federal Bureau of Investigation's Uniform Crime Reports (UCR) (FBI various years). The index consists of violent crimes (murder, rape, robbery, aggravated assault) and property crimes (burglary, larceny, and motor vehicle theft). The FBI was also the source for annual counts of the number of police (sworn officers) by county. Maltz and Targonski (2002) and Maltz (2006) documented the presence of missing values in the county-level monthly UCR and the difficulties of imputing these missing values. A relatively simple imputation or smoothing procedure was used here. When data were reported only quarterly, biannually, or annually, the reported figure was averaged over the relevant time period and that average value was assigned to each month. Missing values necessitated this reallocation for only about 7% of the observations in the sample, and as shown below, this imputation does not affect the conclusions.

Demographic characteristics of each county, as well as information on each county's median income, were obtained from the Census Bureau. Table 1 reports summary statistics on these variables. It is worth noting that the Census Bureau does not determine the number of immigrants or noncitizens in each county. It does determine for each county the number of foreign-born and Hispanic persons, and these groups are closely correlated with immigrant status. Income measures, foreign-birth, and household composition were gathered from decennial censuses and linearly interpolated. Table 1 reports the summary statistics on these characteristics.

Secure Communities program data was obtained from DHS. The date of each county's activation under Secure Communities was publicized by the agency. Through several Freedom of Information Act (FOIA) requests, the authors obtained from DHS tallies of the immigrants detained under Secure Communities during each month in each county since the program's launch. ICE classifies detainees into four groups according to their criminal histories. Level 1 includes noncitizens convicted of "aggravated felonies," an enumerated set of offenses defined in the Immigration and Nationality Act, or convicted of two or more felonies. The category of aggravated felonies is complex and not easily summarized, but it includes violent felonies like murder and rape, as well as some property crimes and a wide swath of drug offenses.[19] All of the violent index crimes—murder, rape, arson, and aggravated assault—qualify as aggravated felonies. Level 2 includes noncitizens convicted of any felony that is not an "aggravated felony," or convicted of three or more misdemeanors. Level 3 includes noncitizens convicted of one or two misdemeanors. ICE's final category, "noncriminal immigration violations" includes noncitizens who have no recorded criminal conviction. These noncitizens have only civil violations of immigration law, such as overstaying a visa or entering the country without inspection (ICE 2013).

[19] See INA § 101(a)(43). For example, theft and burglary are treated as aggravated felonies if the offense is one "for which the term of imprisonment is at least one year," but not otherwise.

Figure 4 shows the proportions of all detainees in these categories by month since the program's launch. Immigrants with no convictions compose nearly a third of all Secure Communities detainees. Those in the lower criminal categories make up another 40%. The most severe offenders, who are categorized as L1, account for only 28% of detainees. Figure 4 additionally demonstrates that these percentages have remained relatively stable over time. The comparatively low share of detainees with aggravated felony or multiple felony convictions does not necessarily contradict ICE's repeated public statements that it prioritizes the most serious offenders within Secure Communities. Rather, it likely reflects two factors: the underlying pool of arrestees, as well as the requirement that all immigrant offenders serve out their full state criminal sentences before the federal government takes custody of them pursuant to deportation proceedings. This requirement delays ICE's apprehension of the most serious offenders since, in expectation, those arrestees will serve the longest sentences.

To control for earlier forms of federal-local cooperation, we also assembled by hand the dates and locations of "287(g)" agreements by reading the actual agreements on ICE's webpage. ICE made these agreements with local police and sheriff's departments and with states' prison systems. We created a binary variable for the presence of any active 287(g) agreement in a county that takes the value one when any law enforcement agency in the county or at the state level was operative and is zero otherwise.[20]

[20] In the regression analysis, the coefficient on this variable is a differences-in-differences estimate of the impact of a 287(g) agreement. These estimates are not reported in order to conserve space, but they suggest that 287(g) agreements had at best modest crime-reducing effect. For example, in the regression reported in Specification A in column (1) of Table 2, the estimate for a 287(g) agreement was -.0322 (s.e.=.0144), and in column (2) with county-level trends, it was -.0136 (s.e.=.0096).

The core sample consisted of 84 monthly observations of 2,985 counties,—a sample size of 336,204. But the tallies of police employment were not available for every county in each year, and controlling for police employment seemed particularly important because the program piggybacks on arrests by local police. Therefore, the baseline sample consisted of 302,388 observations in an unbalanced panel.

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

$$lnC_{it} = g(Activate_{it})\,\delta + X_{it}\,\beta + \alpha_i + \alpha_t + \varepsilon_{it}, \qquad (3)$$

where lnC_{it} is the log of the crime rate in county i at calendar month t.[21] When a county experienced no offenses in a particular month, the dependent variable was assigned the value of one, and these observation were permitted to have a separate intercept.

The term $Activate_{it}$ is a variable representing whether Secure Communities is active in county i on date t. Several different functional forms of g(•) are used to capture the activation of Secure Communities. The simplest is an indicator variable that takes the value one when a county has been activated and zero otherwise, and in this case, the coefficient δ is the differences-in-differences estimate of the program's impact. In other specifications, activation is expressed with two indicators or two differences-in-differences estimates in order to isolate locations where immigrant populations are largest and where any impacts of Secure Communities should be strongest. In other regressions, the indicator variables are replaced with direct measures of the intensity of Secure Communities' intervention. As described above, these are the rates of detention in a

[21] As is standard, a crime's rate is defined as the number of offenses per 100,000 people.

county, which are expressed as the log of cumulative number of people from a county detained under the program, per 100,000 persons.

The vector X_{it} contains a set of county- and date-varying control variables that are commonly included in studies of crime. These are shown in Table 1, with the exception that police employment, 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. As discussed below, an extension of equation (3) includes county-specific trends. The term ε_{it} captures the error. The regressions are weighted by a county's population, and the standard errors are clustered by county.

5. Results

5.A. Estimates for Aggregate Crime Rates

Panel A of Table 2 reports the differences-in-differences estimates for the total index crime rate. Specification A shows a negative and statistically significant coefficient that implies a county's activation under Secure Communities is associated with a statistically significant decline of 4% in the crime rate. Specification B decomposes the basic estimate into two: 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). It shows that almost all of the 4% decline observed in the baseline estimate is due to reductions in the counties with high concentrations of the foreign born. The other counties experience no drops in crime following Secure Communities activation; in fact, the estimate for these counties is positively signed. Specification C makes a similar comparison for counties on and not on the southern border. It shows a very large decline in border counties, about 21%, following activation, compared to a statistically insignificant effect of 3% in non-border counties. In sum, the basic differences-in-differences estimates suggest that the activation of Secure Communities in counties with high concentrations of foreign-born persons was associated with sizable declines in crime.

The period of Secure Communities' activation, 2008-2012, was a time of falling crime rates nationally. This raises the possibility that places chosen for earlier activation were locations where crime was falling fastest.[22] Column (2) presents regressions testing this possibility by including county-specific trends. These specifications eliminate variation in crime rates caused by factors that vary linearly over time and that are specific to individual counties. Identification of the impact of Secure Communities in these equations comes from within-county variation after netting out county-specific trends. The county trends have a dramatic effect on the estimates. Their inclusion drives all the differences-in-differences estimates to zero, and they lose statistical significance. The baseline estimate, for example, falls from -.0400 to .0025. Column (3) verifies this pattern by specifying the continuous variables on both the left- and right-hand side in first-differences. This is akin to county-level trends because when the dependent variable is first differenced, the county fixed effects capture trends. Here, the estimate again reverses sign from the baseline result, becoming positive, but remaining small, less than .003.

[22] For example, if the federal government targeted the border for reasons related to immigration enforcement rather than crime (as suggested by Cox & Miles 2013), but crime happened to be falling faster along the border than in other parts of the country, the basic difference-in-differences estimates could wrongly lead one to attribute falling crime rates to program activation.

To obtain a clearer portrait of the time-series of crime rates around the dates of activation in counties with high concentrations of immigrants, we estimated a specification analogous to an event study. The data for each county were restricted to observations 75 months before activation and 40 months after it, and a series of indicator variables for each month relative to the date of activation in counties with high concentrations of the foreign born were created.[23] The indicator for the *n*th month relative to the activation date takes the value one in counties at or above the 75th percentile of share of the foreign-born population during month *n* and takes the value zero otherwise. These indicators are equivalent to a set of lead and lag variables for the month of activation for counties with large foreign-born populations. These indicators were included in a regression that had the same set of controls as in equation (3). This specification captures the time path of crime rates in counties where Secure Communities produced the most detentions relative to other counties.

Figure 5 presents the coefficients for the indicators. It shows a steady downward decline in crime rates that begins well before the activation date and continues through the end of the observation period. The figure clarifies why including county trends has such a dramatic effect on the differences-in-differences estimates. Total index crime rates in counties with the highest concentrations of the foreign-born were declining before the rollout of Secure Communities began, and the decline continued steadily throughout the rollout period.

Panel B of Table 2 turns to the direct measures of immigrant detentions under Secure Communities. The measure is the (log) cumulative number of persons detained

[23] This restricts the sample to N = 259,097.

under Secure Communities between the activation date and the current date, per 100,000 persons in population. In the months before a county's activation, this variable is assigned the value zero. In each month following activation within a county, this variable captures how many noncitizens have been incapacitated by the program through that month. Specification A in column (1) shows a negative relationship between Secure Communities detentions and crime. The coefficient in this log-log regression equation can be read as an elasticity, and its magnitude, -.0163, is smaller than analogous estimates for prison populations (Johnson and Raphael 2012).

Specification B shows that the negative relationship in the baseline is due almost entirely to counties with the highest concentrations of foreign-born persons. This is consistent with the patterns of Figure 3; nearly all Secure Communities detentions occurred in counties with the highest shares of foreign-born population. Specification C shows that the association between crime and detentions in column (1) is most negative in counties directly on the southern border. Specification D decomposes the detainees by their ICE classifications. A persistent criminological finding is that criminal history predicts recidivism (Gendreau, Little, and Goggin 1996), and therefore, one might expect detentions of persons with more serious criminal histories to be associated with more substantial declines in crime. The estimates in Specification (D) and column (1) provide some support for this view. The coefficient on the number of detainees with more moderate criminal pasts (the L2 and L3 detainees) is slightly less negative than that for detainees with the most serious criminal histories (the L1 detainees). Detentions of immigrants without any criminal convictions has a slightly positive estimate, but none of the estimates for detentions in this specification achieve statistical significance.

In column (2), the same specifications are repeated but including county-specific trends. As with the differences-in-differences estimates in Panel A, the introduction of county trends drives the estimates to zero; none are statistically significant. Many of them fall by more than an order of magnitude, such as Specification A, which drops from -.0163 to -.0006. The precision of this estimate is worth noting. Its standard error implies that that even a coefficient as small as .009 would be statistically significant at the 5% level. Suggestive patterns, including the correlation between detainees' criminal history classification and the level of crime reduction, disappear.

The regressions in column (3) confirm the importance of crime trends within each location. In these regressions, the data are again expressed in first differences, and in effect, they relate changes in crime to the flow of new detainees under the auspices of Secure Communities. As with column (2), none of the coefficients imply a decline in crime rates. The only estimate above standard levels of significance is positively signed. The analysis of detentions thus confirms what the differences-in-differences estimates indicated: after accounting for each county's trend, the evidence shows that Secure Communities has not reduced the total index crime rate.

5.B. Robustness of Estimates

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

Table 3 first assesses the sensitivity of the estimates to two measurement issues. Missing values for the size of the local police force cause the panel to be unbalanced. Column (1) reports a regression in which the policing variable is excluded, and the sample is the balanced panel of counties by months. In column (2), the total index crime rate is calculated without adjusting for missing values in the UCR. Neither of these changes alters the inference drawn about the program's impact on crime. The coefficients are slightly more negative than the baseline estimate of -.0006, but both remain small (less than .01 in absolute value) and statistically insignificant.

The next three columns report results from alternative measures of the program's intensity. The regression in column (3) replaces the cumulative number of immigrants taken into federal custody under Secure Communities with the number taken into custody during that specific month. In effect, it measures the monthly "flow" of immigrants into custody under the program rather than the "stock." If the program shapes offending rates principally by changing the probability of ICE apprehension, then this flow measure would be a more appropriate measure of the policy intervention. The regression in column (4) considers nearly the opposite case. It 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 (5) includes both the cumulative detention and deportation measures. A possible theoretical justification for including both measures is that it permits one to disentangle the effects of shorter- and longer-term incapacitation. Yet, such fine theoretical distinctions may not be possible in practice. ICE detains immigrants before deporting them, making these measures highly correlated.

None of these specifications suggests a different conclusion about Secure Communities' impact. All the estimates imply small effects on crime, and none are statistically significant.

Since 2012, the popular media's coverage of Secure Communities has often focused on local "resistance" the program. As described above, local law enforcement agencies have no way to resist Secure Communities' core mechanical feature of screening all local arrestees for federal immigration violations, short of refusing to check arrestees' fingerprints. But local political authorities could in theory resist the program in other ways.

First, they could adopt policies that interfere with ICE's ability to take custody of immigrants identified through Secure Communities. Under such a policy the local authorities would refuse to honor ICE "detainers"— requests by ICE that local authorities hold an immigrant for up to forty-eight hours after the person would ordinarily be released, in order to give ICE time to take physical custody of the person. Doing so may increase the likelihood that a person will be released from local custody before ICE is able to take custody of the person. There were few such policies before 2013, but they have expanded rapidly in 2014, following a decision by a federal court in Oregon concluding that some detainers violate arrestees' Fourth Amendment rights (Medina 2014). We measure these policies using a comprehensive list assembled by the Catholic Legal Immigration Network (CLINIC 2014).[24]

[24] There is considerable variation in the stringency of these anti-detainer policies. As CLINIC describes them, they "range from broad limitations prohibiting local law enforcement from honoring any ICE detainer requests to more narrow measures restricting compliance to cases where ICE has obtained a warrant from a judge backed by probable cause, or when ICE has agreed to reimburse the locality for the costs of the detention, or when the individual has been convicted of a certain felony or other serious crime."

A second way in which a local government might resist Secure Communities is to adopt a so-called "sanctuary" policy. A sanctuary policy restricts local participation in immigration enforcement in various ways, and it is potentially broader than an anti-detainer policy insofar as it may limit other forms of local enforcement or cooperation.[25] The adoption of sanctuary policies occurred primarily before the launch of Secure Communities, and Gardner (2014) provided the authors with a hand-assembled list of sanctuary policies adopted between 2001 and 2008. It is worth noting that only about 10% of counties have adopted either of these policies, and they are correlated: 55% of counties with sanctuary policies also adopted anti-detainer policies.

While the timing of these policies prevents us from obtaining differences-in-differences estimates of their effects, we can interact the Secure Communities variables with dummies for the presence of these policies. This allows us to test whether Secure Communities had a different impact in counties that adopted such policies (or will adopt them shortly after our sample ends) than in other counties. In Column (6), the indicator is for an anti-detainer policy; in column (7), it is an indicator for a sanctuary policy; and in column (8), it is an indicator for either policy. If Secure Communities were effective in reducing crime and if local resistance impeded its efficacy, any drops in crime should be concentrated in counties without these polices. The regressions provide no support for this prediction. The coefficients for counties with these policies are negatively signed, while those without these policies are positively signed. None of them are statistically significant, and all of them imply changes in crime that are smaller than one percentage point.

[25] As with anti-detainer policies, there is significant variation in the degree to which any given policy restricts local enforcement collaboration.

5.C. Estimates for Individual Crime Rates

The existing literature on immigration and crime has devoted most attention to homicides and aggregate categories of violent and total index crimes. Under Secure Communities, large numbers of immigrants with histories of less serious offending or no offending at all have been detained. For these persons, less severe crimes such as property crimes may be more relevant. Table 4 presents a series of specifications for each of the seven specific categories of offenses comprising the FBI's index. Panel A contains results for four categories of violent crimes (homicide, rape, robbery, and aggravated assault), and panel B has the results for a fifth category of violent crime (simple assault) and the property crimes (burglary, larceny, and motor vehicle theft). For each offense, the odd-numbered column reports specifications with county and time fixed effects, and the even-numbered columns report the analogous specifications enhanced with county-specific trends.

The patterns for the individual offense categories largely mirror those for the total crime index. The violent crime categories show a high degree of sensitivity to the presence of county-level trends, often in opposite directions. For example, Specification B shows that the estimate for aggravated assault moves closer to zero, from -.0208 to -.0074, when the regression includes county trends, while that for robbery becomes more negative, moving from -.0118 to -.0233. To obtain a clearer view of the time path of crime rates in counties with a high proportion of the foreign born, event study estimates analogous to Figure 5 were estimated for the rates of individual offenses. Figure 6.A shows the patterns for offenses in the violent crime category. The estimates for aggravated assault show a steady, almost linear, decline which is consistent with Table 4's estimates moving from negative to zero in the presence of county-specific trends. Other categories have less linear patterns. Robbery, for example, is flat at the beginning of the observation window, appears to rise towards the middle, and falls at its end.

Simple assault is not an element of the FBI's crime index, but it is an offense that occurs frequently. It often reflects public disorder, and immigration has long been cited in social disorganization theories of crime (Shaw and McKay 1943, Sampson 2008). Figure 6A shows that simple assaults exhibit a decline that is similar to that of aggravated assaults but less pronounced. The rate of simple assaults appears to decline about 30 months before the activation of the program, and accordingly, the estimates in Panel B of Table 4 for simple assaults move from negative to zero in the presence of county-level trends.

Panel B of Table 4 displays the patterns for property crimes, which have received less attention in the immigration-crime literature. In most specifications without county trends, all of the property offenses have negative and statistically significant correlations with Secure Communities activation and detentions. Two of them – burglary and motor vehicle theft – continue to have negative and statistically significant estimates in the presence of county trends. In addition, the estimates for these two crime categories are more negative in counties with the highest concentrations of the foreign born and in counties on the southern border.

To determine why these two negative estimates are robust to the presence of county trends, Figure 6.B shows estimates for the time paths of the three property offenses. All three of these crime rates are relatively stable for at least four years before the activation of Secure Communities, and in the months after activation they all experience steep declines. If the declines commenced on the date of Secure Communities' activation or shortly thereafter, it would suggest that the program was the causal mechanism for the declines. The trend for burglary most closely matches this prediction, with declines appearing to commence right around the time of activation. In contrast, larceny does not conform to the prediction: the downward slope is evident about 20 months before the activation date. Motor vehicle theft is an intermediate case: it appears to decline 10 months before activation, but after activation the drop accelerates. These estimates provide some support for the conclusion that the detention of immigrants under Secure Communities reduced two categories of property crime: burglary and motor vehicle theft.

5.D Interpreting the Results

In short, the evidence shows that Secure Communities did not cause a meaningful reduction in either the FBI's overall index crime rate or in rates of violent crimes. This is important because Secure Communities specifically, and criminal deportation policies more generally, have long been publicly justified primarily on grounds that they keep communities safer from violent crime. At the same time, we find suggestive evidence that Secure Communities is associated with modest reductions in two property offenses. Homicide and violent crimes, rather than property crimes, have typically been the focus of the empirical literature on the immigration-crime connection (e.g., Martínez and Rosenfield 2001, Sampson et al. 2008, Martínez et al. 2010).

If the estimated declines in burglary and motor vehicle theft are interpreted as the causal effects of the expansion in detentions rather than artifacts of downward trends in crime, the question arises how one might evaluate the significance of the estimates. One approach is to compare the crime-reducing potential of criminal incarceration with detention through Secure Communities. From this perspective the estimates denote quite modest effects on these crimes. Evaluated at the sample mean, they imply that the annual detention of one additional immigrant prevents .18 additional burglaries and .12 additional motor vehicle thefts. By comparison, Johnson and Raphael (2012) estimate that during the period 1991-2004, the annual imprisonment of one additional criminal prevents about .514 burglaries and about .505 motor vehicle thefts. (In addition, they find that increasing imprisonment by one criminal prevents approximately 1.5 larcenies and .3 violent crimes.) This implies that the marginal Secure Communities detainee is a far less frequent and serious offender than the marginal prison inmate.

Put differently, these estimates imply that Secure Communities prevented a very small share of criminal offenses nationwide. The growth of the detainee population and the estimated elasticities in specification B of Table 4 imply that Secure Communities prevented 65,586 burglaries and 46,093 motor vehicle thefts since its launch. These figures represent .75% and 1.55% of the burglaries and motor vehicle thefts committed, respectively, nationwide between 2009 and 2012.

Another way of assessing the estimates—one that circles back to the literature on the relative rates of offending by immigrants and citizens—is to consider their implications for the difference in offending patterns of immigrants and the native born. To see the relevant parameters, it is helpful to rearrange equation (2):

$$\beta_i - \beta_n = (\partial\alpha_i/\partial S)^{-1}[\partial\beta/\partial S - \alpha_i\partial\beta_i/\partial S]. \tag{4}$$

The left-hand side of the equation now represents the gap in the two groups' offending rates. All of the parameters on the right-hand side, except one, are known or estimated in the empirical analysis. The main finding of Table 2 was that absence of any impact on aggregate crime rates, or $\partial\beta/\partial S \approx 0$. The term α_i is known because as discussed above, immigrants comprise roughly 11% of the population. ICE's FOIA disclosures show that Secure Communities has detained roughly 1.13% of the foreign-born population, which permits us to estimate $\partial\alpha_i/\partial S$.

The one unknown parameter on the right-hand side of equation (4) is $\partial\beta_i/\partial S$, which is the deterrent response of immigrants to the program. If this effect were zero, it would imply, given the other known values, that immigrants offend at rates similar to the native born. Yet, determining the magnitude of any deterrent effect is difficult, and there are few convincing estimates of deterrence, as distinguished from incapacitation (Miles and Ludwig 2007; Dulauf and Nagin 2011). A prominent estimate was produced by Kessler and Levitt (1999), who exploited variation in sentence enhancements for recidivists. Their estimates range from a 4% decline in crime in the first year following enactment of the enhancement to an 8% decline in three years. An implication of the paper's estimates and equation (4) is simply that even modest deterrent responses to the program's threat of detention and deportation would imply that immigrants offend less frequently than the native born.

6. Conclusion

The finding that Secure Communities does not reduce rates of violent crime or the overall rate of FBI index crime calls into question the longstanding assumption that deporting noncitizens who commit crimes is an effective crime control strategy. Our estimates suggest that the marginal immigrant detainee is a much less serious offender than the marginal prisoner in the criminal justice system--even when that immigrant detainee has been selected for detention using a program designed to target the most serious immigrant criminal offenders. This lower offending rate undercuts the effect that incapacitation-through-deportation has on overall crime rates. Moreover, as we explained earlier, the elasticity of immigrant offending rates to deportation policies is likely to be inversely correlated to the seriousness of the crime. While proponents have focused on the program's potential impact on violent and more serious crimes, a deterrent effect may be more likely for less serious offenses. Our estimates our consistent with this hypothesis, as the only index crimes for which there was even suggestive evidence of a small reduction associated with Secure Communities were the less serious property crimes burglary and perhaps motor vehicle theft.

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Table 1.
Summary Statistics

Variable	Mean	Standard Deviation
Secure Communities Enforcement		
Activated?	.2359	.4226
Detentions per 100,000 (all counties)	14.3209	60.5598
Detentions per 100,000(only activated counties)	59.6907	112.1534
Fraction Foreign Born	.1225	.1051
Fraction Hispanic	.1582	.1671
Fraction Black	.1205	.1232
Population Density	1,067.42	1,636.87
Median Household Income	51,035.04	1,3551.73
Fraction Female-headed Households	.1890	.0512
Sworn officers per 100,000	7.263	5.125
287(g) Agreement?	.2631	.4403

Notes: N = 302,388. Observations are monthly, county-level data from 2004-2012. Number of counties in sample = 2,985. Weighted by population.

Table 2.
Impact of "Secure Communities" on Rate of Total Index Crime:
OLS Regression Estimates

Panel A. Differences-in-Differences Estimates

	Specification of Dependent Variable		
	Log Levels	Log Levels	Log Changes
Explanatory Variable	(1)	(2)	(3)
Regression Specification A			
Activated	-.0400**	.0025	.0025**
	(.0173)	(.0118)	(.0010)
Regression Specification B			
Activated x 75^{th} Percentile of Fraction Pop. Foreign Born	-.0545**	-.0024	.0039**
	(.0191)	(.0144)	(.0010)
Activated x Below 75^{th} Percentile of Fraction Pop. Foreign Born	.0067	.0120	-.0016
	(.0154)	(.0095)	(.0014)
Regression Specification C			
Activated x Border County	-.1887**	-.0067	.0080**
	(0.0282)	(.0569)	(.0019)
Activated x Not Border County	-.0312	.0028	.0023
	(.0170)	(.0114)	(.0011)
County Fixed Effects	Y	Y	Y
County-level Trends	N	Y	N

Notes: ** $p < 0.05$, * $p < 0.1$. The dependent variable is the log of the monthly index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 302,388. Number of counties in sample = 2985.

Table 2 (con't).
Impact of "Secure Communities" on Rate of Total Index Crime:
OLS Regression Estimates

Panel B. Measures of Activation Interacted with (Log) Cumulative Persons in ICE Custody per Capita

	Specification of Dependent Variable		
	Log Levels	Log Levels	Log Changes
Explanatory Variable	(1)	(2)	(3)
Regression Specification A			
Persons in ICE Custody	-.0163**	-.0006	-.0013
	(.0051)	(.0045)	(.0029)
Regression Specification B			
Persons in ICE Custody x 75th Percentile of Fraction Pop. Foreign Born	-.0165**	-.0006	-.0017
	(.0052)	(.0048)	(.0036)
Persons in ICE Custody x Below 75th Percentile of Fraction Pop. Foreign Born	-.0067	-.0007	-.0004
	(.0063)	(.0045)	(.0037)
Regression Specification C			
Persons in ICE Custody x Border County	-.0410**	-.0013	.0121**
	(.0042)	(.0112)	(.0042)
Persons in ICE Custody x Not Border County	-.0134**	-.0006	-.0021
	(.0054)	(.0047)	(.0030)
Regression Specification D			
Persons in L1 Category in ICE Custody	-.0189*	.0036	-.0013
	(.0116)	(.0053)	(.0034)
Persons in L2 & L3 Categories in ICE Custody	-.0144	-.0059	.0044
	(.0091)	(.0054)	(.0027)
Persons in Non-criminal Category in ICE Custody	.0132	.0022	-.0037
	(.0100)	(.0057)	(.0028)
County Fixed Effects	Y	Y	Y
County-level Trends	N	Y	N

Notes: ** $p < 0.05$, * $p < 0.1$. The dependent variable is the log of the monthly index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 302,388. Number of counties in sample = 2,985.

Table 3.
Impact of "Secure Communities" on Rate of Total Index Crime: Sensitivity of Estimates

Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Persons in ICE Custody	-.0013 (.0050)	-.0020 (.0033)	-.0038 (.0059)		.0066 (.0044)			
Persons Deported				-.0010 (.0043)	-.0029 (.0041)			
Persons in ICE Custody x Local Immigrant Policy						-.0064 (.0042)	-.0010 (.0048)	-.0055 (.0042)
Persons in ICE Custody x No Local Immigrant Policy						.0082 (.0064)	.0003 (.0055)	.0057 (.0064)
Change to Baseline Regression Specification:	Exclude Police per Capita	Crime Measured without Imputation	Persons in ICE Custody Measured as Flow					
Type of Local Immigrant Policy:						Anti-detainer	Immigrant Sanctuary	Either Anti-detainer or Sanctuary

Notes: ** p < 0.05, * *p* < 0.1. The dependent variable is the log of the monthly index crime rate. The table reports regression coefficients, with standard errors in parentheses. The Baseline Regression Specification on which each regression is based is the regression in Table 2, Panel B, column 2, which includes county-specific trends. In all columns but column (1), N = 302,388, and the number of counties in sample = 2,985. In column (1), N=306,244, and the number of counties = 3,114.

Table 4.
Impact of “Secure Communities” on Rates of Specific Offenses: OLS Regression Estimates

Panel A. Violent Offenses – Log Levels

	Homicide		Rape		Robbery		Aggravated Assault	
Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Regression Specification A.								
Activated Indicator	-.0162	.0448**	-.0133	-.0187	-.0206	.0048	-.0432**	.0033
	(.0296)	(.0214)	(.0294)	(.0194)	(.0144)	(.0109)	(.0204)	(.0109)
Regression Specification B.								
Persons in ICE Custody	-.0260**	-.0130*	-.0099	-.0208**	-.0118**	-.0233**	-.0208**	-.0074*
	(.0081)	(.0078)	(.0073)	(.0074)	(.0042)	(.0048)	(.0072)	(.0043)
Regression Specification C.								
Persons in ICE Custody x	-.0262**	-.0170**	-.0098	-.0215**	-.0118**	-.0260**	-.0212**	-.0072*
75th percentile foreign born	(.0082)	(.0083)	(.0073)	(.0075)	(.0043)	(.0050)	(.0072)	(.0045)
Persons in ICE Custody x	-.0184*	.0147	-.0113	-.0158	-.0110	-.0045	-.0064	-.0088
Not 75th percentile foreign born	(.0101)	(.0123)	(.0127)	(.0122)	(.0087)	(.0087)	(.0095)	(.0089)
Regression Specification D.								
Persons in ICE Custody x Border County	-.0250*	-.0249	.0019	-.0165	-.0162**	-.0167**	-.0140**	.0158**
	(.0128)	(.0177)	(.0089)	(.0153)	(.0043)	(.0075)	(.0053)	(.0053)
Persons in ICE Custody x	-.0261**	-.0122	-.0112	-.0211**	-.0113**	-.0238**	-.0216**	-.0091**
Not Border County	(.0087)	(.0081)	(.0076)	(.0075)	(.0045)	(.0050)	(.0077)	(.0045)
Regression Specification E.								
Persons in ICE Custody x	-.0308**	-.0168	-.0056	-.0074	-.0261	-.0074	-.0314**	-.0034
L1 Persons in ICE Custody	(.0155)	(.0145)	(.0136)	(.0111)	(.0145)	(.0096)	(.0168)	(.0090)
Persons in ICE Custody x L2/L3 Persons	.0066	-.0016	.0129	-.0181	.0078	-.0168	-.0026	-.0100
in ICE Custody	(.0157)	(.0183)	(.0153)	(.0125)	(.0129)	(.0112)	(.0135)	(.0100)
Persons in ICE Custody x Noncriminal	-.0146	-.0120	-.0221	-.0030	.0021	-.0070	.0068	.0058
Persons in ICE Custody	(.0149)	(.0183)	(.0143)	(.0113)	(.0125)	(.0092)	(.0123)	(.0095)
County-level Trends	N	Y	N	Y	N	Y	N	Y

Note: ** $p < 0.05$, * $p < 0.1$. The dependent variable is the log of the monthly index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 302,388. Number of counties in sample = 2,985.

Table 4 (con't).
Impact of "Secure Communities" on Rates of Specific Offenses: OLS Regression Estimates

Panel B. Simple Assault and Property Offenses – Log Levels

			Property Offenses					
	Simple Assault		Burglary		Larceny		Motor Vehicle Theft	
Explanatory Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Regression Specification A.								
Activated Indicator	-.0269**	-.0034	-.0411**	-.0032	-.0342**	.0083	-.0617**	-.0161
	(.0106)	(.0078)	(.0181)	(.0085)	(.0154)	(.0074)	(.0277)	(.0111)
Regression Specification B.								
Persons in ICE Custody	-.0156**	-.0044	-.0185**	-.0146**	-.0127**	.0006	-.0173**	-.0230**
	(.0036)	(.0029)	(.0047)	(.0038)	(.0043)	(.0031)	(.0085)	(.0044)
Regression Specification C.								
Persons in ICE Custody x	-.0156**	-.0039	-.0189**	-.0162**	-.0130**	-.0004	-.0175**	-.0234**
75th percentile foreign born	(.0037)	(.0034)	(.0047)	(.0041)	(.0044)	(.0034)	(.0085)	(.0047)
Persons in ICE Custody x	-.0140*	-.0079	-.0063	-.0030	-.0012	.0081*	-.0105	-.0204**
Not 75^{th} percentile foreign born	(.0077)	(.0058)	(.0074)	(.0059)	(.0067)	(.0043)	(.0113)	(.0075)
Regression Specification D.								
Persons in ICE Custody x	-.0245**	.0033	-.0357**	-.0209**	-.0347**	.0226	-.0633**	-.0864**
Border County	(.0064)	(.0050)	(.0063)	(.0066)	(.0055)	(.0259)	(.0094)	(.0170)
Persons in ICE Custody x	-.0145**	-.0050*	-.0166**	-.0141**	-.0101**	-.0010	-.0120	-.0183**
Not Border County	(.0038)	(.0030)	(.0048)	(.0040)	(.0043)	(.0032)	(.0088)	(.0043)
Regression Specification E.								
Persons in ICE Custody x	-.0134	-.0010	-.0304**	-.0158**	-.0242**	-.0005	-.0100	.0061
L1 Persons in ICE Custody	(.0120)	(.0066)	(.0116)	(.0072)	(.0112)	(.0047)	(.0166)	(.0092)
Persons in ICE Custody x	.0027	-.0020	-.0137	-.0020	-.0034	.0047	-.0133	-.0235**
L2/L3 Persons in ICE Custody	(.0127)	(.0069)	(.0104)	(.0073)	(.0091)	(.0063)	(.0159)	(.0098)
Persons in ICE Custody x	-.0104	-.0037	.0201*	-.0053	.0111	-.0042	.0037	-.0120
Noncriminal Persons in ICE Custody	(.0100)	(.0060)	(.0107)	(.0065)	(.0099)	(.0052)	(.0151)	(.0095)
County-level Trends	N	Y	N	Y	N	Y	N	Y

Note: ** $p < 0.05$, * $p < 0.1$. The dependent variable is the log of the monthly index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 302,388. Number of counties in sample = 2,985.

Figure 1.
Pattern of Secure Communities Activation

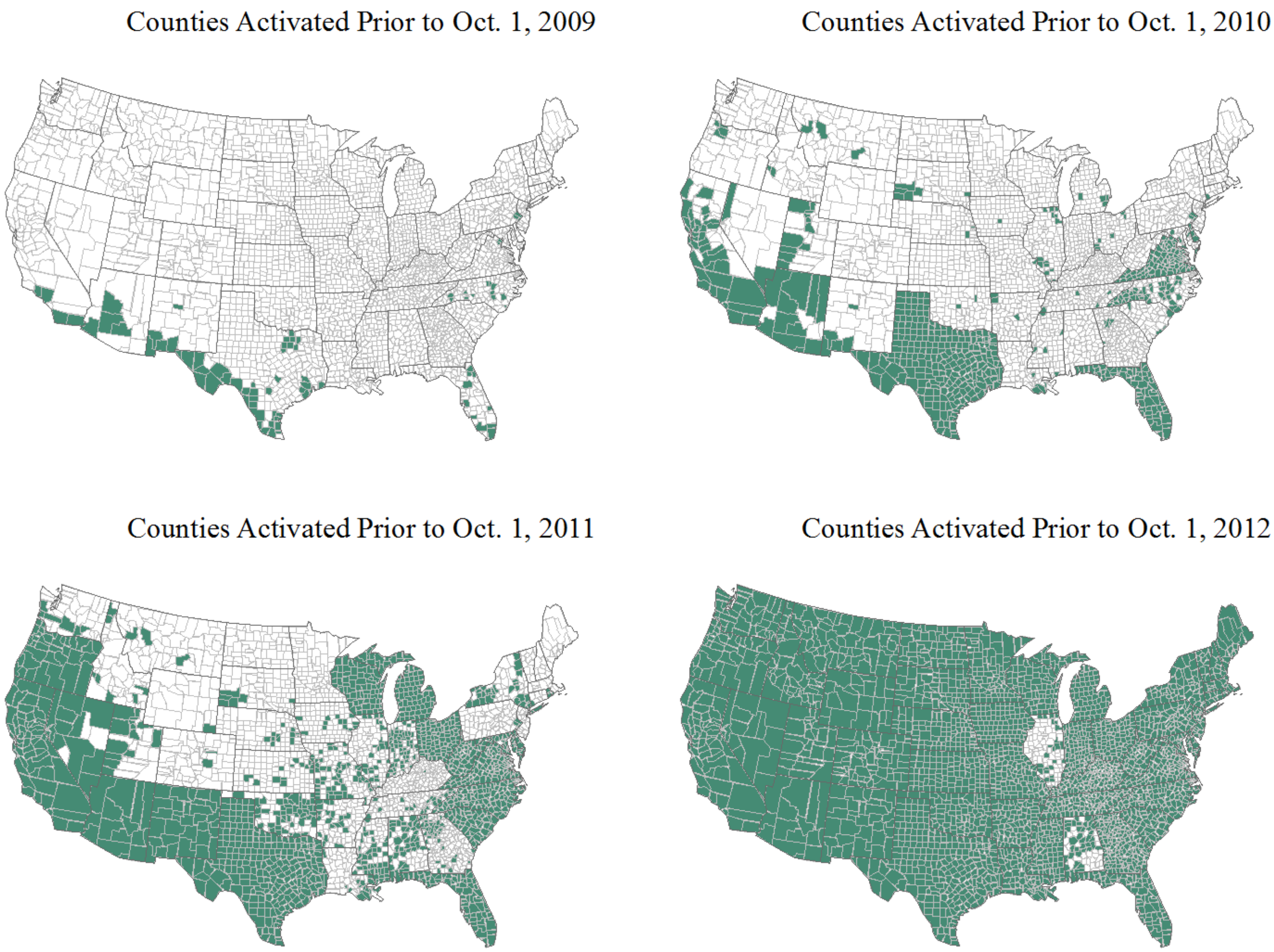


Figure 2.
Share of Population Covered by Secure Communities by Date

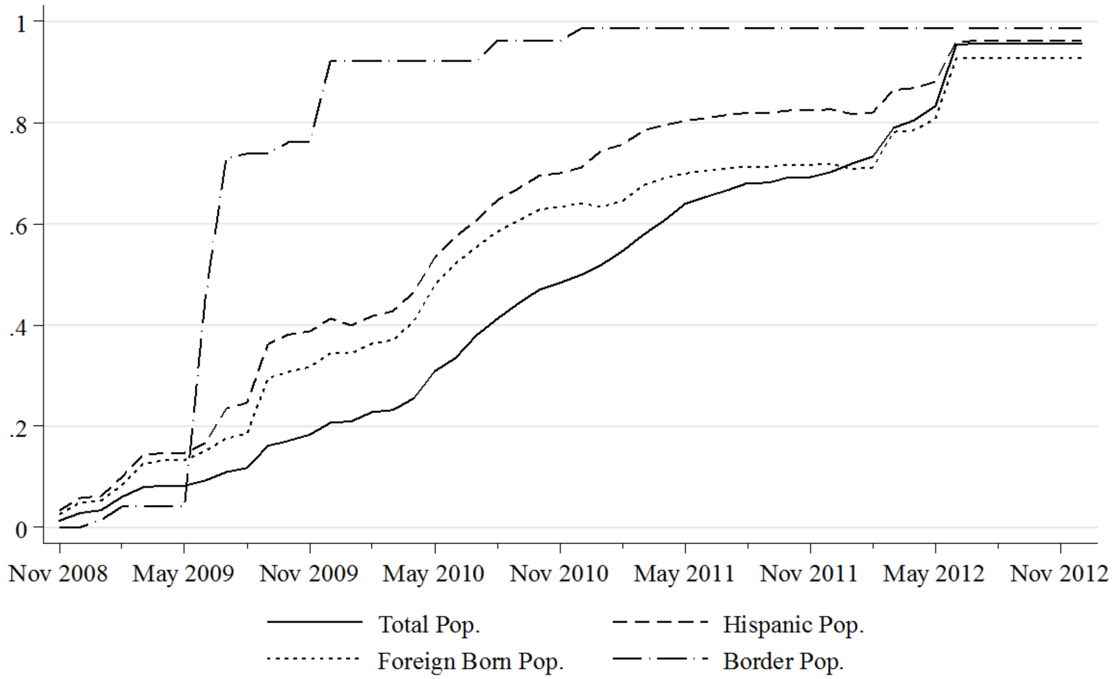
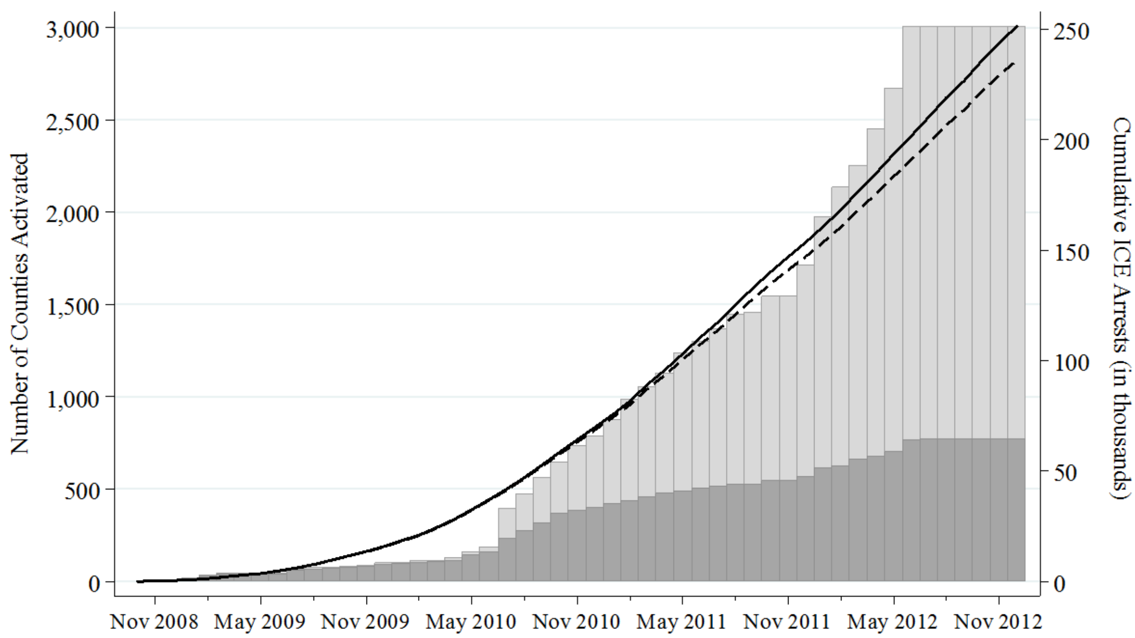


Figure 3.
Persons Taken into ICE Custody under Secure Communities
and Number of Activated Counties by Date



Note: Lines (right scale) measure number of persons taken into federal custody under Secure Communities. The solid line indicates Secure Communities arrests for all counties while the dotted line indicates arrests for counties whose share of foreign born residents is in the top quartile of all counties. Bars (left scale) tally the number of counties that have been activated under Secure Communities. The lighter (taller) bars indicate all activated counties while the darker (shorter) bars indicate the number of activated counties whose share of foreign-born residents is in the top quartile of all counties.

Figure 4.
ICE Classification of Persons Taken into Federal Custody under Secure Communities

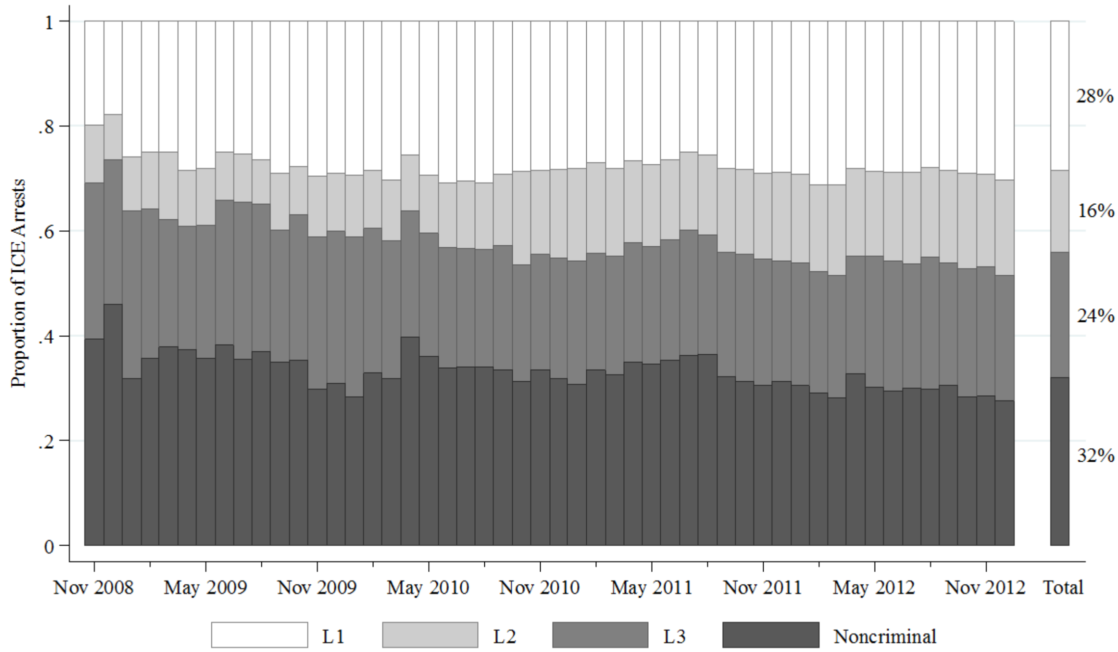
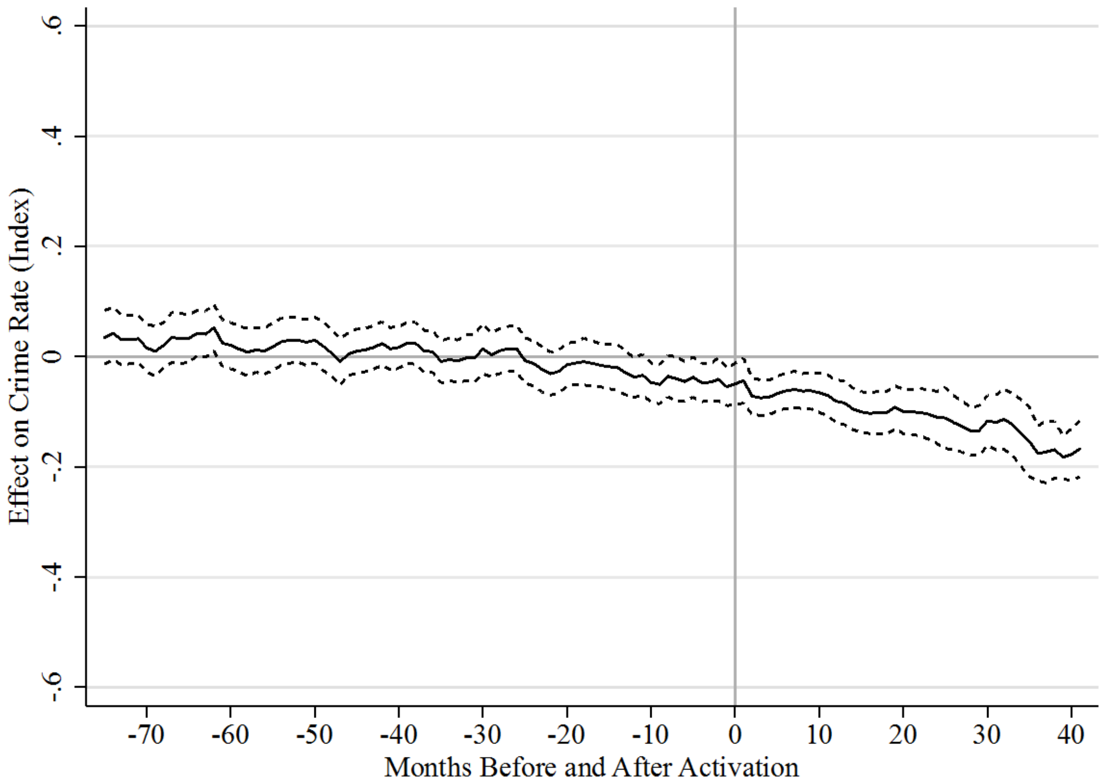


Figure 5.
Log Total Index Crime Rates in Counties in 75th Percentile of Foreign-born Population, Before and After Secure Communities Activation



Note: The Solid line plots point estimates, and dotted lines plot 95% confidence intervals.

Figure 6.
Estimates of Log Crime Rates in Counties at or above the 75th Percentile of Foreign-born Population, Before and After Secure Communities Activation

A. Violent Crimes (Homicide and Rape)

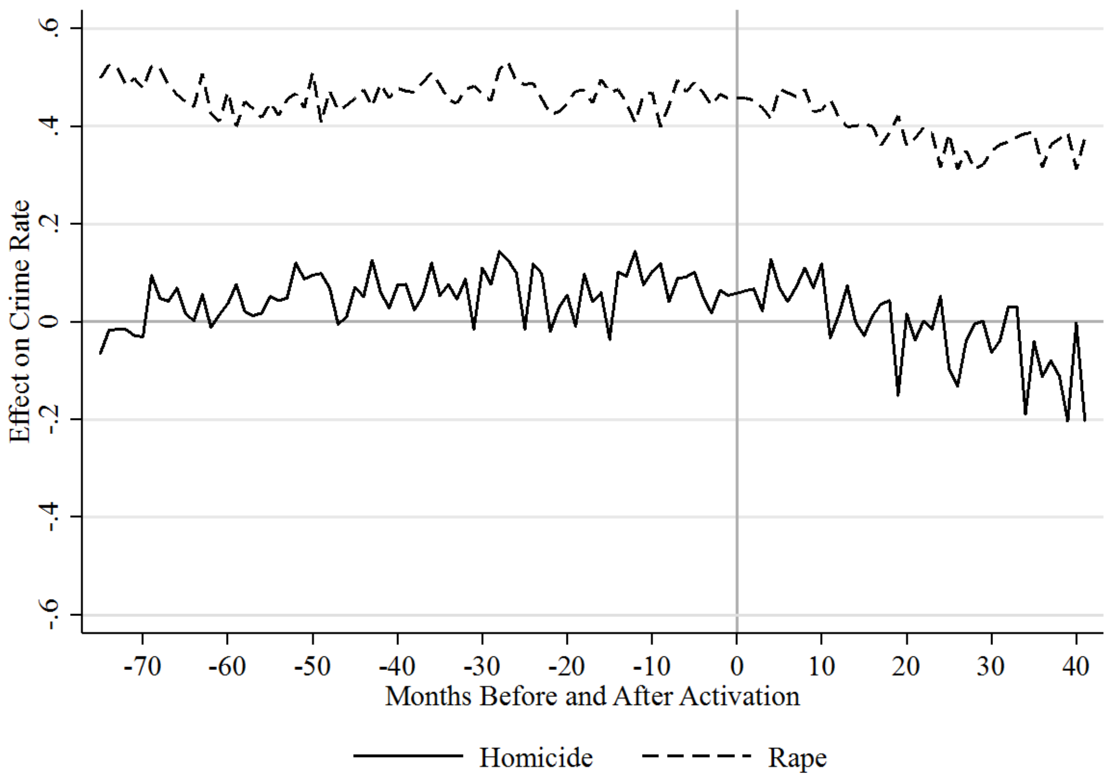


Figure 6 (con't).
Estimates of Log Crime Rates in Counties at or above the 75th Percentile of Foreign-born Population, Before and After Secure Communities Activation

A. <u>Violent Crimes (Robbery, Aggravated Assault, Simple Assault)</u>

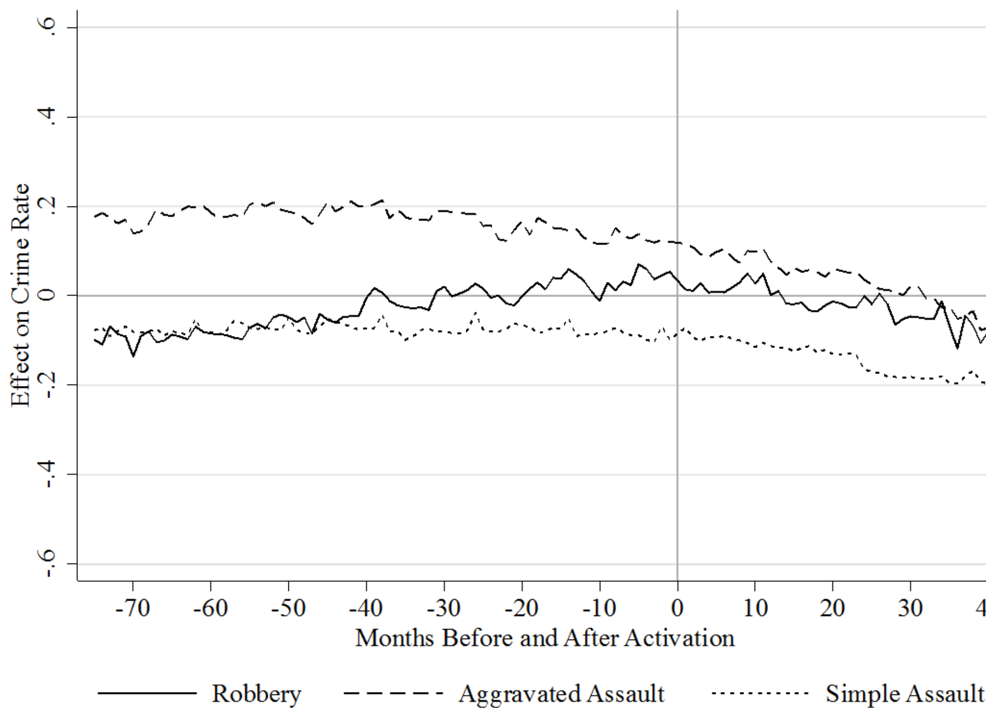


Figure 6 (con't).
Estimates of Log Crime Rates in Counties at or above 75th Percentile of Foreign-born Population, Before and After Secure Communities Activation

B. Property Crimes