Immigration Enforcement and Crime: Evidence from the 287(g) and Secure Communities Programs*

Rebecca Jackson | Policy Analysis and Management, Cornell University

*College of Human Ecology Senior Honors Thesis, Spring 2018

Immigration Enforcement and Crime:
Evidence from the 287(g) and Secure Communities Programs

Abstract

The primary question of this analysis is whether policies that create a streamlined deportation process for unauthorized immigrant offenders decrease crime rates. Using the policy variations of Section 287(g) of the Immigration and Nationality Act and the Secure Communities program, I estimate the effect of immigration policy enforcement on crime rates using a difference-in-difference strategy. The results are congruent with previous immigration enforcement literature estimating that neither program had a meaningful effect on crime rates. While there is no strong evidence pointing to negative effects of the programs on crime, certain specifications and subsamples analyzed in this paper show that the 287(g) program may be associated with an increase in crime. I suggest that this effect may be driven by changes in crime detection and recommend replication with more precise crime and victimization data to strengthen the estimates and provide evidence for understanding the mechanisms through which these programs operate.

Introduction

An extensive body of research studies the impact of immigration on America's labor markets, political composition and social dynamics. For decades researchers have been asking how immigrants differ from natives and what impact they have on the fabric of American life. One fundamental concern is that immigrants may bring crime to American cities, straining law enforcement agencies and making communities more dangerous across the nation.

Much of the United States' current immigration enforcement policy is grounded in the concern that opening our borders threatens the safety of native communities. With the creation of the Department for Homeland Security, following the 9/11 attacks, politicians and legislators alike have become more vigilant to the threat of outsiders breaching American borders. Recent legislation has further built upon this belief, increasingly honing in on criminality as a basis for immigration regulations and using deportation as a tool to remove the most dangerous immigrants by force.

This paper serves to shed light on one central question: do our current immigration enforcement policies that target unauthorized immigrant offenders achieve their central goal of reducing crime in American neighborhoods? Through a difference-in-difference approach, I will analyze the effect of Section 287(g) of the Immigration and Nationality Act and the Secure Communities program on crime rates in order to assess the efficacy of deportation-focused policies. This question is at the heart of current immigration policy debate and is crucial for ensuring that the government implements productive and efficient policy.

The Relationship Between Immigration and Crime

According to a classical labor economic framework known as the Roy Model, immigrants who are unskilled see positive returns to migration and are thus incentivized to come to the U.S. (Heckman and Honore, 1990). This phenomenon is known as "negative selection" where the poorest, least educated foreign-born individuals migrate to the U.S. seeking opportunity. As a result, much of the immigrant population is associated with high levels of poverty and low levels of education, characteristics that are often strongly linked to criminal activity. Further weight is given by Shaw and McKay's (1942) theory of "social disorganization" that describes how the propensity for crime increases with the breakdown of social organization. Such disorganization, the authors explain, is characterized by residential mobility, high levels of poverty and ethnic heterogeneity, all factors that occur with increased immigration. Similarly, Strain Theory, introduced by sociologist Robert K. Merton in 1938, explains how the societal pressures of achieving the American dream matched with a lack of resources leads to a strain in which immigrants are more likely to commit crimes to realize their goals. Each of these theoretical models supports the idea that immigrant populations have higher crime rates than their native counterparts.

While fear of immigrant crime is widespread, research has shown that immigrants do not commit proportionally more (and maybe even fewer) crimes than natives. Using FBI Uniform Crime Report data, Butcher and Piehl (1998) found that immigration flows have no effect on crime rates and that youth born abroad are in fact less likely to commit crimes than native-born youth. Using cross-sectional variations, Adelman, Reid and Markle (2016) reaffirmed this finding by examining crime rates from 200 metropolitan areas and the density of their immigrant populations over a forty-year period (1970-2010). After controlling for a series of neighborhood characteristics, they found that areas with a higher share of immigrants had lower rates of violent and property crime.

Following the Mariel boatlift, a mass immigration of Cubans onto the shores of Miami in 1980, discussion over Hispanic immigration came to the forefront of policy debate. The influx of 125,000 immigrants onto American soil within just seven months presented substantial research opportunities for the study of immigration. Ramiro Martinez Jr. and Jacob I. Stowell (2012) evaluated the effect of immigration on two dominantly Latino cities: San Antonio, Texas and Miami, Florida. While San Antonio did not see the sudden influx of immigrants that Miami did in the eighties, it is known for its steady flow of Mexican immigrants given its proximity to the southern border. The authors found that with the increase in immigration between 1980 and 1990, homicide and drug-related homicide in both cities decreased. Martinez and Stowell (2012) generalized this research by running regressions on 1,000 “new destination” counties in the year 2000. Their results echoed what they found in Miami and San Antonio, reiterating the negative correlation between immigration and homicide rates.

Despite low crime rates in areas with recent flows of immigrants, research shows that immigrants are disproportionally represented in correctional facilities. John Hagan and Alberto Palloni (1999) investigated this phenomenon in two U.S. border cities, El Paso and San Diego and attributed this over- representation to biases in the length and conditions of pre-trial detention and the corresponding effects on conviction and sentencing. The authors concluded that immigrant incarceration exaggerates the immigrant crime rate between three and seven percent.

Butcher and Piehl (2005) suggest that the discrepancy in crime rates between immigrants and natives is due to the self-selection of individuals who choose to migrate (quite in contrary to some theoretical models). They conclude that these individuals either have lower criminal propensity or are more responsive to the deterrent effects of criminality than the average native, making them inherently less likely to commit crimes.

Institutional Background

Despite empirical evidence that suggests otherwise, American policy discussions have long surrounded the belief that strict immigration enforcement is directly linked to the safety of

American cities. As a result, a series of bills have been passed in recent years to ensure that the United States prioritizes the removal of unauthorized immigrants with a specific focus on immigrant offenders. Unlike U.S. *citizen* criminal offenders who are placed through the criminal justice system upon conviction, unauthorized immigrant offenders can be deported and do not have the right to stay in the country upon conviction. This allows the federal government to simply remove undesirable individuals if they lack sufficient legal documentation.

The United States has a long history of using quota systems to ensure only the most desirable immigrants are granted entry. After the Great Depression and World War II, a global halt in migration began to harm the U.S. labor market. To rectify this, the government created the Bracero program in 1942. In order to fill the cheap labor pools, the government sponsored the entry of Mexican immigrants to fill secondary sector jobs that natives refused to work. This created a precedent for Mexican migration that has sparked much of the debate over immigration in the U.S. Currently, over 11 million unauthorized immigrants reside within U.S. borders, many of whom work in the agriculture, manufacturing and construction industries, often living in extreme poverty due to low wages. As the unauthorized population has grown, the government has become increasingly concerned about their effects on native society.

Over the past century, the U.S. government has crafted extensive screening systems and visa programs attempting to keep out the "bad" immigrants and allow in the "good" ones (Migration Policy Institute). In practice, it is very difficult to ensure that those who cross the border both legally and illegally are not bringing crime to their destination cities. Due to a lack of successful border enforcement and the large unauthorized immigrant population currently residing in the U.S., the government has shifted towards retroactive policies aimed at the removal of the "bad" immigrants.

Known as the largest crime bill in the history of the United States, The Violent

Crime Control and Law Enforcement Act (HR 3355, P.L. 103-322) created an explicit legislative connection between immigration and crime. The bill, passed by Congress in 1994, authorized $1.2 billion in federal resources to increase border control, prioritize the deportation of criminal immigrants and help states jail criminal unauthorized immigrants. This federal support set the stage for a campaign against unauthorized criminal offenders.

Focal Programs: Secure Communities and 287(g)

With the Bush administration came a new series of bills to address immigration and crime.

Piloted in 2008, Secure Communities streamlined the process of deporting unauthorized criminal offenders by creating a partnership between federal and local correctional facilities. Upon receiving a criminal suspect, local law enforcement agencies run fingerprints that are sent to Immigration and Customs Enforcement (ICE) to verify the suspect's immigration status. If they are found to be unauthorized, ICE may place a "detainer" on them, requesting that the local jail hold the suspect for 48 hours beyond the scheduled release date. This gives ICE the chance to obtain custody of the suspect and begin deportation proceedings. This program was implemented between the years 2008 and 2013 and has now been universally adopted in every county in the United States.

Targeting the same population from a slightly different angle, Section 287(g) of the Immigration and Nationality Act (8 U.S.C. § 1357(g)) created federal-local law enforcement partnerships to improve the efficiency of deportation of criminal immigrants. The 287(g) program allowed for the Department of Homeland Security to train state and local authorities to identify, process and detain criminal immigrants. This partnership deputizes local police to work as immigration authority, using their newfound power to identify and detain unauthorized individuals without the involvement of federal agencies. Under the program, 287(g) officers are given the power to check DHS databases for immigration status information, interview immigrants to ascertain their status, issue ICE detainers, place immigration charges to initiate formal removal proceedings and transfer unauthorized immigrants into ICE custody. The basic program has three models: jail enforcement, task force and hybrid. The jail enforcement model allows officers to inquire into immigration status and act upon their findings once an individual is brought to the local jail. The task force model deputizes officers to inquire into immigration status in the field, to issue arrest warrants for immigration violations and execute search warrants. The hybrid model is a combination of the jail enforcement and task force model, where officers within local jails work closely with officers in the field to identify and detain unauthorized criminal offenders.

Under the 287(g) program, ICE has agreements with 60 law enforcement agencies in 18 states, and according to DHS, has identified over 402,000 "removable aliens". In a memo to ICE federal officials in February 2017, DHS reiterated the importance of 287(g) deputized law enforcement presence at local jails to prioritize the removal of criminal immigrants.

Effects of Deportation Policies

While there is substantial research on the negative link between immigration and crime, U.S. policies continue to target criminal immigrants. In an era of new political leadership many are looking to the government to shape the future of immigration policy. The Trump administration has been explicit in its efforts to reinforce legislation that target unauthorized criminals through the restoration and revitalization of deportation policies. As promised, on January 25 2017, President Trump signed the "Enhancing Public Safety in the Interior of the United States" executive order, which revitalized both Secure Communities and the 287(g) programs.

This revitalization raises an important question about whether the targeting and deportation of unauthorized immigrants who commit crimes actually reduces crime rates. Both 287(g) and Secure Communities have come at a high cost. Under 287(g), detainees were held for an average of 81 days, with a cost of $60 per day, incurring an average cost per individual detention of $5,000. Between the years 2006 and 2016, the federal government allocated over $460 million to the program. Similarly, data from Los Angeles County shows that Secure Communities costs the county over $26 million annually, due to the high costs of holding suspects in local jails on immigration detainers at ICE's request. Across the nation these programs prove to be extraordinarily costly.

Determining the efficacy of these policies in attaining their goals is therefore essential. While evidence on the link between immigration and crime reported earlier suggests that deporting immigrant offenders may not be the most efficient way to reduce crime rates, it is possible that it will have an impact nonetheless. Given that, on average, unauthorized immigrants commit fewer crimes than natives, deporting the most egregious offenders may in fact reduce crime rates. Despite their magnitude, few empiricists have focused on the effects of these deportation-focused policies. An exception is Miles and Cox (2014), who evaluated the effect of the Secure Communities program across more than 3,000 U.S. counties. They found that it had no meaningful effect on the FBI index crime rate nor did it reduce violent crime rates. Their analysis is evidence against the belief that deporting criminal immigrants makes our cities, neighborhoods and communities safer. This paper serves to expand upon their research and determine whether their findings can be generalized to other similar immigration enforcement programs.

While Miles and Cox (2014) present a thorough analysis of the Secure Communities program's effect of crime rates, this paper will explore some aspects that their model did not account for. While Secure Communities was implemented in selected counties across the nation and presents an opportunity for a difference-in-difference analysis, its implementation overlapped heavily with the Great Recession, which has been shown to be associated with trends in crime (Uggen, 2012), likely biasing Miles and Cox's estimates downward. Additionally, the Secure Communities program was rolled out in a non-random pattern, targeting states closer to the border first and inland states only later. This paper will examine a similar question to the one Miles and Cox evaluate but from a different perspective and with the introduction of the 287(g) program as an important policy treatment in itself.

Despite the fact that the program was not implemented on a national level, including the 287(g) program in this analysis has advantages over analyzing only Secure Communities. In contrast to Secure Communities, the 287(g) program presents an opportunity to reduce the selection bias in that there were counties that applied for the program and were rejected. It is harder to argue that counties that implemented 287(g) partnerships were substantively different from those that did not because it is likely that most counties that applied had baseline commonalities. Including precise 287(g) data may yield a more precise and accurate estimate of the effect of deportation policy on crime rates.

The primary question of this analysis is whether deportation-focused immigration policies decrease crime rates. As the explicit goal of both Secure Communities and the 287(g) program was to decrease crime, one would expect that if these program were to be effective, crime rates in counties with Secure Communities and 287(g) partnerships should decrease at a faster rate than those that did not implement the programs (or implemented them at a later date). Despite the fact that immigrants commit crimes at a lower rate than natives, presumably removing those who *do* commit crimes would still decrease crime rates overall. Following this logic, counties that went into Secure Communities and 287(g) partnerships with the Department of Homeland Security should see more of a reduction in crime rates than similar counties that did not.

Data and Empirical Strategy

Policy and Outcome Measures

To evaluate the effect of immigration enforcement policy on crime, I have assembled a panel dataset from a variety of sources. For each county-year observation the data include crime statistics, demographic information and variables indicating whether the 287(g) and Secure Communities programs were implemented at that point in time. The data span from the year 2004 to 2012. The time frame begins with 2004 to allow ample time before the programs were implemented and terminates with the latest publicly available crime data in 2012.

Annual county crime measures come from the FBI's Uniform Crime Reports. Crimes reported include property crimes (burglary, larceny, motor vehicle theft and arson) and violent crimes (murder, rape, robbery and aggravated assault) recorded for each county each year. These data are made publicly available by the FBI and accessed through the Inter-University Consortium for Political and Social Research at the University of Michigan.

The independent variables in my analysis are whether a county has implemented a 287(g) partnership or Secure Communities program (or both). Immigration and Customs Enforcement (ICE) released 287(g) data through a series of Freedom of Information Act (FOIA) requests obtained by Matthew Hall of Cornell University. These data include both a binary variable indicating whether an agreement is activated in a given county and year as well as counts of persons identified for removal by ICE and those who were detained through the program. The majority of agreements were through county sheriff's offices, although several were enacted on the municipal level (at which point the implementation status was applied to the whole county). These data also include an indicator for whether a given county borders a county that implemented 278(g). This variable was added to the panel file in order to control for spillover effects of the program and is used in the model as a covariate.

Additionally, FOIA requests include information on counties that submitted an application to enter into a 287(g) partnership but withdrew or were rejected by the Department of Homeland Security. These observations are essential for my analysis as they serve as a control group to compare the treated counties to. A primary concern in any treatment model is that the counties that were treated are fundamentally different from those that were not treated, causing bias in the model. The theory behind this choice of control group is that it eliminates much of this bias. We can assume that counties that applied for the program and were rejected are in fact more similar to the treated counties than those that did not apply at all on a variety of unobservable characteristics. Using this control group strengthens the robustness of our model. Information on the reason for rejection was not included, but media reports suggest it is likely due to lack of county resources to implement the program effectively.

Secure Communities data come from the Immigration and Customs Enforcement division of the Department of Homeland Security. Data were obtained through Catalina Amuedo-Doranrtes of the Department of Economics at San Diego State University. Overall, adoption of Secure Communities was at the county level, yet some state corrections departments or sheriff's offices adopted the policy on their own. These non-county adoptions were dropped from the panel data set. The ICE data provides specific dates of adoption for each county in the United States.

To account for differences between counties, demographic data was added to the panel file. Demographic data was obtained from the Census Bureau (largely from the 2000 decennial Census and the American Community Survey) and includes information on racial composition, median household income, annual population counts, unemployment rates and industry employment information. One central variable to this analysis is the estimate of the unauthorized immigrant population in each county. These data were compiled based on the data fusion approach developed in Capps et. al (2013) (see Rugh and Hall 2016). (These data are only available for the large counties (about two thirds of the total sample). Primary analyzes were rerun to ensure that this sampling did not bias the estimates and results were identical to the estimates presented in the paper.) The total core sample consists of 3,163 counties across 9 years, yielding a sample size of 28,740.

Tables 1 through 4 show characteristics of all the counties in the United States broken down by program treatment for both 287(g) and Secure Communities. Tables 1 and 2 show that counties that were approved for the 287(g) partnership were on average larger, had more foreign born residents, more Hispanics, a greater proportion of unauthorized immigrants, lower unemployment rates, higher median household income and higher crime rates than other counties in the dataset. This suggests that 287(g) treatment was not randomly assigned, and we must therefore be cautious in assigning a causal interpretation to the results presented below. While most characteristics in the demographic panel file are more similar between the approved and never-applied counties, White-Hispanic dissimilarity, unemployment rate and median household income are more similar in approved and denied counties. These variables may in fact be more potent predictors of crime as income-related struggles and racial tensions have been shown in the literature to be highly associated with crime (Maume et al. 2010).

The final row of Table 2 estimates the predicted value of log crime rates based on the covariates outlined in Table 1. These results suggest that the never-applied and approved counties are overall observably more similar to each other than the approved and denied counties. While these estimates suggest that the never-applied counties may serve as a stronger control group based on observables, there could be many unobservable characteristics such as policing practices and political affiliations that impact crime rates that would likely be more similar for counties that applied for the 287(g) program. Given that these characteristics are difficult to detect in standard demographic data, I will report results from both the broad sample of all counties in the United States and the subsample of only counties that applied for a 287(g) partnership throughout the analysis.

Table 4 shows that counties that adopted Secure Communities prior to 2011 were similarly distinct from counties that adopted the program later on the same set of characteristics. These differences are to be expected, as the DHS was responsive to counties that were eager to implement immigration policy.

Empirical Model Specification

The first stage of my analysis leverages variation in timing and location of 287(g) agreements to assess the effect of deportation-focused immigration policy on crime rates. The estimating equation takes the form:

$$lnC_{it} = Activate(SC)_{it}\delta + Activate(287)_{it}\Gamma + X_{it}\beta + \alpha_i + \alpha_t + \varepsilon_{it}, \tag{1}$$

where lnC_{it} is the natural log of the crime rate in county i at calendar year t. The term $Activate(SC)_{it}$ is a binary variable indicating whether Secure Communities was adopted and $Activate(287)_{it}$ is a binary variable representing whether a 287(g) partnership was implemented (applied and accepted) in county i on date t. X_{it} represents the demographic covariates discussed above including total and unauthorized populations, racial composition, employment statistics, income information and whether the county bordered a 287(g) county. The control group used for the 287(g) analysis is the set of counties that applied for a 287(g) partnership and were rejected, while for Secure Communities the early adopter counties serve as a control group for the later adopter counties. α_i and α_t are county and year fixed effects and the ε_{it} term represents the error in the regression. $_t\delta$ and Γ measure the effects of the agreements on crime in the baseline model of (1).

The second stage of my analysis includes an interaction term between the two programs to assess the effect of both Secure Communities and 287(g) being in implemented in a county. This equation mirrors the first in form, but includes the interaction term. The basic model is as follows:

$$lnC_{it} = Activate(SC)_{it}\delta + Activate(287)_{it}\Gamma + Activate(SC+287)_{it}\theta + X_{it}\beta + \alpha_i + \alpha_t + \varepsilon_{it}, \quad (2)$$

where $Activate(SC+287)_{it}\theta$ yields the effect of both programs being in place simultaneously in county i at date t and all other variables remain the same. This regression allows me to evaluate the difference in crime trends for counties that implemented one or both of the programs. In the third stage of the analysis I analyze various subgroups in order to gauge the heterogeneity in the effects of the programs on crime. All regressions are weighted by the county population and standard errors are clustered at the state-level.

Results

Estimates of Total Index Crime Rate

Panel A of Table 5 reports initial difference-in-difference estimates for the log of total index crime rates for both Secure Communities and 287(g). Regression Specification A column 1 shows a simple difference-in-difference estimate indicating that Secure Communities decreased crime by 3.24% while 287(g) increased crime by 4.92%. These coefficients are significant at the 90% level and suggest that the programs may be having a small impact on crime rates. The estimate of the effect of Secure Communities is very similar Miles and Cox's baseline finding of a 4% reduction in crime due to the program's implementation. As we would expect these programs to decrease crime rates, the positive sign on the 287(g) coefficient in this regression is particularly surprising.

While 287(g) was only implemented in a select set of counties across the nation, by the year 2013 Secure Communities was universally implemented across every county in the United States. This means that all counties with 287(g) also saw the introduction of Secure Communities before 2013. While I am interested in the effect of both programs individually, here I investigate the effect of having both programs in place simultaneously. While we might expect that an increase in enforcement of immigration policy would have diminishing marginal returns, the estimates on the program interaction variables suggest otherwise.

Regression 2 includes a program interaction variable that looks at the effect of having both Secure Communities and 287(g) in place simultaneously in a given county and year. For all regressions that include an interaction term, the coefficient on the interaction between the two programs tends to be negative (and statistically insignificant), suggesting that having the two programs in place may have increasing marginal returns in reducing crime. Table 5 Regression 2 shows that including the program interaction term drives the coefficient on the both programs down to nearly zero at -0.0073. This is a relatively similar estimate to Miles and Cox who found a .0025 baseline estimate when including county time trends in their regression.

Regression Specification B of Table 5 reports a subsample analysis of the effects of these programs. These regressions use a sample of only counties that applied for a 287(g) partnership and leverages whether they were approved for the program by the Department of Homeland Security or not as the treatment. This subsample reduces selection bias in the model by comparing counties that were both interested in implementing the program. The first regression column shows the impact of the two programs on crime rates. Both coefficients are small in magnitude and not statistically significant suggesting that for this subsample the programs were largely ineffective. While the Secure Communities estimate does not change much from the previous regressions (-.0324 to -.0223), the 287(g) estimate drastically drops from .0492 to .0024. Using the denied counties as a control group allows us to better estimate the effect of the 287(g) program by eliminating much of the selection bias in implementation. These estimates presented in Specification B serve as the most robust in this analysis and are the basis for concluding that neither program had substantial effects on crime rates.

While I am interested in analyzing the effect of treatment for the subsample of counties that applied for 287(g) throughout the following regressions, given the small sample size, the standard errors remain large throughout and the results are therefore difficult to interpret. As such the following analyses will be conducted using the entire sample of counties.

Estimates of Violent and Property Crime

While immigration policy focuses on overall crime, much of the rhetoric about immigrants and crime hones in on violent offenses. Table 6 provides estimates for the effect of the two programs on violent and property crimes. For the most part, these estimates mirror the previous table’s estimates in sign and magnitude yet the 287(g) estimates seem overall larger than for total index crime rates. For most regressions, the changes in violent crime rates were more substantial than that of property crime. Specifications A of Table 6 suggest that Secure Communities was effective at targeting violent offenders and slightly less effective when it came to property crime. In contrast, 287(g) generated large *increases* in violent crime rates while having little effect on property crime. Including the program interaction variable suggests that the two programs together reduced violent crime by 3% but increased property crime by 0.42%, a number so small in magnitude that we can argue there was no substantial effect.

The estimates on violent and property crime rates converge more in the sample of counties that applied for 287(g) partnerships. For these counties, the two programs in unison caused a 9.2% reduction in violent crime and a 4.27% reduction in property crime. While most of these estimates are not statistically significant their magnitude suggests that the programs may have a strong impact on crime rates when combined.

Subsample Analyses: Unauthorized Population and Adoption Timing

Programs like 287(g) and Secure Communities rely on a substantial population of unauthorized immigrants in order to have an effect on crime reduction. If an individual is arrested and does have legal documentation status the authorities running the programs have no authority to detain him/her on the grounds of immigration status. We might expect that counties with high levels of unauthorized immigrants are already more vigilant to issues of immigration and might initially have higher levels of ICE involvement with local authorities. It follows that they would be more prepared to implement Secure Communities and 287(g) and might therefore see more significant returns to the programs as a result of their efficiency and vigilance. Table 7 examines the differences in how effective the programs were at reducing crime in counties with high proportions of unauthorized immigrants relative to those with low proportions.

Table 7 differentiates between counties that were above and below the median of the fraction of the population of unauthorized immigrants. Estimates for Secure Communities, 287(g) and the interaction between the programs are larger in magnitude for counties below the median than those above. This suggests that the programs had larger impacts in counties with lower unauthorized immigrant populations, counter to my hypothesis. For counties with low unauthorized counts, Secure Communities decreased crime by 3.54% while 287(g) increased crime by 9.58% and the two programs together combined for an increase in crime by 7.54%. In contrast, counties above the median saw very limited effects. For such counties, Secure Communities decreased crime by 2.37% and 287(g) increased crime by 2.39%. These estimates show that the programs were much stronger in counties with low levels of unauthorized immigrants and that these effects overall increased crime[1].

The second subsample analysis distinguishes between counties that implemented Secure Communities prior to 2011 (the median implementation year) and those that implemented it after 2011. While Secure Communities was enforced throughout the nation by 2013, the rollout of the program was staggered, with some counties adopting it as early as 2008 and others as late as 2013. It is possible that early

[1] Unauthorized counts are only available for large counties resulting in approximately 2/3 of U.S. counties being dropped in this analysis. The main regressions in Table 5 were replicated with this subsample to ensure that the remaining counties were representative of the broad sample and the coefficients yielded were identical to those presented in Table 5.

adopter counties were different from late adopter counties on many dimensions. For example, it's possible that the counties that adopted Secure Communities prior to 2011 were more eager to implement immigration enforcement policy and were therefore more prepared to put such a program in place. Therefore we might expect that early adopter counties would see a greater reduction in crime from both Secure Communities and 287(g).

These estimates show, however, that the late adopter counties experienced much stronger effects due the programs. Table 8 shows the impact of the programs on early and late adopter counties on three dimensions: total index crime rate, violent crime rate and property crime rate. Following the trend of previous estimates Secure Communities minimally reduced crime while 287(g) minimally increased crime in early adopter counties. In contrast, the magnitude of the estimates for late adopter counties is substantially larger. At most, Secure Communities reduced crime by 9.18% and 287(g) increased crime by 15%. The effects of both programs on total index crime rate, violent and property crime rate are significant for late adopter counties. It is possible that this effect is due to increased efficiency over time or economies of scale. As the programs get larger, the Department of Homeland Security perfects their training programs and materials and trains county sheriffs more effectively for 287(g) and improves the speed and efficiency of their technology and contact with local authorities for Secure Communities. Such changes in the programs could lead to a stronger effect over time, as both the federal government and local county authorities ran the programs more smoothly.

Discussion

Despite differences in the magnitudes of my estimates, the effect I find from Secure Communities is generally congruent with Miles and Cox's findings that Secure Communities had no meaningful impact on total index crime rates. Using the sample of counties that applied for the 287(g) program, I highlight the null effects of 287(g) in reducing crime rates, suggesting that both programs did not achieve their central objective in reducing crime and making American communities safer. The estimates presented in this paper suggest that despite data limitations, my conclusions are in line with Miles and Cox's findings and with access to similarly precise data the estimates would likely converge more in magnitude.

While the main findings suggest that 287(g) was not associated with any significant changes in crime rates, the estimates of the effect of 287(g) on crime rates using the broad sample are almost all positive. This suggests that the program may have had an unusual and unanticipated effect. While Miles and Cox do incorporate 287(g) in their regressions and emphasize it as an important covariate, they choose not to explicitly report on its effects on crime rates. One major contribution of this paper is the focus on 287(g) as an important policy variation and the significance of its interaction with the Secure Communities program.

In addition to focusing on the effects of 287(g), I was able to isolate a subsample that yields more precise causal estimates of the impact of the program. As 287(g) functioned as an opt-in program, meaning that counties could voluntarily apply for the partnership, we can conclude that counties that opted in for 287(g) were fundamentally different from those that did not on a series of unobservable characteristics. The type of county that would choose to implement an immigration enforcement policy is likely very different from one that does not choose to. While the basic demographics are similar between the never-applied and denied counties (see Table 1), it is likely that the political atmosphere and the policing strategies converge more for counties that applied for the program. While these variables are largely unobservable, they are important and must be addressed to the best of our ability in order to estimate a causal treatment effect. Thus, the variation in approval status likely allows me to better isolate the effect of treatment into the 287(g) approved group more thoroughly, reducing the bias of the model. This subsample analysis provides opportunity for a more precise estimate of the effect of the 287(g) program than previously presented in the literature.

While Secure Communities has been shown to be ineffective at reducing crime, 287(g) seems to have increased crime in certain specifications. This may initially seem to indicate that because of the program, people commit more crimes. I would like to suggest otherwise. In looking at the differences in the institutional details between the two programs, it seems as though the discrepancy in efficacy may be due to their inherent differences in implementation. One possible explanation of the positive sign on the 287(g) coefficients is as follows.

The 287(g) program functions by appointing and training officers in designated counties to serve as local immigration enforcement officers. Once approved for the program, local authorities select a candidate to attend the ICE training. Upon return, this individual is given the authority to check for immigration status and detain unauthorized offenders accordingly. In contrast, Secure Communities does not empower officers but rather implements a technology that enables local authorities to check for immigration status by fingerprinting a suspect upon their arrival at the local jail. In the case of 287(g), the empowerment of specific officers tasked with the obligation to reduce crime committed by immigrant offenders could incentivize officers to become more vigilant and active in their pursuit of criminal activity. As such, it is possible that the power and responsibility encourages these newly deputized officers to be more aggressive in their roles. This, in turn, could drive up the crime rates simply by having officers who are more effective in catching crime. Given that for Secure Communities, there is no individual increase in authority, it seems logical that such an effect would not be generated for this program. This narrative suggests that crime rates may in fact not be changing, but it is crime detection that is driving the magnitude and sign of the 287(g) coefficients found in this analysis.

In order to assess this claim I have analyzed several subsamples. While the effects of the programs on counties with high and low levels of unauthorized immigrants seemed initially surprising, with this narrative these findings fall more into line. As discussed previously, 287(g) seems to increase crime much more substantially for counties below the median of unauthorized immigrant population than for those above it. We can expect that if an officer becomes more vigilant and detects more crimes, some of those crimes would likely be committed by unauthorized immigrants. This should therefore reduce the crime rate as those offenders could be detained and eventually deported, ultimately reducing the number of criminals in the county. This could therefore mitigate the overall increase in crime rates due to detection, driving the effect closer to zero. However, if the county has very few unauthorized immigrants, the increase in crime detection will likely pick up very few unauthorized offenders (especially given the low probability of criminal activity in the immigrant communities). Therefore, we would expect that crime should increase at a higher rate in counties with lower levels of unauthorized immigrants. This is in fact what we see, with a statistically significant effect of 9.58% on 287(g) in low unauthorized immigrant counties and a non-significant effect of 2.39% in counties with high levels of unauthorized immigrants.

Further, we might expect that this increase of crime detection would not have equivalent effects across various types of crimes. For example, it seems unlikely that these newly deputized officers will become more vigilant to high profile crimes such as murder and rape. With some faith in local authorities we can expect that these crimes were already being detected prior to the implementation of 287(g), and would therefore not expect the program to change such crime rates. In contrast, it seems logical that the new detections could be crimes that previously went under the radar, possibly more petty crimes such as larceny or burglary.

In order for this narrative to ring true, it seems as though violent crime rates must not see as large of an increase as property crime rates. For every subsample analyzed violent crime in fact increased at a higher rate than property crime, suggesting that this theory may be inaccurate. In order to analyze this question more comprehensively, I display results of the programs on specific crimes: murder, rape, robbery, aggravated assault (violent crimes) and burglary, larceny, motor vehicle theft and arson (property crimes). Tables 9 and 10 present these results. While more variation of the effect of 287(g) exists between specific crimes, the findings still are not fully congruent with this narrative. For example, it seems incomprehensible that detection of murder should increase by 10% after the implementation of this program but that detection of burglary should only increase by 5%. Despite some evidence to suggest that the increase in crime rates due to 287(g) is due to better crime detection due to increased vigilance and action on the part of the officers, this narrative is not fully backed up by the findings outlined in this paper.

However, it is possible that rather than picking up hardened criminals, the 287(g) program is instead identifying and detaining hard working, unauthorized individuals. If those being picked up by the program are in fact the breadwinners of local families it is possible that the program's implementation tears families apart resulting in a breakdown in family structure and missing parents. This type of family breakdown is associated with many negative outcomes in the literature. In particular, parental separation is associated with poor child outcomes for minority children (Mackay, 2005). Additionally, Brabeck and Xu (2010) found that exposure to deportation is associated with individual's concerns their about ability to provide financially for their families and negatively affects children's school performance. Zinsmeister (1990) reports evidence that family breakdown is the most important source of violence by and among children. Tearing families apart through 287(g) may in fact be a mechanism for increasing crime. The program's implementation is likely making families more desperate, resulting in more dangerous and crime-ridden neighborhoods.

In addition to carefully analyzing 287(g) estimates as well as Secure Communities, this paper also contributes to the existing literature by examining the effect of the two programs running in unison in a given county and year. Given that all 287(g) counties were also eventually counties that implemented Secure Communities, focusing on the interaction between the programs provides another layer of detail to estimating the returns to immigration enforcement policy. While some interaction terms remain small, they range from close to zero to very large in magnitude. These estimates suggest that it may be important to consider the interaction between programs in order to fully understand the strength of immigration enforcement policy.

Limitations and Future Directions

In this stage of the analysis it is important to recall the data limitations and difficulties in identifying a causal treatment effect. The largest statistically significant effects of the 287(g) program estimate around a 14% increase in index crime rates. The lack of precision in crime data and small sample size of 287(g) counties provide a limitation for identifying a causal effect. Miles and Cox's model relies on including county-specific time trends in their regressions to control for exogenous changes in crime rates over time in a given county. This enhances their analysis quite dramatically by allowing for the counties to trend in a linear fashion over time. They are able to control for these trends because their UCR crime data is in monthly rather than annual form, giving them sufficient data before the treatment begins in order to capture county-specific trends. By including county-specific trends they are able to control for trends in specific counties that affect crime rates that are unrelated to the policy. Without using such trends, it is difficult to disentangle the causal effect of the policy from underlying crime trends in the county. Replication with more detailed UCR data would allow me to include county-specific trends and would likely improve the analysis. Without the monthly UCR estimates, the results presented in this paper on Secure Communities deviate from Miles and Cox's despite overall similarities in the conclusions drawn from the resulting coefficients.

In regards to the precision of the 287(g) estimates, another concern for identifying causality arises. While the subsample analysis of only counties that applied for a 287(g) partnership is certainly an improvement on simply comparing treated counties to counties that didn't apply for 287(g), this model is still likely biased. While data has not been released by ICE regarding why some counties were approved and others were denied for the program, it seems unlikely that the Department of Homeland Security's selection was random. This suggests that counties that were approved were likely fundamentally different from those that applied, reducing our ability to claim a causal treatment effect.

In further exploring the effect of 287(g), I would be interested in distinguishing between different variants of the program and different degrees of implementation. Three subtypes of 287(g) were implemented: jail enforcement, task force and hybrid. These variants functioned slightly differently and it would be interesting to assess the effects of the different subtypes on crime rates in order to gain more insights into the mechanisms driving the effects. Evidence from the Migration Policy Institute suggests that the program's implementation varied not only in 287(g) program type but in intensity as well. As 287(g) was not implemented in uniform form and intensity across counties, I would be interested in replicating this analysis but excluding notoriously tough-on-immigration counties such as Maricopa County, Arizona to determine whether these counties are driving the effects of the programs.

While this analysis would be strengthened by including an event study analysis to determine the effects of the programs on crime rates over time, the relatively few data periods coupled with two sets of event dummy variables – one set for each program – result in the omission of key variables due to multicollinearity. As such, the event study was not included in this version of the paper. Future versions will be sure to include estimates of crime rates in the years before and after implementation to assess the strength of the baseline estimates over time.

While this paper does identify new strategies to identifying a causal impact, and recommends focusing on 287(g) as an important policy treatment, the analysis presented would also benefit from replication with more precise crime data. Given that it seems that crime detection rates may be playing a large role in the effects of these programs, I would recommend including data from the National Crime Victimization Survey. This survey consists of data from a nationally representative sample on victimization rates. Analyzing the discrepancy between victimization rates and rates of documented offenses may provide insight into changes in crime reporting and detection as a result of these immigration enforcement policies. Evidence from the Migration Policy Institute's report on the effects of 287(g) suggests that in counties where 287(g) was implemented, immigrants were less likely to venture into public spaces, interact with the police, report crimes, interact with local schools and patronize local businesses. Using the NCVS data would allow us to analyze changes in crime detection and reporting, and determine whether FBI generated crime rates diverge from victimization reports with the implementation of 287(g) and Secure Communities. This would greatly enhance the analysis and would provide further evidence into the mechanisms through which these programs operate.

Table 1.
287(g) Summary Statistics

Variable	Mean	Standard Deviation
287(g) Agreement Enforcement		
Activated?	.012	.110
Denied?	.677	.468
Identification*	742	1,758
Detainment*	702	1,266

	Approved counties n=54		Denied counties n=113		Never-applied counties n=3,179	
	Mean	SD	Mean	SD	Mean	SD
County population (thousands)	3,494	3,541	573	374	847	1,021
Fraction Foreign Born	.222	.101	.105	.057	.133	.113
Fraction Hispanic	.293	.154	.096	.084	.143	.160
Fraction Unauthorized Immigrant	.085	.037	.033	.022	.039	.031
White-Hispanic Dissimilarity	.596	.092	.625	.100	.694	.124
Unemployment Rate	4.445	.948	4.67	1.152	5.500	1.827
Median Household Income (dollars)	46,257	10,817	44,529	11,869	34,787	8,370

All demographic data are from the year 2000 (aside from unauthorized immigrant count which is extracted from the 2005 ACS)
*Counts only available for 287(g) activated counties

Table 2.
287(g) Outcome Variable Summary Statistics

	Approved counties n=54				Denied counties n=113				Never-applied counties n=3,179			
	Mean	SD	Min	Max	Mean	SD	Min	Max	Mean	SD	Min	Max
Crime count	33,937	57,797	466	377,855	8,032	10,602	0	73,527	2,651	8,946	0	211,649
Crime rate (CR) *	4,082	1,582	908	8,518	3,342	1,489	0	8,317	3,736	1,657	0	14,918
Violent CR*	485	282	45	1,655	362	234	0	1,182	395	277	0	2,578
Property CR*	3,596	1,358	860	7,151	2,980	1,300	0	7,193	3,341	1,452	0	12,340
Predicted log CR*ƚ	8.203	.325	7.407	9.010	8.019	.3564	7.244	8.691	8.178	.290	7.163	9.099

*Crime rates are crime counts per 100,000 people. ƚ Predicted log crime rates are generated from regression on 2004 crime rate data.

Table 3.
Secure Communities Summary Statistics

Variable	Mean	Standard Deviation
Secure Communities Activated?	.2920	.4547
County population	1,042,393	1,869,944
Fraction Foreign Born	.1103	.1082
Fraction Hispanic	.1256	.1509
Fraction Unauthorized Immigrant	.0492	.0557
White-Hispanic Dissimilarity	.6893	.1239
Unemployment Rate	4.3773	1.6944
Median Household Income	35,332	8,874

Notes: N =31,467. Observations are annual, county-level data from 2004-2012. Number of counties in sample=3,148. Weighted by population. All demographic data are extracted from Census 2000.

Table 4.
Secure Communities Summary Statistics

Variable	Mean		Standard Deviation	
Secure Communities Activated?	.2920		.4547	
	Early Adopter Counties (n=17,910)		Late Adopter Counties (n=10,288)	
	Mean	SD	Mean	SD
County population	1,102,330	2,028,364	888,953	1,372,333
Fraction Foreign Born	.1119	.1062	.1063	.1131
Fraction Hispanic	.1418	.1637	.0841	.1002
Fraction Unauthorized Immigrant	.0540	.0620	.0368	.0311
White-Hispanic Dissimilarity	.6740	.1236	.7218	.1181
Unemployment Rate	4.4101	1.7002	4.3202	1.6827
Median Household Income	35,608	8,858	34,850	8,880

Notes: N =31,467. Observations are annual, county-level data from 2004-2012. Number of counties in sample=3,148. Weighted by population. All demographic data are extracted from Census 2000. Early adoption counties implemented SComm prior to 2011 and late adoption counties implemented SComm after 2011 (the mean program implementation year). Observation counts (n) for White-Hispanic dissimilarity and fraction unauthorized immigrant differ from other variables as county-level data were only available for larger counties.

Table 5.
Impact of Secure Communities and 287(g) on Rate of Total Index Crime:
OLS Regression Estimates

Panel A. Difference-in-Difference Estimates

	Specification of Dependent Variable	
	Log Levels	Log Levels
Explanatory Variable	(1)	(2)
Regression Specification A		
Secure Communities Activated	-.0324*	-.0277
	(.0177)	(.0188)
287(g) Activated	.0492*	.0671**
	(.0271)	(.0314)
Program Interaction Variable		-.0467
		(.0303)
Regression Specification B		
For sample of 287(g) Applied Counties (n=1,002)		
Secure Communities Activated	-.0223	.0376
	(.0469)	(.0795)
287(g) Activated	.0024	.0364
	(.0466)	(.0322)
Program Interaction Variable		-.1223
		(.0808)
County Fixed Effects	Y	Y
Year Fixed Effects	Y	Y
Program Interaction Variable	N	Y

Notes: **p<0.05, *p<0.1. The dependent variable is the log of annual index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 31,467. Number of counties = 3,148. Number of counties that implemented 287(g)= 186. All counties implemented Secure Communities.

Table 6.
Impact of Secure Communities and 287(g) on Crime Rate by Type:
OLS Regression Estimates

Panel B. Violent and Property Offenses – Log Levels

Specification of Dependent Variable	Violent Crime		Property Crime	
Explanatory Variable	(1)	(2)	(3)	(4)
Regression Specification A				
Secure Communities Activated	-.0488	-.0363	-.0306*	-.0268
	(.0328)	(.0351)	(.0176)	(.0186)
287(g) Activated	.0840**	.1320***	.0462	.0606*
	(.0316)	(.0404)	(.0275)	(.0314)
Program Interaction Variable		-.1253***		-.0380
		(.0458)		(.0304)
Regression Specification B For sample of 287(g) Applied Counties (n=1,002)				
Secure Communities Activated	-.1000	-.0376	-.0140	.0449
	(.0629)	(.0974)	(.0460)	(.0781)
287(g) Activated	.0374	.0728	-.0008	.0326
	(.0542)	(.0460)	(.0466)	(.0320)
Program Interaction Variable		-.1273		-.1202
		(.0896)		(.0802)
County Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Program Interaction Variable	N	Y	N	Y

Notes: ***p<0.001, **p<0.05, *p<0.1. The dependent variable is the log of annual crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 31,467. Number of counties = 3,148. Number of counties that implemented 287(g)= 186. All counties implemented Secure Communities.

Table 7.

Impact of Secure Communities and 287(g) on Rate of Total Index Crime:
OLS Regression Estimates

Difference-in-Difference Estimates

Specification of Dependent Variable			
Log Levels	Log Levels		
Explanatory Variable		(1)	(2)
For Counties Below Median of Fraction Pop. Unauthorized Immigrant (n= 28,333)			
Secure Communities		-.0354*	-.0360*
(.0181)	(.0182)		
287(g)		.0958*	.0892*
(.0481)	(.0512)		
Program Interaction Variable			.0222
(.0314)			
For Counties Above Median of Fraction Pop. Unauthorized Immigrant (n= 28,333)			
Secure Communities		-.0237	.0169
(.0216)	(.0233)		
287(g)		.0239	.0405
(.0326)	(.0336)		
Program Interaction Variable			-.0428
(.0318)			
County Fixed Effects		Y	Y
Year Fixed Effects		Y	Y
Program Interaction Variable		N	Y

Notes: *p<0.1. The dependent variable is the log of annual index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 31,467. Number of counties = 3,148. Number of counties that implemented 287(g)= 186. All counties implemented Secure Communities.

Table 8.
Impact of Secure Communities and 287(g) on Crime Rates: Program Adoption Time
OLS Regression Estimates

Difference-in-Difference Estimates

<u>Specification of Dependent Variable</u>	Index Crime	Violent Crime	Property Crime
Explanatory Variable	(1)	(2)	(3)
Early Adopter Counties (n= 28,333)			
Secure Communities	-.0165	-.0365	-.0134
	(.0183)	(.0334)	(.0192)
287(g)	.0351	.0829***	.0249
	(.0235)	(.0274)	(.0358)
Late Adopter Counties (n= 28,333)			
Secure Communities	-.0851**	-.0510**	-.0918**
	(.0318)	(.0187)	(.0378)
287(g)	.1360***	.1486*	.1501***
	(.0385)	(.0781)	(.0386)
County Fixed Effects		Y	Y
Year Fixed Effects		Y	Y
Program Interaction Variable		N	N

Notes: *p<0.1. The dependent variable is the log of annual index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 31,467. Number of counties = 3,148. Number of counties that implemented 287(g)= 186. All counties implemented Secure Communities.

Table 9.
Impact of Secure Communities and 287(g) on Specific Offenses
OLS Regression Estimates

Panel A. Violent Offenses – Log Levels

	Specification of Dependent Variable			
	Murder	Rape	Robbery	Aggravated Assault
Explanatory Variable	(1)	(2)	(3)	(4)
Secure Communities	-.0265	-.0293	-.0478	-.0408
	(.0323)	(.0370)	(.0408)	(.0354)
287(g)	.1023	.0593**	.1329***	.0679*
	(.0724)	(.0288)	(.0486)	(.0367)
County Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Program Interaction Variable	N	N	N	N

Notes: *p<0.1. The dependent variable is the log of annual index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 31,467. Number of counties = 3,148. Number of counties that implemented 287(g)= 186. All counties implemented Secure Communities.

Table 10.
Impact of Secure Communities and 287(g) on Specific Offenses
OLS Regression Estimates

Panel B. Property Offenses – Log Levels

	Specification of Dependent Variable			
	Burglary	Larceny	Motor Vehicle Theft	Arson
Explanatory Variable	(1)	(2)	(3)	(4)
Secure Communities	-.0288	-.0298*	-.0256	-.0169
	(.0256)	(.0158)	(.0241)	(.0408)
287(g)	.0575*	.0449*	.0740	.1443**
	(.0293)	(.0266)	(.0519)	(.0563)
County Fixed Effects	Y	Y	Y	Y
Year Fixed Effects	Y	Y	Y	Y
Program Interaction Variable	N	N	N	N

Notes: *p<0.1. The dependent variable is the log of annual index crime rate. The table reports regression coefficients, with standard errors in parentheses. N = 31,467. Number of counties = 3,148. Number of counties that implemented 287(g)= 186. All counties implemented Secure Communities.

Works Cited

Adelman, R., et al. (2017) "Urban crime rates and the changing face of immigration: Evidence across four decades." *Journal of ethnicity in criminal justice* 15.1 52-77.

Brabeck, K., & Xu, Q. (2010). The Impact of Detention and Deportation on Latino Immigrant Children and Families: A Quantitative Exploration. *Hispanic Journal of Behavioral Sciences, 32*(3), 341-361. doi:10.1177/0739986310374053

Bump, P.. (2015) “Where America’s unauthorized immigrants work.” The Washington Post, WP Company, www.washingtonpost.com/news/the-fix/wp/2015/03/27/where-americas-unauthorized-immigrants-work/?utm_term=.2ce8dc2ac35f. Accessed 22 Aug. 2017.

Butcher, K. F., & Piehl, A. M. (1998). Cross- city evidence on the relationship between immigration and crime. *Journal of Policy Analysis and Management,17*(3), 457-493.

Butcher, K., & Piehl, A. M. (2007). Why are Immigrants Incarceration Rates so Low? Evidence on Selective Immigration, Deterrence, and Deportation. *National Bureau of Economic Research*.

Capps, R., Rosenblum, M. R., Chishti, M., & Rodriguez, C. (2011). Delegation and Divergence: 287(g) State and Local Immigration Enforcement. *Migration Policy Institute*.

Chavez, L., Cornelius, W. & Jones, O. (2010). Utilization of Health Services by Mexican Immigrants in San Diego. *Journal of Women and Health, 2(11*), 3-20.

Davis, R., Erez, E. & Avitabile, N. (2001). Access to Justice for Immigrants Who are Victimized: The Perspectives of Police and Prosecutors. *Criminal Justice Policy Review. 3(12),* 183-196.

Foley, E. (2012, August 23). Secure Communities Costs Los Angeles County More Than $26 Million A Year: Report. Retrieved March 02, 2018, from https://www.huffingtonpost.com/2012/08/23/secure-communities-los-angeles_n_1824740.html

Hagan, J., & Palloni, A. (1999). Sociological Criminology and the Mythology of Hispanic Immigration and Crime. *Social Problems, 46*(4), 617-632.

Heckman, J. J., & Honore, B. E. (1990). The Empirical Content of the Roy Model. *Econometrica, 58*(5), 1121

Kelly, J. (2017, February 20). Implementing the President's Border Security and Immigration Enforcement Improvements Policies. Retrieved from https://www.dhs.gov/sites/default/files/publications/17_0220_S1_Implementing-the-Presidents-Border- Security-Immigration-Enforcement-Improvement-Policies.pdf

Krikorian, M.(2004) “Keeping Terror Out: Immigration Policy and Asymmetric Warfare.” *The National Interest*, no. 75 , pp. 77–85. *JSTOR*, www.jstor.org/stable/42897528.

Mackay, R. (2005). The Impact of Family Structure and Family Change on Child Outcomes: A Personal Reading of the Literature. *Social Policy Journal of New Zealand* , (24).

Martinez, R., & Stowell, J. I. (2012). Extending Immigration and Crime Studies. *The ANNALS of the American Academy of Political and Social Science, 641*(1), 174-191.

Maume, M. O., Kim-Godwin, Y. S., & Clements, C. M. (2010). Racial Tensions and School Crime. *Journal of Contemporary Criminal Justice, 26*(3), 339-358. doi:10.1177/1043986210369283

McDonald, W. F. (n.d.). Crime and Illegal Immigration: Emerging Local, State, and Federal Partnerships. Retrieved August 02, 2017, from https://www.ncjrs.gov/app/abstractdb/AbstractDBDetails.aspx?id=184605

Merton, R. K. (1938) "Social structure and anomie." *American sociological review* 3.5: 672-682.

Miles, T. J., and A. B. Cox. (2014) "Does Immigration Enforcement Reduce Crime? Evidence from Secure Communities." *The Journal of Law & Economics*, vol. 57, no. 4, pp. 937–973. *JSTOR*, www.jstor.org/stable/10.1086/680935.

Rugh, J., & Hall, M. (2016). Deporting the American Dream: Immigration Enforcement and Latino Foreclosures. *Sociological Science, 3*, 1077-1102. doi:10.15195/v3.a46

Shaw, C. R, and McKay, H. D. (1942) "Juvenile delinquency and urban areas."

Transcript: Donald Trump's full immigration speech, annotated. (2016, August 31). Retrieved August 02, 2017, from http://www.latimes.com/politics/la-na-pol-donald-trump-immigration-speech-transcript- 20160831-snap-htmlstory.html

Uggen, C. (2012). Crime and the Great Recession: a Great Recession Brief. *The Russell Sage Foundation and The Stanford Center on Poverty and Inequality*.

U.S. Immigration and Customs Enforcement: Delegation of Immigration Authority Section 287(g) Immigration and Nationality Act. (n.d.). Retrieved August 02, 2017, from https://www.ice.gov/factsheets/287g

U.S. Immigration and Customs Enforcement: Secure Communities. (n.d.). Retrieved August 03, 2017, from https://www.ice.gov/secure-communities

Wang, C. (2015, April 26). What's Next for Arizona's SB 1070 and Other Copycat Laws. Retrieved August 02, 2017, from https://www.aclu.org/blog/whats-next-arizonas-sb-1070-and-other- copycat-laws

Wang, M., Holahan, J. (2003) The Decline in Medicaid Use by Noncitizens since Welfare Reform. Urban Institute: Health Policy Online.

Wilson, J. Q. (2011, May 28). Hard Times, Fewer Crimes. Retrieved March 11, 2018, from https://www.wsj.com/articles/SB10001424052702304066504576345553135009870?mod=WSJ_hp_LEFTTopStories

Zinmeister, K. (1990). Growing Up Scared. *Atlantic, 265*(6), 49-66.