THE IMPACT OF IMMIGRATION POLICIES ON LOCAL ENFORCEMENT, CRIME AND POLICING EFFICIENCY

Alberto Ciancio

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Supervisor of Dissertation	Co-Supervisor of Dissertation
Petra Todd, Edmund J. and Louise W. Kahn Term Professor of Economics	Camilo Garcia-Jimeno, Assistant Professor of Economics

Graduate Group Chairperson

Jesus Fernandez-Villaverde, Professor of Economics

Dissertation Committee:
Petra Todd, Edmund J. and Louise W. Kahn Term Professor of Economics
Camilo Garcia-Jimeno, Assistant Professor of Economics
Holger Sieg, The J.M. Cohen Term Professor of Economics

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To my parents

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ABSTRACT

THE IMPACT OF IMMIGRATION POLICIES ON LOCAL ENFORCEMENT, CRIME AND POLICING EFFICIENCY

Alberto Ciancio

Petra Todd

Camilo Garcia-Jimeno

Following a period of increasing immigration enforcement under George W. Bush's administration, the Obama administration reversed immigration policies and issued strict new guidelines to relax enforcement in 2011. The purpose of this paper is to exploit this natural experiment in the enforcement of the immigration laws to study the effects of federal immigration policies on local enforcement, crime and policing efficiency. I use a unique and new data set obtained through a Freedom of Information Act request on several steps of the deportation process. I estimate how the drop in federal immigration enforcement affected county level enforcement, local crime rates and policing efficiency. My empirical analysis suggests that Democratic counties complemented federal policies, by reducing their immigration enforcement, whereas Republican counties tended to maintain higher levels of enforcement and to not react much to the guidelines. Employing a triple-difference approach, I find that Democratic counties with higher non-citizen population shares saw greater increases in clearance rates, a measure of policing efficiency, with no increase in crime rates. The results indicate that reducing immigration enforcement did not increase crime and rather led to an increase in policing efficiency, either because it allowed police to focus efforts on solving more serious crimes or because it elicited greater cooperation of non-citizens with police.

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1 Introduction

> ‘When Mexico sends its people, they’re not sending their best ...
> They’re sending people that have lots of problems, and they’re bringing those problems with us. They’re bringing drugs. They’re bringing crime. They’re rapists. And some, I assume, are good people.”
>
> Donald Trump, June 16, 2015

Immigration policy is central to political debates in the United States and in many European countries. During the 2016 US Presidential campaign, President Donald Trump proposed strong measures intended to stem the flow of immigrants and to reduce the undocumented population. These measures included building a wall on the southern border with Mexico, drastically increasing the number of deportations and reducing access to employment and welfare benefits for undocumented immigrants. His opponent the Democratic candidate Hillary Clinton, favored a path to citizenship for undocumented immigrants and excluding some categories of undocumented immigrants from deportation.

One reason politicians focus on immigration is an assumed causal relationship between immigration and crime. Those in favor of strong immigration enforcement argue that because immigrants commit a disproportionate number of crimes, removing criminal aliens should be a top priority. Those favoring a more lenient approach believe that strong immigration enforcement is counterproductive, because it diverts law enforcement resources from fighting more serious crimes and makes immigrants less likely to cooperate with the police.

These disparate beliefs regarding immigration manifest themselves through variation in local governments' enforcement of immigration laws. In the US, self-declared sanctuary cities such as San Francisco protect undocumented immigrants from deportation and guarantee limited access to health care and other social services. In contrast, Sheriff Arpaio of Maricopa County, Arizona gained notoriety for his workplace immigration raids. Because local governments choose their own levels of immigration enforcement, they can impede implementation of federal immigration policy. [1]

The goal of this paper is to measure the effect of federal immigration policies on local enforcement, crime and policing efficiency. As a source of exogenous variation in immigration enforcement, I use a 2011 policy change that drastically reduced non-border deportations in the US. These non-border deportations typically start with an arrest by a local police officer. Officers of the federal immigration agency, Immigration and Customs Enforcement (ICE), can then communicate to the local enforcement agency that they want to take the arrestee into custody by issuing

[1] For example, several bills have been proposed in Congress to defund sanctuary cities. In the opposite direction, in May 2012, the Justice Department under the Obama Administration sued Sheriff Joe Arpaio for racial profiling.

a so-called detainer. After receiving a detainer, the local enforcement agency in charge of jails, usually the county's Sheriff office, chooses whether the arrestee is expediently transferred to ICE custody to be later deported.

Following a period of increased enforcement under the George W. Bush administration, the trend was reversed when, in 2011, the Obama administration issued guidelines to relax enforcement. This was done partly to appeal to Hispanic voters in the run-up to his re-election campaign. These guidelines prioritized deportations of individuals representing an imminent threat to the country. The number of removals from the interior of the US peaked in 2010-2011 and then fell to about 30% of their 2010 level by the end of 2015. Some counties went further to walk back enforcement by passing "no detainer" ordinances designed to limit cooperation with ICE. In practice, this meant ordering the sheriff to stop handing over detainees to the federal authorities unless the detainees had committed serious crimes.

In this paper, I evaluate the effects of the 2011 reversal in immigration policy on county-level immigration enforcement, crime and policing efficiency. In so doing, I use a difference-in-difference as well as a triple difference methodology that exploits county characteristics to determine which counties are most affected by the policy change. Through a Freedom of Information Act request, I obtained unique data gathered under the Secure Communities program for the period 2008 to 2014. This program required fingerprints of arrestees that are sent to the FBI to also be shared with ICE. ICE can then cross-reference the fingerprints with information in their immigration database and detect potential illegal immigrants. The dataset includes monthly deportations at the county level along with information on the deportation process, between the arrest by the local enforcement agency to the final removal. The Secure Communities dataset is particularly useful because it enables the construction of a continuous and consistent measure of enforcement, namely the share of non-citizen arrestees that end up in ICE custody, in each jurisdiction over time. As described below, I decompose this enforcement measure into components due to local enforcement and those due to federal enforcement. I measure federal enforcement using the issuance of detainers while I measure local enforcement using the share of detainers that end up in ICE arrests. I supplement the data with monthly crime and clearance rates from the FBI's Uniform Crime Report as well as with county characteristics from the Census and the American Community Survey. I aggregate the data into quarters. The merged dataset enables examination of the impact of immigration enforcement on crime and on clearance rates, the number of crimes cleared by an arrest, a standard measure of policing efficiency in the criminology literature.

Using this unique data, I first document changes over time in county level enforcement and explore how enforcement relates to county characteristics. I find that both federal and local enforcement dropped significantly after the issuing of the Obama guidelines. However, counties reacted differently depending on preferences for immigration. My empirical analysis finds that Democratic counties complemented lenient federal policies, by reducing their immigration enforcement, whereas Republican counties tended to maintain higher levels of enforcement and to not react much to the guidelines.

To analyze the effects of the Obama guidelines on crime and policing, I first use a difference-in-difference approach comparing counties with different percentages of non-citizens before and after the change in policy. I use the non-citizen share of the population within counties as a proxy measure for the potential impact of the policy change with the assumption that the policy should have no effect on crime and policing outcomes in places with very few immigrants (e.g., Montana) and potentially strong effects in places with a large immigrant community (e.g., Los Angeles). I find that the relaxation of immigration enforcement in 2011 had no effect on crime levels or crime rates but had a small positive effect on clearance rates. My results also show that a one standard deviation increase in non-citizen share increases clearance rates for violent crimes by nearly 1%. This difference-in-difference analysis, however, does not take into account how county-level characteristics, such as the share of Democratic voters, affect the level of enforcement. Therefore, I employ a triple-difference framework to incorporate these characteristics. I find that counties with higher non-citizen population shares in more Democratic counties saw greater increases in clearance rates, my measure of policing efficiency, but experienced no significant change in crime. I also find that for a one standard deviation increase in non-citizen share, moving from a county with the lowest to the highest share of Democratic voters would increase the clearance rate for violent crimes by 3.5%, approximately 6.1% percent of the 57.1% average clearance rate for violent crimes.

For identification in the triple-difference analysis, I assume that for a given increase in non-citizen share, there would not have been differential changes in trend between Democrat and Republican counties without the Obama guidelines. By implementing an event study around the policy change, I provide evidence in favor of the parallel trend assumption by showing that, for a given increase in the non-citizen share, the Democratic share does not predict differential trends in clearance rates before the guidelines were issued.

I examine the robustness of the results to a number of factors, including changes in economic conditions, changes in the size of police department, other changes in immigration enforcement at the state or local level, and different ways of subsampling the data to create a common support between the treatment and control groups. Finally, I supplement the baseline analysis of the federal policy change by examining the effects of the California Trust Act, implemented in January 2014. This state law forced California counties to restrict their cooperation with ICE to include only immigrants guilty of serious crimes. Using a triple difference analysis, I find that, similar to the Obama guidelines, the Trust Act increased clearances and had no effect on crime. Following implementation of the Trust Act, a one standard deviation increase in the share of non-citizens in California counties raises the clearance rate by 3.9 percentage points relative to unaffected states.

This paper has two key findings. First, I show that tougher immigration enforcement does not reduce crime and appears instead to make the job of the local police harder, as reflected by the lower clearance rates in my results. Second, this paper explores how political considerations affect the implementation of immigration policy. I find that the impact of the policy can be heterogenous depending on county characteristics. The results underscore the importance of considering how local authorities will respond to federal policies in determining overall enforcement levels and the policies impact on county level outcomes. In particular, when local and federal preferences are aligned, the effect of federal policies is amplified.

The remainder of the paper is organized as follows. Chapter 2 describes the literature. Chapter 3 provides the institutional background of the deportation process and the policy change. Chapter 4 describes the data. Chapter 5 outlines the hypotheses of the project. Chapter 6 details the estimation strategy and the results. Chapter 6 presents the aforementioned robustness checks. Chapter 7 offers the summary and conclusion.

2 Literature

The relationship between immigration and crime is gaining importance in the literature, although existing empirical evidence is still scant. Examining at Italian provinces, Bianchi et al. [2012] find no significant impact of immigration on overall crime rates except for an increase in the incidence of robberies. Bell et al. [2013] examine immigration in the UK. They find a positive effect of immigration on property crime rates when looking at asylum seekers but no effect when considering the inflow of workers in 2004 from the rest of the EU. Pinotti [2014] uses a regression discontinuity design to show that legal status has a significant impact on the propensity to commit crimes. Baker [2015] analyzes the legalization of undocumented immigrants in the US following the 1986 Immigration Reform and Control Act and its effect on crime. He finds a strong decline in the number of crimes, particularly property crimes, which he attributes to greater labor market opportunities for the newly legalized population. My research is most closely related to Miles and Cox [2014], which analyzes the impact of the Secure Communities program on crime rates. After controlling for county-specific linear time trends, they do not find that the program has any significant effect on crime rates. While I utilize some of the same data, the focus of this paper differs, because I instead study the change in immigration policy that occurred under president Obama as well as the legal change under the California Trust Act.[2]

A growing body of research has examined the effects of recent local immigration policies in the US. For example, Watson [2014] shows that deportations reduce the welfare participation rates of both illegal and legal immigrants. Watson [2013] shows that counties that enrolled in a special partnership with the federal government to act directly as immigration officers experienced a drop in the immigrant population by driving immigrants to more lenient counties rather than to their country of origin as hoped by the promoters of such partnerships. Several other papers analyze the effects of E-Verify, a national employment verification program, on the labor market outcomes of undocumented workers (Amuedo-Dorantes and Bansak [2012], Bohn et al. [2015], Orrenius and Zavodny [2015]) and the immigrant population (Bohn et al. [2014]), by exploiting state laws that made the program mandatory for firms.

The literature on the political economy of immigration usually focuses on conflicts between the rich and the poor (Benhabib [1996], Mayda [2006]) or between skilled and unskilled workers (Ortega [2005]). Skilled workers tend to be in favor of low skilled immigration because their skills are complements to those of the immigrants. Native low skilled workers tend to be substitutes for immigrants and tend to oppose immigration. However, they face a potential trade-off because they may

[2] Also, in Miles and Cox [2014], they do not have precise information on the level of enforcement prior to Secure Communities, while I can measure the change in enforcement following the policy. Moreover, they cannot decompose local from federal enforcement.

benefit from more political power if low skilled immigrants become citizens and vote for pro-worker policies. While immigration policy is usually studied at the national level, I differ from the existing literature by showing the local level as well. From a policy perspective, I show that the effects of federal and state immigration reforms crucially depend on the reaction of local governments. For example, deferring removal action for some categories of undocumented aliens or restricting access to welfare benefits may trigger an opposing reaction of local communities trying to keep their desired amount of deportation intensity.

This paper is also closely related to the literature on the political economy of law enforcement. García-Jimeno [2016] analyzes the dynamics of law enforcement during the Prohibition and is able to disentangle the explanatory power of the evolution of beliefs over the success of the law from the evolution of moral values in the observed changes in Prohibition enforcement. Similar to my analysis, the local enforcement decisions are an essential determinant of the success or failure of the federal policy. Casaburi and Troiano [2016] analyze the effects of a large anti-tax-evasion program on the reelection of incumbent mayors. They find significant positive effects on reelection, particularly in areas with both lower tax evasion tolerance and higher efficiency of public goods provision, suggesting complementarities among enforcement policies and civic capital. The methodology of this paper is similar to that of Cascio and Washington [2014], which investigates the impact of the Voting Rights Act on voter turnout and state transfers to black communities using a triple difference estimator comparing states with literacy tests and shares of the black population in different counties.

3 Background

In this section, I describe details of the deportation process, the historical context that led to the policy change and the local reaction to the policy change. In the United States, any non-citizen can be deported. By law, undocumented immigrants must be deported because they do not have the legal right to stay in the country. The majority of undocumented immigrants come from Latin America. They have low levels of education and are more likely to be male (Passel and Center [2005], Borjas [2017]). Hispanic immigrants, both documented and undocumented, were initially concentrated in few states: California, Texas, Arizona, Florida, Colorado and New Mexico. Recently hispanic immigrants moved to other areas of the United States like North Carolina, Alabama, Georgia, New York and Illinois. The undocumented population grew substantially during the 1990s and early 2000s. It stabilized after 2006 at around 11 million, or 4% of the US population. In addition, non-citizen legal residents, can be deported if they commit what US immigration law defines as an aggravated felony. The Immigration and Nationality Act provides a list of aggravated felonies that includes violent crimes but also non-violent offenses such as counterfeiting and theft or burglary if offender is sentenced to at least one year in prison.[3].

Deportations may include both individuals apprehended at the border by the Customs and Border Protection and people already living in the US. Border removals consist almost entirely of people crossing the border from Mexico who are immediately sent back. Deportations at the border drastically decreased from 1.6 million in 2000 to 340.000 in 2011. This decline is not due to lower enforcement, which actually increased with more patrol agents deployed at the border, but is merely the consequence of less people trying to cross the border. Conversely, the majority of non-border deportations begin with the arrest of a non-citizen by a local police officer. Subsequently, ICE can ask local officials to hold the identified criminal alien in jail by issuing a detainer. Detainers are requests to the local enforcement agencies to hold the arrestee for 48 hours until ICE is able to pick them up from jail. If the local enforcement agency cooperates with the request, then the detainee will enter into ICE custody to be later removed. ICE has multiple ways to know that there is a deportable alien in a local jail. Through the Secure Communities program, fingerprints of arrestees are sent to ICE which stores a database of all non-citizens that have any previous encounter with the Department of Homeland Security. This includes all the non-citizens legally in the US and all the undocumented that overstay their visa, which is estimated to be around 50% of all undocumented immigrants. Moreover, it includes all the undocumented that illegally crossed the border and who have been captured at least once by the Border

[3]https://www.uscis.gov/ilink/docView/SLB/HTML/SLB/0-0-0-1/0-0-0-29/0-0-0-5684.html

Patrol. Therefore, most undocumented individuals should be in the ICE database. For the immigrants not in the database, ICE periodically visits local jails to conduct one-on-one interviews with inmates that are suspected to be violating immigration laws.

In the aftermath of 9/11, which shifted the focus of the Bush administration to national security, interior removals of non-citizens increased both in absolute numbers and relative to the estimates of undocumented population. Removals peaked during the first term of the Obama administration and then fell sharply after several policy changes were introduced in Obama's second term. ICE deported 69,478 immigrants from the interior of the United States in 2015, down from 229,235 in 2010 (figure B.1). This fall cannot be attributed to a decrease in the undocumented population which was roughly constant over this period. This implies some change in policy must be responsible for the decrease.

Local jurisdictions played an important role in the surge of interior immigration enforcement during the 2000s. The inability of Congress to pass comprehensive immigration legislation because of political disagreements resulted in the proliferation of local immigration measures. These local measures were either supportive of immigrants, as in so called sanctuary cities, or anti-immigrant, making it harder for them to obtain employment, housing and welfare. Steil and Vasi [2014] find that the Democratic share of votes in the presidential election of 2004 and education levels are the main significant predictors of pro-immigrant local ordinances. An important predictor of anti-immigrant ordinances is latino population growth. During this period, the federal immigration agency, known as Immigration and Customs Enforcement (ICE), introduced several partnerships with local enforcement agencies, such as the 287(g) program, the Criminal Alien program and Secure Communities. The ability to access local jails simplified dramatically the task of identifying undocumented people for deportation and potentially allowed ICE to focus on individuals who represented a threat to national security. The Secure Communities program further eased the task of identifying removable aliens by automatically sending fingerprints of arrestees to ICE. The enrollment in the Secure Communities program, contrary to the 287(g), was not optional for local governments and was instead mandated by ICE. They gradually introduced the program county by county, starting with the places with the highest concentration of immigrants.

Reflecting a shift in government priorities of the Obama administration, under pressure from immigration advocacy groups, in June 2011 the ICE director issued a memo to ICE agents, instructing them to prioritize deportation of criminal aliens who represent a serious threat to national security.[4] The memo particularly cites to limited enforcement resources and thus to the need to prioritize deportations. In practice, the result was a dramatic reduction in the number of immigrants deported by ICE. In June 2012, a new memo from ICE explicitly stated that certain categories of undocumented immigrants would not be deported, in particular children, protected by the introduction of deferred action for childhood arrivals (DACA). Fi-

[4] https://www.ice.gov/doclib/secure-communities/pdf/prosecutorial-discretion-memo.pdf

nally in October 2014, President Obama, by executive order, deferred deportation for other categories of the undocumented,. He introduced the Deferred Action for Parents of Americans and Lawful Permanent Residents (DAPA), and replaced the Secure Communities program with a new program called the Priority Enforcement Program, which still sent fingerprints to ICE but acknowledged that cooperation of local enforcement agencies is voluntary.

Despite these federal guidelines, in some cases ICE still sought the deportation of immigrants that had committed no serious crime. Beginning in 2011, several counties limited their collaboration with ICE to cases of serious crimes. In practice, they released deportable aliens from jail before ICE could arrest them, unless they had committed a serious crime. Sheriffs and county councils motivated these decisions with concerns regarding immigrants' trust in the police and their cooperation in criminal investigations. They also point to limited resources diverted to paying for inmates on hold for ICE. For example, Cook County's council in Illinois passed an ordinance approving limits to cooperation with immigration authorities, stating that

> "...it costs Cook County approximately \$43,000 per day to hold individuals "believed to be undocumented" pursuant to ICE detainers, and Cook County can no longer afford to expend taxpayer funds to incarcerate individuals who are otherwise entitled to their freedom ... ... having the Sheriff of Cook County participate in the enforcement of ICE detainers places a great strain on our communities by eroding the public trust the Sheriff depends on to secure the accurate reporting of criminal activity and to prevent and solve crimes..."
>
> Cook County Board of Commissioners, September 7, 2011

The \$43,000 per day translates into roughly 17 million dollars per year, a significant cost for the sheriff department. It is also clear from the language of the ordinance how ideological motivations further influenced the decision. Among the counties that passed similar ordinances, I find many counties are Democratic strongholds, such as San Francisco, Santa Clara, Philadelphia and Washington. In figure B.2, I show a map of the United States highlighting the counties that passed an ordinance of similar intent from July 2011 to September 2014 at the end of my sample. More policies have been passed after September 2014. It is clear that these policies are concentrated in areas with high hispanic immigration and a strong presence of the Democratic Party. County governments usually give the general rules regarding local immigration enforcement, while the sheriffs make the day-to-day decisions.[5]

[5] County governments have different structure. Depending on the US state, there is a board of supervisors, a commission or a council.

It is not unusual for sheriffs to disagree with the decision of the county government, but they are forced to follow their guidelines.[6]

Decisions to limit collaboration with ICE were also driven by several court decisions that made counties liable for holding immigrants in jail when no crime was committed, due to constitutional violations (Altis [2014]). These rulings implied that a suspected immigration violation does not legally constitute a sufficient reason to imprison someone. Finally, there were changes at the state level. California passed the Trust Act, which went into effect in January 2014. The law forced counties to limit their cooperation with ICE to serious crimes if they were not already doing so.[7]

[6]This is the case for example of Los Angeles.

[7]A similar policy was also passed in Connecticut.

4 Data

4.1 Enforcement

My empirical strategy requires data on deportations at the county level for multiple time periods. To obtain this information, I made a Freedom of Information Act request to the Department of Homeland Security (DHS) and got access to monthly deportations data at the county level for the period October 2008 to September 2014. These data are from the Secure Communities program which is a data interoperability system that automatically transmits information on arrestees to ICE. Prior to its creation, fingerprints taken by Local Enforcement Agencies (LEA) were routinely transmitted to the FBI for the purposes of conducting criminal background checks. Under Secure Communities, these fingerprints are also checked against the Department of Homeland Security's Automated Biometric Identification System (IDENT), which contains data on known immigration violators, known and suspected terrorists, criminal aliens and non-citizens subject to the US-Visit program. Counties were gradually enrolled in the program starting from October 2008. All counties were enrolled by January 2013. By June 2011, at the time of the policy change, more than 70% of the US population was living in counties enrolled in the program. Consequently, I have an unbalanced panel of counties with up to 73 months and 3181 counties.

The data include the different steps of the deportation process:

1. A: local arrest
2. D: ICE decides whether to initiate deportation (detainer request)
3. C: ICE takes arrestee into custody if local agency allows
4. removal

Federal discretion plays a role in deciding whether to initiate deportation through detainer requests.[8] Local discretion plays a role by arresting the immigrant and by deciding whether to honor the detainer request. The number of non-citizens arrested is not an adequate measure of immigration enforcement because estimates of the undocumented population are imprecise and there are many causes of arrests that are unrelated to immigration status. Due to these shortcomings, I instead consider the following measures:

- Total enforcement: C/A = ICE Custody / Local Arrests
- Federal enforcement: D/A = Detainers / Local Arrests
- Local enforcement: C/D = ICE Custody / Detainers

An advantage of using the Secure Communities dataset is that it allows me to construct a continuous measure of enforcement for all counties in the US that

[8] Federal discretion plays a role also in picking up the detainee. Even though they issued a detainer and so they are interested in deporting the individual, because of limited resources they may give up. However,I cannot separate federal efforts in picking up the detainee from local collaboration. Therefore, I focus on detainers issued for federal enforcement.

varies over time. Previous studies relied on one time policies implemented in few jurisdictions, while I am able to track the dynamics of immigration enforcement. In addition to utilizing richer data, my measures of enforcement avoid issues common in other papers. Fasani [2009] and Watson [2014] use total number of deportations as their measure of enforcement. Even including various controls, total deportations is likely to capture changes in crime levels. By focusing on what happens after the arrest, I am able to avoid the problem. My dataset is particularly useful in this capacity because it contains all of the steps of the deportation process. This allows me to separate local enforcement from federal enforcement.

One way to obtain the response of counties is to use the county ordinances that limit cooperation with detainers. I retrieve these "no detainer" policies from the Immigrant Legal Resource Center website and I use them as an alternative measure of local enforcement. The language of such ordinances is not sufficient to predict their impact on enforcement. By using Secure Communities data, I can measure the extent to which these laws decrease enforcement. Examining a small number of these counties, I find that local enforcement often drops after an ordinance is passed, but, in some cases, I do not observe any change in enforcement following a no-detainer policy.[9]

One difficulty with my proposed measures stems from the time between arrest and entering into ICE custody. Arrestees should first serve the sentence for which

[9] For example the ordinance in Philadelphia in 2014 only marginally reduced the share of detainers not in ICE custody.

they are taken into custody by the local police. It is not clear that this is always the case or if the arrestee can be handed over to ICE prior the end of the judicial process and conviction. In the data, I find an immediate change in enforcement after the Obama guidelines and also find a sudden decrease in local enforcement of several counties after passing a "no detainer" policy. A possible explanation for this rapid reaction of enforcement to the policy change is that most immigrants are arrested for minor violations such as traffic violations or solely immigration violations.[10] Moreover, the dataset allows me to focus the analysis on less serious offenses which require less time in jail.

Another issue is that only a small fraction of counties was enrolled in Secure Communities before the Obama guidelines. Early adopters are likely to have different characteristics than other counties. Cox and Miles [2013] analyze the correlates of early enrollment in Secure Communities. They find that the most relevant county level explanatory variable is the share of the population that is hispanic. They also reject the hypothesis that Secure Communities was first activated in counties favorable to strict immigration enforcement. I perform a similar analysis and confirm that Democratic share is not an important predictor of activation date after taking into account the share of non-citizens and other control variables (table B.2). In the empirical analysis, I use the entire sample of US counties as well as the restricted sample of activated counties. I highlight when the restriction of the sample sample makes a difference but generally the results are very similar.

[10]TRAC [2013].

By examining enforcement measures over time, I can observe the effects of the policy changes. Figure B.3 shows that total enforcement drastically decreased after the first federal policy change was implemented in June 2011. The enforcement measure in the graph is an average weighted by 2010 population of counties enrolled in Secure Communities before May 2010, allowing for a consistent measure of enforcement over time. Federal enforcement mirrors the change in total enforcement, while local enforcement drops only slightly after the policy and has a further strong decrease after the California Trust Act and the court decision described in Altis [2014].The measure of local enforcement is sometimes above 1, because, in some cases, ICE takes a detainee into custody without issuing a formal detainer request. [11] Table B.3 shows the striking variation in federal enforcement across districts.

4.2 Crime and Clearances

When limiting coordination with ICE, counties give two main justifications:

1. Reluctance of immigrants to contact police in case, when they are the witness or the victim of a crime
2. Detaining immigrants for ICE is costly and diverts resources from other police duties.

These perceived effects may lower police efficacy. At the same time, critics of these non-compliant policies claim that they reduce the deterrence of the Secure Com-

[11] It is clear that the first federal policy change had a strong effect on enforcement while the second guidelines did not seem to impact the trend. Therefore, in the rest of the paper I will consider only the first federal policy change and the California Trust Act.

munity program and therefore increase crime. One measure of police efficacy is the clearance rate. The clearance rate measures the number of reported criminal offenses that is cleared by an arrest (İmrohoroğlu et al. [2004], Mas [2006] and Paré et al. [2007]). Clearance is a term used by the FBI and other reporting agencies to describe when police have obtained enough evidence to arrest someone for a particular offense. If immigrants cooperate less with police, it may become harder to arrest offenders. Moreover, diverted resources may decrease overall police productivity, by limiting their focus to immigration violations. Using monthly data from the Uniform Crime Report (UCR), I construct a measure of the crime rate, $\frac{crime}{population}$, and the clearance rate $\frac{clearances}{crimes}$. As shown in figures B.4 and B.5, these distributions have a mass at zero and there are a few outliers from the median value. Therefore, I replicate my results using an inverse hyperbolic sine transformation with a less skewed distribution (figure B.6 and B.7). In addition, I use yearly data from the FBI on the number of police officers per capita to serve as an additional control. The UCR covers violent crimes, including murders, manslaughters, rapes, robberies, assaults, as well as property crimes such as burglary, larceny and vehicle theft. It does not include traffic violations, driving under the influence or drug related crimes, which constitute the majority of cited offenses. For the majority of detainees, there is no offense cited other than an immigration violation.

One of the limits of the data is that the UCR uses reported crimes instead of the actual number of crimes. This may be problematic for accurately measuring changes in crime because if undocumented immigrants are scared of contacting the police, they will not only avoid serving as a witness but will also avoid reporting crimes to the police. However, by focusing on crimes, like murders, where the probability of police discovery is higher, I can lower the gap between reported and actual crime. Moreover, if this is the case, it provides more evidence that immigration enforcement makes police work harder. Unfortunately, there is also well known measurement error in UCR data. To overcome this issue, I follow the existing literature by imputing year estimates to quarters when needed (Maltz and Targonski [2002]). Around 7% of the sample requires such adjustments.

4.3 County Characteristics

The empirical analysis requires exogenous cross-sectional variation between counties in terms of preferences for enforcement and the potential impact of the policy change. I utilize several covariates that may be important determinants of preferences. To capture political preferences, I focus on the Democrat and Republican share of voters from the 2008 presidential election from Dave Leip's atlas of US presidential elections (Leip [2012]). This may be relevant because the Republican party has favored stricter immigration enforcement in recent years. According to a 2015 survey by the Pew Research Center, 71% of Republicans say immigrants in the U.S. make crime worse, compared with just 34% of Democrats. Meanwhile, Republicans are half as likely as Democrats (24% vs. 55%) to say immigrants have almost no effect on crime. In alternative specifications of the empirical model, I include the percentage of people voting Republican in the 2012 presidential election.

There are other characteristics of counties that may be important determinants of attitudes toward immigration. As previously mentioned, several papers on the political economy of immigration emphasize the importance of the conflict between skilled and unskilled labor in shaping tastes given the different effects that immigration has on labor market outcomes of those groups (Mayda [2006]). Accordingly, I consider education, specifically the share of the population with a bachelor degree, to control for skill-related worker preferences. To represent labor demand, I look at the sectoral composition of the economy and, in particular, the share of workers in the service sector. Since firms' recruiting needs may influence leniency of the local governments toward immigrants, it is important to include this information. Finally, I use a measure of how rural counties are from the National Center for Health Statistics. The American Community Survey provides this information at the county level as well as the non citizen share of the population by county using a five years sample from 2006 to 2010. From the American Community Survey, I also find the hispanic non citizen population share and use it as an alternative measure of deportable aliens in a county with the justification that most of the undocumented come from Latin America.

5 Hypotheses

In this section, I characterize the strategic relationship between federal and local enforcement and the channels through which they affect crime and policing. In the appendix, I present a simple model that formalizes this discussion. There are two ways in which local enforcement could react to a drop in federal enforcement. If federal and local efforts have some degree of technical complementarity in determining the overall level of enforcement, then a fall in federal enforcement reduces incentives for a local government that derives some utility from immigration enforcement to invest in local enforcement. Alternatively, counties may instead increase local enforcement to compensate for a lower federal enforcement and to satisfy their own preferences. Anti immigrant counties may be particularly risk averse and want to avoid a substantial drop in total enforcement. If this is true, it would result in a larger reduction of local enforcement in more lenient counties. More generally, preferences of local governments and the elasticity of substitution between federal and local enforcement determine the degree of strategic complementarity and whether the two efforts are substitutes or complements. Therefore, counties with disparate characteristics, meaning different preferences, may react differently to the Obama guidelines.

Federal enforcement may vary by county and within ICE federal districts. One reason for this is that it can strategically react to local enforcement. For example federal officials may disinvest from counties that stop collaborating with ICE, because if a sheriff does not hand over certain immigrants, there is no point to issuing a detainer request. Another source of variation is that ICE may prioritize certain counties over others. They may care more about counties with certain characteristics, such as counties with more immigrants or those in urban areas. Even local politics could potentially impact the investment priorities of the federal government by encouraging contributions to more politically aligned constituencies. When the ICE directives emerged, federal districts may further focus resources on these counties or uniformly reduce resources. In the absence of clear details on ICE goals and procedures, ICE enforcement remains an empirical object.

Immigration enforcement may directly affect crime at least in two ways. First, it deters immigrants from committing crimes in order to avoid being deported (Becker [1968], Abrams [2012]). Second, it affects the size of the immigrant population, since deportations actually remove people from the county. The latter may reduce the crime level but has an ambiguous effect on crime rates depending on whether immigrants commit more crimes than natives. If immigrants have a higher propensity to commit crimes than natives, crime rates should fall if immigration enforcement decreases the immigrant population.

Enforcement may also impact crime rates and levels by changing the ability of the police to fight crime. Sheriffs and county supervisors that limited their coordination with ICE cited two motivations. One is that enforcement disrupts the relationship between police and the immigrant community. In particular, fear of contacting the police induces undocumented individuals to report fewer crimes and to avoid serving as witnesses. This undermines what is known in the criminology literature as community policing (Greene and Mastrofski [1988]). Second, immigration enforcement could divert the resources of law enforcement agencies from fighting crime. Keeping inmates in jail solely for immigration violations is very costly and may diminish resources available to patrol and arrest criminals.

Together, these two channels may reduce the probability of arresting those who commit crimes. I can then test whether the reform had an impact on the county level crime rates and clearance rates, or the ratio of crimes cleared by an arrest. When studying at the effect of the policy on outcomes at the county level, I exploit the fact that counties with a higher non citizen share should face a greater impact from these policies. County level enforcement changes at different rates depending on county preferences for immigration, which in turn depend on their characteristics. The effect of the Obama guidelines should be greater among counties with higher non-citizen shares and with characteristics that are associated with larger drops in enforcement. In particular, I focus on the county share of Democrats, which is a significant determinant of local policy changes in immigration.

6 Empirical Analysis

6.1 Enforcement

The first objective of this paper is to analyze the heterogenous response of enforcement. to new federal guidelines. Then, I exploit variation in the local response to analyze crime related outcomes. While describing the institutional background, I highlighted that several counties decided to formally limit their cooperation with ICE and that this occurred mostly in communities that tended to vote Democrat. Therefore, I focus on the Democratic share of voters in the 2008 presidential election, as the primary source of heterogeneity.

I run the following regressions where total, local and federal enforcement are the dependent variables and the policy interacted with county characteristics serve as the explanatory variables:

$$\text{Enforcement}_{ct} = \alpha_c + \alpha_t + \phi(\text{Guidelines}_t \times \text{democratic share}_c) + \gamma W_{ct} + \zeta_{ct}$$

where α_c denotes county fixed effects, α_t denotes time (quarter) effects and W_{ct} represents county specific time varying controls. Standard errors are clustered at the county level in order to control for autocorrelation in the error term ζ_{ct}. In all specifications, W_{ct} includes dummies for the ICE federal districts and states interacted with time dummies to allow for the wide heterogeneity in federal enforcement changes among districts and to measure the importance of local characteristics in enforcement changes.

If the Obama guidelines generate a different reaction in heavily Democratic counties, more specifically causing them to reduce local enforcement to a greater extent than Republican counties, the coefficient on the interaction between *Guidelines* and Democratic share should be negative. Column (1) in table B.4 reports the basic specification with local enforcement and Democratic share interacted with the policy. The relevant coefficient is negative and statistically significant at the 1% level, indicating that Democratic counties had a larger reduction in local enforcement. The result is in line with the anecdotal evidence described earlier. While this simple specification predicts this intuitive result , it may be the case that partisanship is not the relevant driver of local preferences after the addition of other factors. Column (2) expands the analysis by introducing several county characteristics interacted with the policy change. In particular, I consider the non-citizen population share, share of the population with bachelor degrees and a measure of how rural the county is. A negative significant coefficient shows that Democratic share is still a significant predictor of change in local enforcement and that the other factors are not significant. The result implies that a one standard deviation increase in the Democratic share decreases the ratio of individuals in ICE custody to total detainers by around 5%.

In columns (3) and (4) I perform a similar analysis for federal enforcement, that is measured as the ratio of detainers over local arrests of non-citizens. The coefficient for Democrat is not significant showing no evidence that federal districts offset lower local enforcement in Democratic counties by increasing federal enforcement. Finally, in columns (5) and (6) I analyze the effects on total enforcement which I measure as the ratio of ICE arrests over local arrests of non-citizens. In both cases, the coefficient for Democrat share is negative although significance is lost with the extra controls.

In order to check that the results are not driven by outliers, as shown in table B.5, I redo the analysis using the inverse hyperbolic sine transformation of the enforcement measures. This transformation corresponds to $log(x+\sqrt{1+x^2})$. With the transformation, I find very similar results, implying outliers do not drive my earlier results.

In the data section, I discussed concerns regarding the enforcement measures I constructed. One of the concerns was the timing between local arrest and ICE arrest. Another was that enforcement measures will be less comparable between counties if the type of crimes for which the immigrants are arrested is very different in different counties. To provide evidence that neither is driving my results, I restrict the sample to non-serious crimes that have shorter jail sentences. Democratic share is still a significant predictor of local enforcement when only considering non-serious crime, meaning I can dismiss these earlier concerns (table B.6).

All the results shown above are limited to counties enrolled in Secure Communities at the time of the policy. However, in the data section I provided evidence that the Democratic share is not a significant predictor of the month of activation in the program. I will also provide evidence that the results for crime and policing are not affected by restricting the analysis to counties enrolled before the Obama guidelines.

Finally, I look directly at the correlation between passing a no-detainer ordinance and the Democratic share of voters. I measure the correlation by running a simple OLS regression with a dummy for passing an ordinance regressed on the Democratic share and other county characteristics. Results from table B.7 clearly show that counties that passed no-detainer policies were disproportionally more Democratic.

6.2 Crime and Policing

In this section, I outline my approach to estimate the effects of the Obama guidelines on crime and policing. Regressing outcomes on the measures of enforcement would lead to misspecification. This is because there is an obvious endogeneity problem. For example, an increase in crime may induce the local law enforcement agency to attempt more deportations while an increase in efforts to fight crime may reduce resources devoted to immigration enforcement. Another issue is that there is spurious correlation between enforcement and clearance rates because clearances also include the arrests of immigrants. The first problem could be addressed by an IV strategy by using an instrument constructed by interacting the policy change with county characteristics. However, the spurious correlation between the dependent and independent variables would create a non linear measurement error problem that would seriously bias the estimates.

To avoid these issues, my initial empirical strategy exploits variation across counties to determine different levels of treatment of the policy. I implement a difference-in-differences approach using the variation in the non-citizen share of the population. The idea is that immigration enforcement should only impact counties with non-citizens. For example, deportations may impact crime in Los Angeles but should have no impact in North Dakota. Our initial specification is

$$y_{ct} = \alpha_c + \alpha_t + \beta(\text{Guidelines}_t \times \text{non citizen share}_c) + \gamma W_{ct} + \zeta_{ct}$$

where y can be the clearance rate, the inverse hyperbolic sine transformation of the number of crimes or the crime rate and *Guidelines* is a dummy that is equal to 1 after the policy decision. I include as controls, county and time fixed effects as well as some time variant W which in the baseline specification are state and federal district dummies interacted with time dummies. The sample consists of a quarterly panel of all the US counties from October 2008, the start of the Secure Communities program, to September 2014. I chose October 2008 as a start date to analyze the same period covered in the enforcement analysis and because it gives roughly the same number of quarters before and after the Obama guidelines.

I first analyze the clearance rate for violent crimes. Column (1) of table B.8 reports the coefficient on the non citizen share as positive and statistically significant at the 1% level. In column (2), I introduce extra controls interacted with the policy change and find a positive coefficient of 0.24 significant at the 1% level. This indicates that a one standard deviation increase in non-citizen share increases the policy's impact on clearance rates for violent crimes by nearly 1%. Columns 3 and 4 show results for clearance rates of property crimes. The effect on these rates is smaller but still statistically significant. When analyzing crime, I show results for levels of violent and property crimes in columns 5 to 8. The results do not find that the policy has statistically significant effect. When I look at crime rates, I find that the policy has negative effects, but these effects lose significance when I impose the inverse hyperbolic sine transformation. This implies the observed crime rate results may be driven by outliers (table B.9). Therefore, my results provide no evidence that the Obama guidelines increased crime and, if anything, they reduced crime. There are multiple explanations for these results. It is possible that immigrants commit few serious crimes thus limiting the deterrent effect of deportations. It is also possible that better policing during this time helped to prevent crimes.

The enforcement analysis shows that there is heterogeneity in the change in immigration enforcement among counties after the policy. Because county-level changes in enforcement will in turn impact crime, it is important to tease out this heterogeneity to isolate the effects of the Obama guidelines on crime. Therefore, I would like to incorporate this heterogeneity into the difference in differences analysis. I argued earlier that the Democratic share of voters is an important determinant of change of immigration enforcement. The difference-in-difference generates the average effect in Democratic and Republican counties. Then, I implement a triple difference in difference strategy using the county-level Democrat share of voters in the 2008 presidential election. The coefficient of interest is given by the triple interaction between the policy, non-citizen and Democrat share. I include an interaction between *Democrat* and *Guidelines* to control for factors unrelated to the policy that would lead to different outcomes for Democratic and Republican counties. Intuitively, this captures whether a difference in the non-citizen share has a greater impact after the guidelines in places with higher Democratic share.

The first identifying assumption for my triple difference analysis is the parallel trends assumption. That is, for a given increase in non-citizen share, there would be no differential change in trend between Democrat and Republican counties without the Obama guidelines. I will provide justification for this assumption with an event study. The second identifying assumption is that there are no contemporaneous events to the policy change that differentially affect the treatment and the control group. I consider alternative explanations for my results in the robustness section.

The main specification is

$$
\begin{aligned}
y_{ct} =&\alpha_c + \alpha_t + \beta(\text{Guidelines}_t \times \text{non citizen share}_c) + \psi(\text{Guidelines}_t \times \text{democratic}_c) \\
&+ \phi(\text{Guidelines}_t \times \text{democratic}_c \times \text{non citizen share}_c) + \gamma W_{ct} + \zeta_{ct}
\end{aligned}
$$

where y is clearance rate or crime rate and the coefficient of interest is ϕ.

Column 1 of table B.10 shows results for clearance rates of violent crimes. I observe that the coefficient on the triple interaction is positive and statistically significant at the 1% level. Column 2 shows results, when adding extra controls and I find a coefficient of 1.1, significant at 1 percent (column 2). This indicates that for a one standard deviation increase in the non-citizen share, moving from the lowest to the highest Democratic share increases the clearance rate by 3.5 percentage points, where the average clearance rate for violent crimes is 57%. This is the main result. The drop in federal enforcement caused by the guidelines has a significant positive effect on police efficiency in arresting criminals who commit violent crimes.

Column 3 shows the results for the clearance rates of property crimes. In this case, the coefficient on the triple interaction is positive and significant but is about one third of the coefficient for violent crimes. However, this finding is not significant while including the extra controls. The results suggest that immigration enforcement has a larger impact on fighting violent crimes than on property crimes. One possible explanation is that undocumented immigrants are more likely to witness violent crimes than natives because they live in neighborhoods with higher concentration of violent crimes.

I next check whether immigration enforcement has any effect on crime. If crime disproportionally increases in Democratic counties with high proportion of non-citizens, then there is evidence that immigration enforcement reduces crime. Columns 5 and 6 in table B.10 show results for total number of violent crimes and columns 7 and 8 show results for the inverse hyperbolic sine transformation of property crimes. Table B.11 shows that immigration enforcement has no significant effect on crime levels or crime rates.

As mentioned earlier, I only observe reported crimes. The bias from not observing unreported crime leads to overestimation of the effects of enforcement on actual crime. Because lower enforcement should increase reporting from undocumented immigrants, the estimate is an upper bound on the effects of enforcement on crime. To provide evidence that the Obama policy did not in itself increase crime, I redo the analysis with murders and manslaughters only since the reporting problem should be less serious than that of other crimes. Again, I do not find a significant effect when focusing only on these crimes, as shown in table B.12.

Another potential source of bias is that immigrants may react to a change in enforcement by moving to other counties. For instance, suppose enforcement drops in county A. This would draw criminals from county B to county A. leading to crime increases in county A. In this case, I would underestimate the effect of enforcement on crime and my estimates would be an upper bound of the effects of the policyinduced reduction in enforcement on crime. It is possible that the policy increased crime and I do not observe that because I cannot capture the migration response. However, given that I use only three years of data after the policy change, the mobility response to the policy should be limited.

6.3 Mechanisms

My results show that immigration enforcement has a negative impact on police efficiency but I would like to determine whether this is due to increased collaboration of immigrants with the police or to resources shifted from immigration enforcement to policing. I have not found suitable data that describe collaboration of immigrants with the police. However, I have data on county expenditures on police, justice and correction that may shed some light on the crowding out of resources story. The data come from the Government Finance Database of Willamette University (Pierson et al. [2015]). It provides local government expenditures at the county level extracted from census of government data. The data has three types of expenditures important for this analysis: corrections, judicial and police. Immigration enforcement may increase correction expenditures because it is costly to hold immigrants for ICE. It may increase judicial costs by causing increasingly frequent lawsuits from holding non convicted immigrants in prison. I use the same triple difference model I applied in my analysis of clearance rates to examine the effects of the Obama guidelines on expenditures. I find mixed evidence. My results show that the policy has a significant positive effect on the log of current police expenditures and a negative effect on the share of judicial expenditures (table B.13). I do not find significant effects on correction expenditures, the share of police expenditures or the log of judicial expenditures. A more in-depth analysis of county finances is needed but I do find some evidence consistent with the theory that immigration enforcement crowds out other policing activities.

6.4 Parallel Trends Assumption

For the triple difference approach to be valid, I need the parallel trends assumption to hold. In a general discrete setting, this requires that the relative dynamics of both the treatment and the control group would be the same in the absence of the policy shock. Specifically, for a given increase in non-citizen share, the relative dynamic of Democrat and Republican counties would be the same in the absence of the policy change. This is crucial because otherwise the results may simply reflect a pre-policy differential trend in unobservables between Democrat and Republican counties given a certain non-citizen share. To test for such trends, I develop an event study analysis with the following specification:

$$
\begin{aligned}
y_{ct} =& \alpha_c + \alpha_t + \sum_{\tau} \beta_\tau (Noncitizen_c \times P_t^\tau) + \sum_{\tau} \psi_\tau (Democrat_c \times P_t^\tau) \\
&+ \sum_{\tau} \phi_\tau (Noncitizen_c \times Democrat_c \times P_t^\tau) + \epsilon_{ct}
\end{aligned}
$$

where P_t^τ is a dummy equal to 1 if $\tau = t$. In figure B.8, I plot estimates of the coefficients ϕ. For the identification assumption to hold, the coefficient ϕ should not be significantly different from zero prior to the policy. I omit the interactions with the dummy for one of the quarters before the policy to identify the model.

The coefficient β captures the change in the gradient of clearance in non-citizen share between the second quarter of 2011 and quarter τ for comparison (Republican) counties, while the sum $\beta + \phi$ captures that change for treatment (Democrat) counties. The triple interaction term is a significant predictor of clearances only after the Obama guidelines. No coefficient is significantly different from zero before the policy and immediately after the policy, the coefficient becomes positive and significant, entering in a new trend. [12]

[12] I am able to test whether the pre-policy coefficients are significantly different from one another. The F test cannot reject the hypothesis that

$$H_0 : \phi_{-9} = \phi_{-8} ... = \phi_0$$

with a p value of 0.46.

7 Robustness Analysis

7.1 Specification Tests

In the previous section, I showed that the clearance rates for violent crimes increased disproportionally more for high immigrant communities in relatively Democratic counties. In this section, I test several specifications to verify that certain implicit assumptions I do while running my main specification were appropriate. Table B.14 shows different specifications for the triple difference analysis using clearances of violent crimes as the dependent variable. Column 1 is the main specification with county and time fixed effects, county specific linear time trends and state and districts fixed effects interacted with time effects. In the previous section, I showed that Democratic counties reduced more enforcement but the result was limited to the sample of counties who were enrolled in Secure Communities prior to the Obama guidelines. I already showed that Democratic share is not a significant predictor of early activation in the program. Now I verify that the results hold when including only counties already enrolled in Secure Communities prior to the Obama guidelines. Doing so, the coefficient of interest is still significant and very close to that of the baseline analysis.

The baseline empirical model assumes the effect of the Democratic share of voters is linear. Instead, it could be that a discontinuity arises when Democrats have a majority of votes because then they can choose the county board and the sheriff. One way to test for nonlinearities is to add the interaction of a dummy for a Democratic majority with guidelines and an additional interaction with non-citizen share. I define a county as having a Democratic majority if the share of Democratic voters in the 2008 presidential election was greater than the Republican share. The coefficient on the triple interaction is significant and higher than in the baseline model, suggesting a slightly lower elasticity for counties with Democratic majorities. However, the coefficient on Democratic majority is negative and not significant, suggesting non-linearities are not a relevant problem.

Another important concern is that Republican counties may not be valid comparisons for Democratic counties even after controlling for several other characteristics. First, I check that the two types of counties are comparable in terms of non-citizen shares. After trimming the sample in order to get a common support over non-citizen share between counties in the top quartile and the bottom quartile of the Democratic share, the results are unaffected. A related concern is that the immigrant population may differ between Democratic and Republican counties. Then, the results may reflect that the counties have different compositions of immigrants instead of reflecting local preferences. It could be, for example, that only undocumented immigrants fear the police and that they are concentrated in

Democratic counties. Then, the greater effects of the policy observed in Democratic counties may be explained by them having a higher share of undocumented immigrants given the same number of non-citizens. To show that this is not a major concern, I replicate the analysis using the hispanic non-citizen share instead of the whole non citizen population with the logic that undocumented immigrants are disproportionally hispanic. Again, the results are very similar to the baseline in magnitude and statistical significance. As an additional check of the parallel trend assumption, I show results including county specific linear time trends. The coefficient is similar to the baseline results and statistically significant at the 1% level. Finally, because of the skewed distribution of the dependent variable, I present results using the inverse hyperbolic sine transformation in the last column of table B.14 . The results do not change when using this transformation.

7.2 Alternative Explanations

My results thus far suggest that the policy, by drastically reducing immigration enforcement, had a larger positive effect on clearance rates in counties with a higher share of immigrants while its impact on crime did not differ based on the county share of immigrants. However, other channels may have contributed to the differential change in clearance rates between counties. In this section, I consider alternative explanations to these observations.

One predictor of clearance rates may be the number of police officers. To control for whether the aforementioned disparities are solely due to different numbers of police, I introduce the yearly (log) number of police officers from the Uniform Crime Report and find very similar results as shown in column 1 of table B.15. Economic conditions may also be an important omitted variable that both affects enforcement and crime. Local governments may change enforcement depending on the hiring needs of firms (Fasani [2009]) or their unemployment levels. At the same time, better economic conditions tend to reduce crime and can also affect the efficiency of the police. The time period covered in my data coincides with the Great Recession, further emphasizing the need to control for economic conditions in this analysis. For this reason, I control for labor demand shocks using a Bartik shock at a yearly frequency[13]. The control is not significant and the coefficients of interest are similar to the baseline, which confirms economic conditions are not driving my results.

A potential threat to identification comes from concurrent changes in immigration enforcement, particularly at the state level. In the last decade, state and local governments were especially active in immigration policy. With Congress unable to

[13]The idea is to use industry shares in a county and changes in employment (or wages) for the different industries at the national level excluding that particular county. National levels are not affected by a single county and interacted with industry shares of the county they are correlated with changes in employment in the county. Therefore, it is a valid instrument for labor demand changes and can be written as

$$\Delta B_{c,t} = \sum_{ind} (log(E_{ind,-c,t}) - log(E_{ind,-c,0}))\frac{E_{ind,c,0}}{E_{c,0}}$$

where period 0 can be considered the year before the start of the sample so the 2008. I construct this measure using several years of the ACS which provides a repeated cross-section of individuals with information on employment, industry and location. The lowest geographical unit is an area called PUMA and for several individuals I need to impute the county using the population share of county in a PUMA provided by the Census crosswalk.

pass comprehensive reforms, states passed laws restricting employment (Amuedo-Dorantes and Bansak [2012]), driver licenses, in-state tuition and access to welfare. Municipalities also passed several ordinances regarding immigration, mostly in the period around 2006. I collected all these policies which I may use as controls. A policy of distinct importance to our context is the 287g agreement between ICE and local or state governments which allowed local law enforcement to directly enforce immigration law. Los Angeles and San Bernardino enrolled in the program in 2005 and subsequently 66 other counties joined the program. The Obama administration ended a portion of these agreements at the end of 2012 and they are no longer in effect.

With regards to concerns about state-level policies, I already include state effects interacted with time effects in my baseline model. However, the effects of the policies may be heterogenous within states. In column 3 of table B.15, I introduce as additional control the (log) number of firms enrolled in the E-Verify program. This program enables firms to electronically check the legal status of the employees. Column 4 introduces an interaction between having a 287g agreement and the dummy for periods after the guidelines to see if the termination of these agreements affect my results. However, since most of the 287g agreements operated in counties leaning Republican, if anything we should see a stronger reduction in enforcement in Republican counties which goes in the opposite direction of my results. In both specifications, the results are not substantively affected (table B.15). If anything, the coefficient on the triple interaction is slightly greater which indicates that the reaction of the counties to the federal program is even greater once we take into account the end of the 287g program.

7.3 California Trust Act

So far, I have analyzed the impact of the Obama guidelines to understand how federal and local enforcement interact as well as their effect on several county-level outcomes. To further explore these phenomena, I analyze the impact of a similar policy, the California Trust Act, which intended to reduce local immigration enforcement. Before the law was implemented in January 2014, several counties already decided to limit their compliance with ICE, especially those with a long history of sanctuary city status, while many other counties continued to fully cooperate with ICE. Pressure from the Latino voters convinced Democratic Governor Brown to sign the law on October 5th 2013, after vetoing a similar measure in 2012. Intuitively, one would expect the Trust Act to have a similar impact to that of the federal guidelines in reducing enforcement. STherefore, it would be reassuring to show that it lead to a similar impact on crime and policing as the federal policy.

Table B.16 shows the effects of the Trust Act on local enforcement, crime and clearances. I first include a simple dummy for the policy, leading to the following specification:

$$y_{ct} = \alpha_c + \alpha_t + \psi(Post_t \times \text{California}_c) + \gamma W_{ct} + \zeta_{ct}$$

where I include the extra controls and the county specific linear time trend but remove district fixed effects to achieve identification. I also add Democrat and non citizen share interacted with time to control for the fact that California has more immigrants and leans Democrat. The sample used in this analysis consists of quarters after the Obama guidelines, since there was likely a differential change in trend between California counties and the rest of the US after the guidelines.

From the results in column (1), it is clear that the policy had the intended effect of reducing local enforcement. The number of detainers that actually become ICE arrests significantly dropped in California compared to the rest of the US. Federal enforcement did not decrease (column 2), while total enforcement dropped following the local enforcement changes (column 3). Analyzing police outcomes, the clearance rate for violent crimes increased by 7.8% while the clearance rate for property crimes increased by 3% compared to what would have happened without the Trust Act. The policy does not seem to have a significant impact on crime levels or crime rates.

Next, I implement a triple difference framework to analyze the effects of the Trust Act. I would expect it to have larger effects on crime and clearances in counties with higher non-citizen shares. My triple difference analysis has the following specification

$$
\begin{aligned}
y_{ct} =& \alpha_c + \alpha_t + \beta(\text{Guidelines}_t \times \text{non citizen share}_c) + \psi(\text{Guidelines}_t \times \text{democratic}_c) \\
&+ \phi(\text{Guidelines}_t \times \text{democratic}_c \times \text{non citizen share}_c) + \delta(Post_t \times \text{non citizen share}_c) + \\
&+ \lambda(Post_t \times \text{California}_c \times \text{non citizen share}_c) + \gamma W_{ct} + \zeta_{ct}
\end{aligned}
$$

where the coefficient of interest is on the triple interaction between a dummy equal to 1 for periods after the Trust Act, a dummy for counties in California and the non-citizen share. In this specification, I use the entire sample starting from October 2008. Therefore, the equation includes the variables relevant to the Obama guidelines in order to account for the changes in trends due to the guidelines.

Table B.17 shows that the results are qualitatively the same as the federal guidelines. When examining the clearance rate of violent crimes, the coefficient on the triple interaction is 0.34 and statistically significant at the 1% level. After adding extra controls, the coefficient is 0.296 and significant at the 5% level. This implies that after the Trust Act, a one standard deviation increase in non citizen share increases the clearance rate by 3.9 percentage points in California Counties. While the impact on the clearance rates of violent crimes is substantial, I find no effect on the clearance rates of property crimes nor on crime levels or crime rates.

8 Conclusions

This paper studies the effects of immigration enforcement on crime and policing using the variation generated by a policy change for the deportation process in the United States that prioritized the deportation of dangerous criminals and precipitated a 70% fall in non-border removals between 2011 and 2015. Because the policy provides a source of exogenous variation in federal enforcement, it allows me to analyze the strategic relationship between local and federal immigration enforcement and their effects on crime and policing efficiency.

I find three main results. First, I find that Democratic counties had larger reductions in local enforcement than Republican counties after the policy. Second, using the Democratic share of voters as a proxy for variation in local preferences for immigration and the non-citizen population share as a measure of the potential impact of the policy, I find that there is no significant evidence that the guidelines led to an increase in violent or property crimes. Third, when using the same method, I find a positive effect on clearance rates, particularly for those of violent crimes. These results indicate that reduced immigration enforcement did not increase crime but rather led to an increase in policing efficiency. This could be either because it allowed police to focus efforts on solving crimes or because it elicited greater cooperation of non-citizens with police. Data on police resources and on crime reporting may shed light on which mechanism drives these results.

In addition, the results suggest that the degree of alignment between local and federal preferences is essential in determining the overall level of enforcement and therefore the real impact of federal policies. This is particularly relevant in the US where multiple layers - county, federal district and national government - are involved in the process. It is also potentially relevant in areas besides immigration. The degree of decentralization and how that affects local outcomes is an interesting avenue for future research. Another interesting future step would be to look at the effects of local enforcement spillovers on neighboring counties. Migrants may react to enforcement over time, relocating and thereby affecting local crime rates. Furthermore, neighboring counties may react by changing their level of enforcement, leading to a strategic game between counties. My results show a degree of substitutability between crime enforcement and pure immigration enforcement. One natural future step would be to explore how shocks to a particular type of enforcement have consequences on other types. In this case, lower immigration enforcement freed resources that may have been used to intensify arrests of certain ethnic groups.

The policy implications of this work apply not only to the US but also to European countries, with the caveat that Europe receives mostly refugees who may be different than those who immigrate to the US. There is an ongoing debate worldwide on whether to create a path to citizenship for undocumented immigrants or to increase deportations. If reducing crime is the primary policy objective, increased deportations may be sub-optimal.

Appendices

A Enforcement Game

After the Obama guidelines, several counties started to limit collaboration with ICE. In this section, I propose a framework where local governments strategically react to changes in federal enforcement. The players of the game are the local government and the federal district that both maximize deportations subject to a (monetary and political) cost. In the model, federal and local enforcement contribute to deportations (total enforcement) with a certain degree of complementarity. The Obama guidelines increase the cost of federal enforcement for the district thereby reducing federal enforcement. Unless local and federal enforcement are perfect substitutes, lower federal enforcement will decrease the returns from local enforcement. At the same time, lower federal enforcement may induce local governments to substitute and increase local enforcement to keep the desired level of deportations. In the model, this willingness to substitute is determined by a risk averse parameter in the utility of deportations.

Now I will formalize the model. Consider a Stackelberg game where the federal district moves first and the local government moves second. Total enforcement d is the combination of local e and federal f according to a CES production function

$$d = (e^{\rho} + f^{\rho})^{\frac{1}{\rho}} \tag{A.0.1}$$

In the second stage, local government maximizes

$$\max_{e \geq 0} \ \frac{d^{\theta}}{\theta} - C(e) \tag{A.0.2}$$

where

$$\theta = F(\lambda X) \ ; \ F'(\cdot) > 0 \tag{A.0.3}$$

can be interpreted as a risk loving parameter (equivalent to CRRA) which increases the elasticity of e with respect to f. It depends on county characteristics X.

In the first stage, federal district maximizes

$$\max_{f \geq 0} \mu d(e(f), f) - K(f, P) \tag{A.0.4}$$

where $\mu = \psi Z$ is a preference parameter that depends on characteristics of the county relevant for the decision of federal district but not for that of the local government. P is a shifter of the cost of federal enforcement and represents our federal policy change. I treat the Obama guidelines as an increase in P. Robust comparative statics deliver the following result

Proposition 1. *Local enforcement e and federal enforcement f are strategic complement (substitute) if and only if $\theta > \rho$ ($\theta < \rho$).*

Moreover, e_f is increasing in θ.

Thus, characteristics of the county determine the sign and the degree of complementarity. This result is intuitive. More risk averse counties (low θ) will tend to substitute more whereas enforcement will be complement if the level of risk aversion is sufficiently low with respect to the technical elasticity of substitution ρ.

Now, I consider comparative statics with respect to P.

- f is decreasing in P
- e is decreasing in P iff $\theta > \rho$
- $|e_P|$ is increasing in X if $\lambda > 0$

In this specific context, I will treat the democratic share of voters as X. Then, local enforcement will decrease relatively more in democratic counties if $\lambda > 0$.

B Tables and Figures

Table B.1: Summary statistics

Variable	Mean	Std. Dev.	N
Enforcement			
Local: ICE arrests over detainers	1.065	0.901	20220
Federal: detainers over non-citizen arrests	0.177	0.253	33256
Total: ICE arrests over non-citizen arrests	0.19	0.285	33256
County Characteristics			
non citizen %	0.027	0.036	3066
hispanic non citizen %	0.019	0.031	3065
democrat 2008 presidential election %	0.415	0.137	3066
democrat 2012 presidential election %	0.384	0.146	3066
bachelor %	0.201	0.089	3065
urbanization index	0.624	0.485	3066
services %	0.599	0.079	3065
Outcomes			
clearance rate violent crimes	0.571	0.342	68469
clearance rate property crimes	0.205	0.184	69543
violent crimes per 100,000 people	225.882	296.272	79713
property crimes per 100,000 people	444.433	445.252	79713

Table B.2: Predictors of Late Activation in Secure Communities

	(1) month activation	(2) month activation	(3) month activation
Democrat	-11.163***	-6.487***	-0.298
	(1.254)	(1.520)	(1.720)
Bachelor		-12.297***	-8.905***
		(2.080)	(2.111)
Services		6.454*	0.772
		(3.875)	(3.916)
Rural		4.050***	3.586***
		(0.503)	(0.503)
Non Citizen			-33.385***
			(4.472)
Constant	32.979***	29.174***	31.157***
	(0.682)	(2.063)	(2.062)
Observations	3065	3065	3065
Adjusted R^2	0.587	0.607	0.613

Notes: The dependent variables is the month of activation in the Secure Communities program. **Democrat** is the share of voters for the Democratic Party in the 2008 presidential election. **Non Citizen** is the share of non citizen in a county measured with Census 2010 data. Regressions are weighted by 2010 population. Other county characteristics include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.3: Variation at Federal District Level in Federal Enforcement D/M

aor	federal enforcement D/M	aor	federal enforcement D/M
BUF	.06	SLC	.153
BOS	.079	SND	.16
CHI	.085	WAS	.168
NEW	.092	DEN	.169
ELP	.096	SEA	.185
SPM	.106	ATL	.203
BAL	..106	PHO	.215
DET	.113	SFR	.229
MIA	.117	HOU	.244
NOL	.118	LOS	.264
NYC	.121	DAL	.287
PHI	.126	SNA	.389

D/M = detainers / local arrests of non citizen. Period 2013-2014

Table B.4: Effect of Obama guidelines on enforcement

	(1) local	(2) local	(3) federal	(4) federal	(5) total	(6) total
Democrat × post	-0.428***	-0.399**	0.010	0.043	-0.089**	-0.038
	(0.120)	(0.191)	(0.040)	(0.043)	(0.037)	(0.046)
Extra Controls		X		X		X
Observations	17736	17736	28926	28926	28926	28926

Notes: The dependent variables are the local enforcement which is the ratio of ICE arrests over detainers, the federal enforcement which is the ratio of detainers over immigrant arrests and total enforcement which is the ratio of ICE arrests over immigrant arrests. **post** is an indicator equal to one for months after the Obama guidelines. **Democrat** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effects and federal district and state dummies interacted with time dummies. Regressions are weighted by 2010 population. In column (2), (4) and (6) I include county-level controls interacted with time dummies. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.5: Effect of Obama Guidelines on Enforcement (hyp transformation)

	(1) local	(2) local	(3) federal	(4) federal	(5) total	(6) total
Democrat × post	-0.285***	-0.288**	0.007	0.039	-0.087**	-0.041
	(0.071)	(0.115)	(0.038)	(0.041)	(0.035)	(0.043)
Extra Controls		X		X		X
Observations	13244	13244	21350	21350	21350	21350

Notes: The dependent variables are the local enforcement which is the ratio of ICE arrests over detainers, the federal enforcement which is the ratio of detainers over immigrant arrests and total enforcement which is the ratio of ICE arrests over immigrant arrests. I transform the variables with inverse hyperbolic sine transformation. **post** is an indicator equal to one for months after the Obama guidelines. **Non Citizen** is the share of non citizen in a county measured with Census 2010 data. **Democrat** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effects and federal district dummies interacted with time dummies. Regressions are weighted by 2010 population. In column (2), (4) and (6) I include county-level controls interacted with time dummies. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.6: Effect of Obama Guidelines on Enforcement. Non Serious Crimes.

	(1) local	(2) local	(3) federal	(4) federal	(5) total	(6) total
Democrat $\times$ post	-0.338***	-0.346**	0.028	0.046	-0.049	-0.045
	(0.114)	(0.138)	(0.043)	(0.046)	(0.036)	(0.039)
Extra Controls		X		X		X
Observations	16495	16495	27507	27507	27507	27507

Notes: The dependent variables are the local enforcement which is the ratio of ICE arrests over detainers, the federal enforcement which is the ratio of detainers over immigrant arrests and total enforcement which is the ratio of ICE arrests over immigrant arrests. **Guidelines** is an indicator equal to one for months after the Obama guidelines. **Non Citizen** is the share of non citizen in a county measured with Census 2010 data. **Democrat** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effects and federal district dummies interacted with time dummies. Regressions are weighted by 2010 population. In even columns, I include county-level controls interacted with time dummies. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.7: Correlation Local Policies and Democratic Share of Voters

	(1) nodetainer	(2) nodetainer
Democrat	0.373***	0.232***
	(0.027)	(0.029)
Non Citizen		0.000***
		(0.000)
Rural		-0.013
		(0.008)
Services		-0.047
		(0.058)
Bachelor		0.366***
		(0.050)
Constant	-0.107***	-0.093***
	(0.012)	(0.032)
Observations	3067	3063
Adjusted R^2	0.057	0.147

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.8: Effect of Obama Guidelines on Policing and Crime

	(1) clearance violent	(2) clearance violent	(3) clearance property	(4) clearance property	(5) violent crimes	(6) violent crimes	(7) property crimes	(8) property crimes
NonCit × post	0.236***	0.296***	0.023	0.080	-1.400	-2.308	-2.138	-3.064
	(0.091)	(0.090)	(0.051)	(0.053)	(1.878)	(1.952)	(2.184)	(2.312)
Extra Controls		X		X		X		X
Observations	64824	64824	65695	65695	72093	72093	72093	72093
Adjusted R^2	0.837	0.837	0.728	0.731	0.620	0.622	0.587	0.590

Notes: The dependent variables are the clearance rate for violent crimes which is the ratio of clearances (arrests) over reported crimes, clearance rate for property crimes and the inverse hyperbolic sine transformations of violent and property crimes. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. **Democrat** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effects, and federal district and state dummies interacted with time dummies. Regressions are weighted by 2010 population. In even columns, I include county-level controls interacted with the guidelines dummy. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.9: Effect of Obama Guidelines on Crime Rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	viol	viol	prop	prop	hyp viol	hyp viol	hyp prop	hyp prop
NonCit × post	-271.89**	-346.80***	-678.87**	-866.71***	-0.780	-1.417	-1.553	-2.232
	(122.66)	(124.72)	(304.94)	(313.17)	(1.291)	(1.331)	(1.508)	(1.575)
Extra Controls		X		X		X		X
Observations	63081	63081	63081	63081	63081	63081	63081	63081
Adjusted R^2	0.962	0.962	0.939	0.939	0.757	0.757	0.725	0.726

Notes: The dependent variables are violent and property crimes per capita. Columns 5 to 8 present results for the inverse hyperbolic sine transformations of the crime rates. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. In all the specifications, there are county and time fixed effectsand federal district and state dummies interacted with time dummies. Regressions are weighted by 2010 population. In even columns, I include county-level controls interacted with time fixed effects. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.10: Effect of Obama Guidelines on Policing and Crime. Triple Difference

	(1) clearance violent	(2) clearance violent	(3) clearance property	(4) clearance property	(5) violent crimes	(6) violent crimes	(7) prop crimes	(8) prop crimes
NonCit × post	-0.340	-0.255	-0.117	0.065	-142.4	-283.4	-1188.9	-1625.1
	(0.238)	(0.241)	(0.165)	(0.184)	(548.2)	(579.0)	(1250.4)	(1358.5)
Dem × post	-0.104***	-0.081**	0.013	0.025	6.6	-15.3	-46.4	-85.0
	(0.035)	(0.036)	(0.049)	(0.042)	(44.4)	(45.6)	(95.8)	(99.0)
NonCit × Dem × post	1.105***	1.000***	0.179	-0.02	-205.8	-67.1	842.9	1273.0
	(0.362)	(0.359)	(0.300)	(0.314)	(960.8)	(979.5)	(2153.6)	(2226.9)
Extra Controls		X		X		X		X
Observations	64478	64478	65348	65348	71637	71637	71637	71637
Adjusted R^2	0.837	0.838	0.728	0.731	0.954	0.955	0.931	0.933

Notes: The dependent variables are the clearance rate for violent crimes which is the ratio of clearances (arrests) over reported crimes, clearance rate for property crimes and the inverse hyperbolic sine transformations of violent and property crimes. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. **Dem** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effectsand federal district and state dummies interacted with time dummies. Regressions are weighted by 2010 population. In even columns, I include county-level controls interacted with time fixed effects. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.11: Effect of Obama Guidelines on Crime Rates. Triple Difference.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	viol	viol	prop	prop	hyp viol	hyp viol	hyp prop	hyp prop
NonCit × post	-142.4	-283.4	-1188.9	-1625.1	-5.134	-6.008	-6.606	-7.558
	(548.2)	(579.0)	(1250.4)	(1358.5)	(5.113)	(5.400)	(5.832)	(6.188)
Dem × post	6.6	-15.3	-46.4	-85.0	-0.519	-0.859	-0.648	-1.003
	(44.4)	(45.6)	(95.8)	(99.0)	(0.629)	(0.650)	(0.723)	(0.739)
NonCit × Dem × post	-205.8	-67.1	842.9	1273.0	7.512	8.390	8.820	9.746
	(960.8)	(979.5)	(2153.6)	(2226.9)	(9.351)	(9.477)	(10.615)	(10.758)
Extra Controls		X		X		X		X
Observations	62682	62682	62682	62682	62682	62682	62682	62682
Adjusted R^2	0.962	0.962	0.939	0.939	0.757	0.757	0.725	0.726

Notes: The dependent variables are violent and property crimes per capita. Columns 5 to 8 present results for the inverse hyperbolic sine transformations of the crime rates. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. **Dem** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effectsand federal district and state dummies interacted with time dummies. Regressions are weighted by 2010 population. In even columns, I include county-level controls interacted with time fixed effects. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors clustered at county level in parentheses
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.12: Effect of Obama Guidelines on Murders and Manslaughters

	(1) murders	(2) murders	(3) manslaughters	(24 manslaughters
NonCit × post	-2.953	-3.725	1.441	1.398
	(2.975)	(3.271)	(1.587)	(1.700)
Dem × post	-0.227	-0.429	-0.066	-0.095
	(0.366)	(0.366)	(0.121)	(0.120)
NonCit × Dem × post	3.700	4.534	-1.105	-1.097
	(5.291)	(5.498)	(2.582)	(2.637)
Extra Controls		X		X
Observations	71637	71637	71637	71637
Adjusted R^2	0.915	0.917	0.663	0.665

Notes: The dependent variables are murders and manslaughters crimes. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. **Dem** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effectsand federal district and state dummies interacted with time dummies. Regressions are weighted by 2010 population. In even columns, I include county-level controls interacted with time fixed effects. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.13: Effect of Obama guidelines on expenditures

	(1) log police	(2) log judicial	(3) log correct	(4) police share	(5) judicial share	(6) correct share
NonCit × Dem × post	-1.183**	0.337	-0.679	0.009	0.118**	-0.005
	(0.587)	(0.497)	(1.965)	(0.052)	(0.047)	(0.066)
Dem × post	-0.291**	0.035	-0.250*	-0.005	0.022*	-0.006
	(0.122)	(0.116)	(0.138)	(0.009)	(0.012)	(0.010)
NonCit × Dem × post	2.052**	-0.400	1.077	-0.051	-0.243***	-0.002
	(1.007)	(0.812)	(2.768)	(0.088)	(0.085)	(0.114)
Observations	11946	11751	10637	11949	11758	10787
Adjusted R^2	0.988	0.989	0.975	0.908	0.902	0.904

Standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Notes: The dependent variables are the log of county expenditure in police, judicial and correctional. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. **Dem** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effectsand federal district and state dummies interacted with time dummies. Regressions are weighted by 2010 population. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.14: Effect of Obama Guidelines on Clearance Rate for Violent crimes. Specification Tests.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
NonCit × Dem × post	1.105***	1.467***	1.109***	1.232*		1.373**	1.040***
	(0.362)	(0.415)	(0.364)	(0.656)		(0.657)	(0.323)
Dem maj × post				-0.015			
				(0.014)			
Dem maj × NonCit × post				-0.104			
				(0.217)			
Hispan × Dem × post					1.386***		
					(0.475)		
	baseline	active before guidelines	common support noncitizen	non linearity	hispanic noncitizen	county specific linear trends	hyperbolic
Observations	64478	30037	64437	64478	64478	64478	64478
Adjusted R^2	0.837	0.844	0.837	0.837	0.837	0.850	0.859

Notes: The dependent variable is the clearance rate of violent crimes which is the ratio of clearances (arrests) over reported crimes. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. **Dem** is the share of voters for the Democratic Party in the 2008 presidential election. **Hispan** is the share of hispanic non citizen in a county measured with Census 2010 data. **Dem maj** is a dummy for counties where the share of Democrat voters is higher than Republican. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.15: Effect of Obama Guidelines on Clearance Rate for Violent Crimes. Alternative Explanations.

	(1) clearance violent	(2) clearance violent	(3) clearance violent	(4) clearance violent
NonCit × Dem × post	1.031***	1.107***	1.145***	1.099***
	(0.381)	(0.361)	(0.368)	(0.350)
Officers	-0.000			
	(0.000)			
Bartik		-0.002		
		(0.066)		
E-Verify enrolled			-0.009*	
			(0.005)	
287g × post				-0.002
				(0.015)
Observations	64478	64452	59325	64478
Adjusted R^2	0.837	0.837	0.840	0.837

Notes: The dependent variable is the clearance rate of violent crimes which is the ratio of clearances (arrests) over reported crimes. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. **Dem** is the share of voters for the Democratic Party in the 2008 presidential election. **Officers** is the log number of police sworn officers. **Bartik** is the Bartik shock as defined in text. **E-Verify Enrolled** is the log number of firms enrolled in E-Verify in that county. **287g** is a dummy for a county ever participating in the 287g program. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B.16: Effect of Trust Act on Enforcement, Policing and Crime

	(1) local	(2) federal	(3) total	(4) clearance viol	(5) crime viol	(6) clearance prop	(7) crime prop
CA × Post Trust	-0.309***	-0.008	-0.067***	0.078***	13.308	0.030***	-7.289
	(0.066)	(0.008)	(0.012)	(0.016)	(12.252)	(0.007)	(28.166)
Observations	14316	24276	24276	32476	36058	32906	36058
Adjusted R^2	0.342	0.432	0.362	0.341	0.912	0.441	0.848

Notes: The dependent variable is the clearance rate of violent crimes which is the ratio of clearances (arrests) over reported crimes. **CA** is a dummy for counties in California. **Post Trust** is an indicator equal to one for months after the California Trust Act. In all the specifications, there are county and time fixed effects and a county specific linear time trend. Regressions are weighted by 2010 population. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

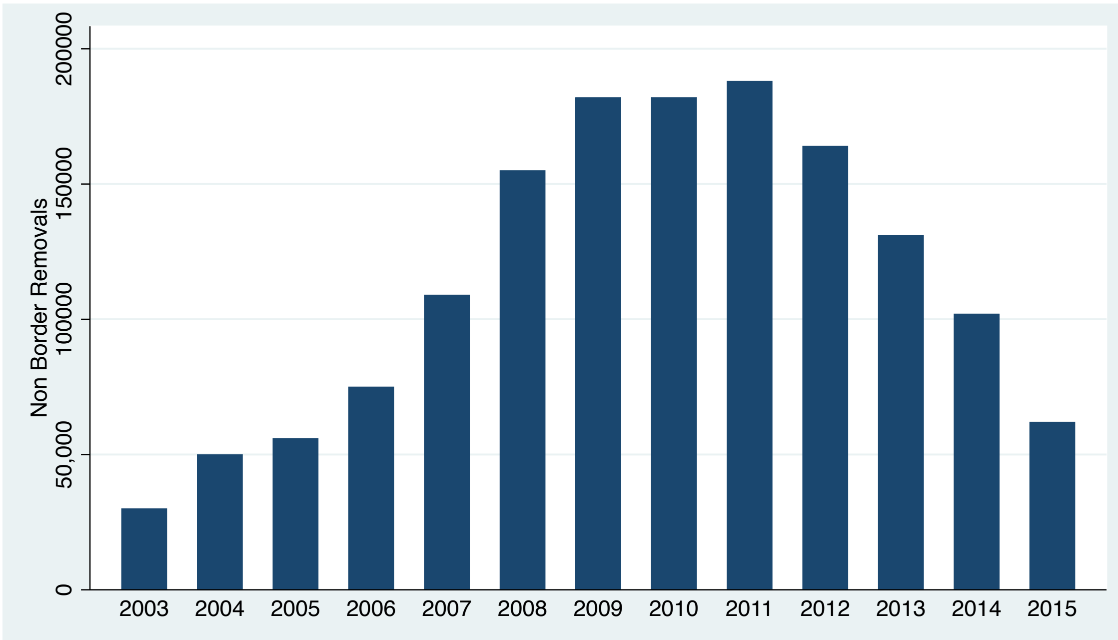
Table B.17: Effect of Trust Act on Policing and Crime. Triple Difference.

	(1) clearance violent	(2) clearance violent	(3) clearance property	(4) clearance property	(5) violent crimes	(6) violent crimes	(7) prop crimes	(8) prop crimes
NonCit × Dem × post	1.299**	1.185*	0.488***	0.399	10.84	16.12	14.02	19.16
	(0.599)	(0.625)	(0.184)	(0.262)	(19.193)	(17.66)	(21.32)	(18.92)
NonCit × Trust	-0.025	-0.004	0.167**	0.189**	-3.26	-4.42	-2.34	-3.46
	(0.086)	(0.092)	(0.082)	(0.087)	(3.61)	(3.90)	(3.89)	(4.22)
NonCit × CA × Trust	0.340***	0.296**	-0.095	-0.122	9.60	10.03	9.33	9.67
	(0.131)	(0.137)	(0.093)	(0.096)	(6.68)	(6.69)	(7.36)	(7.34)
Observations	64478	64478	65348	65348	71637	71637	71637	71637
Adjusted R^2	0.878	0.878	0.800	0.801	0.677	0.678	0.650	0.651

Notes: The dependent variable is the clearance rate of violent crimes which is the ratio of clearances (arrests) over reported crimes. **CA** is a dummy for counties in California. **Trust** is an indicator equal to one for months after the California Trust Act. **post** is an indicator equal to one for months after the Obama guidelines. **NonCit** is the share of non citizen in a county measured with Census 2010 data. **Dem** is the share of voters for the Democratic Party in the 2008 presidential election. In all the specifications, there are county and time fixed effects, a county specific linear time trend and federal district and state dummies interacted with time dummies. In even columns, I include county-level controls interacted with time dummies. Those include share of population with a bachelor degree, share of the services industry and a measure of urbanization. Regressions are weighted by 2010 population. Standard errors clustered at county level in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figures

Figure B.1: Non-Border Removals by Year



Source: ICE

Figure B.2: Local Policies (September 2014)

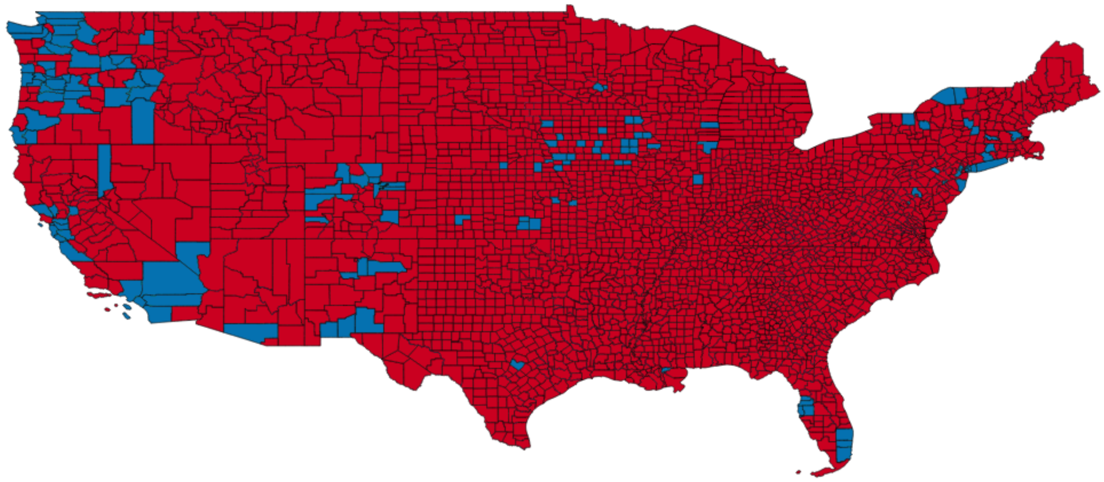
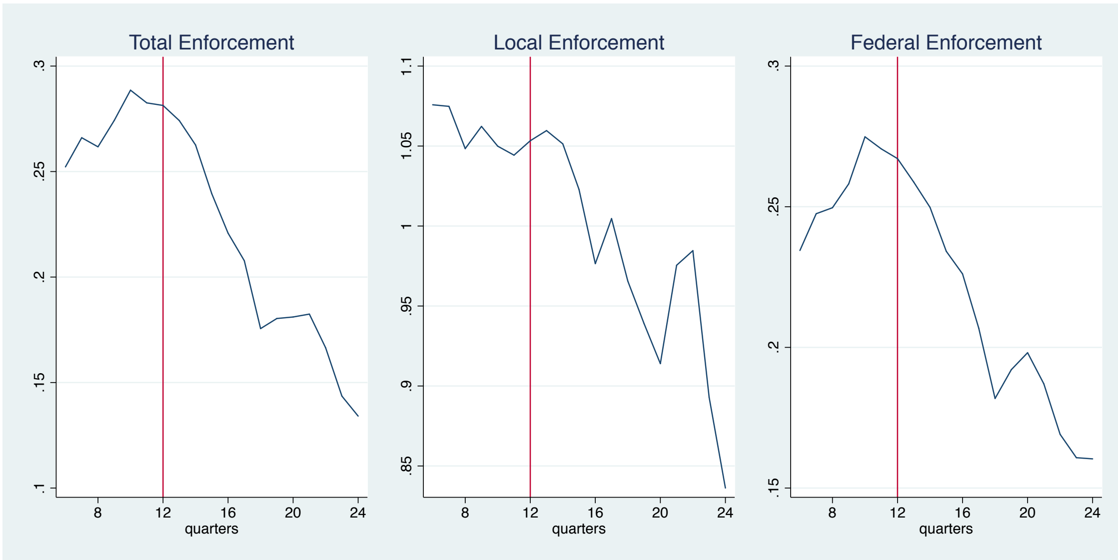
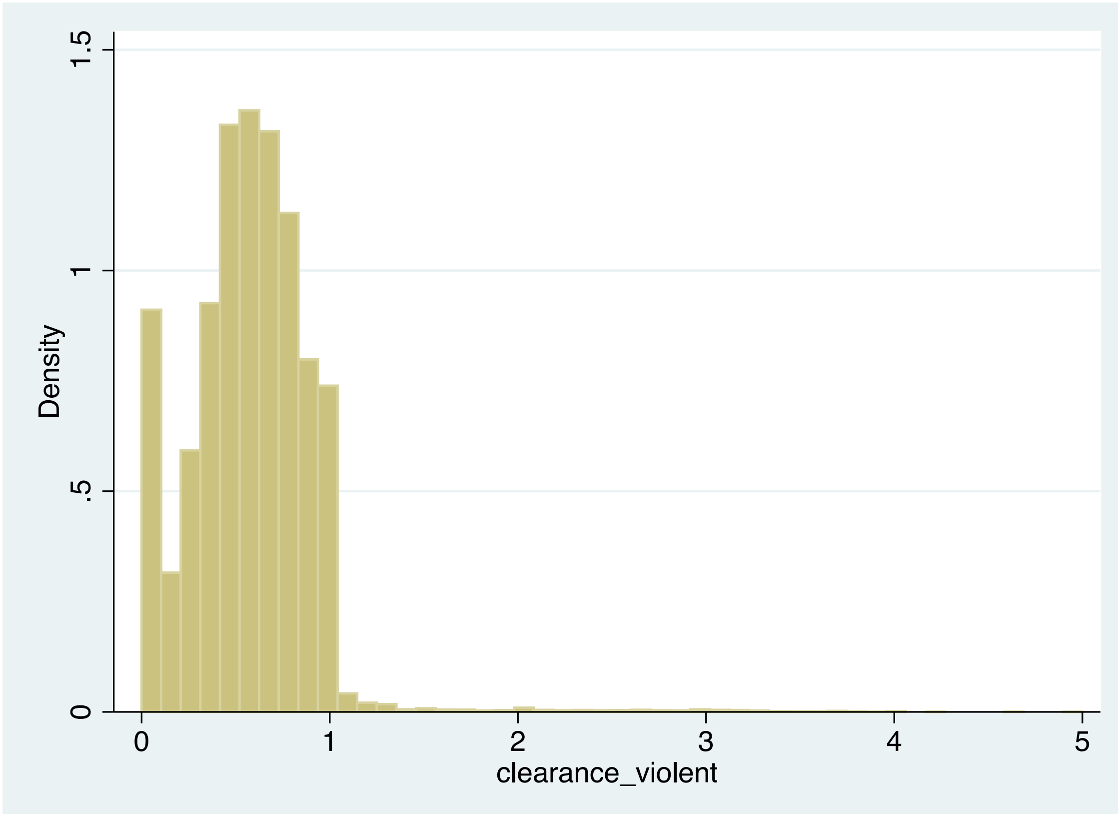


Figure B.3: Different Types of Enforcement by Quarter



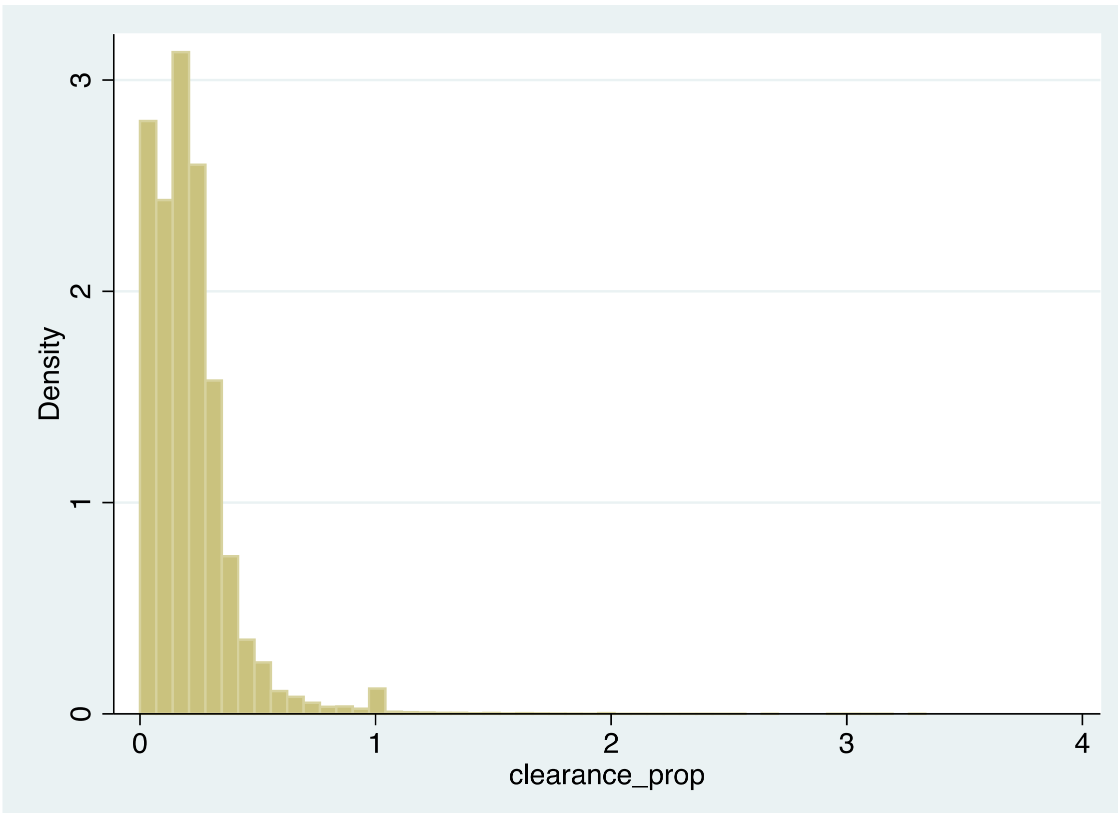
Red line: Obama guidelines. Green line: Trust Act Average weighted by 2010 population of counties enrolled in Secure Communities before May 2010.

Figure B.4: Clearance rate for violent crimes



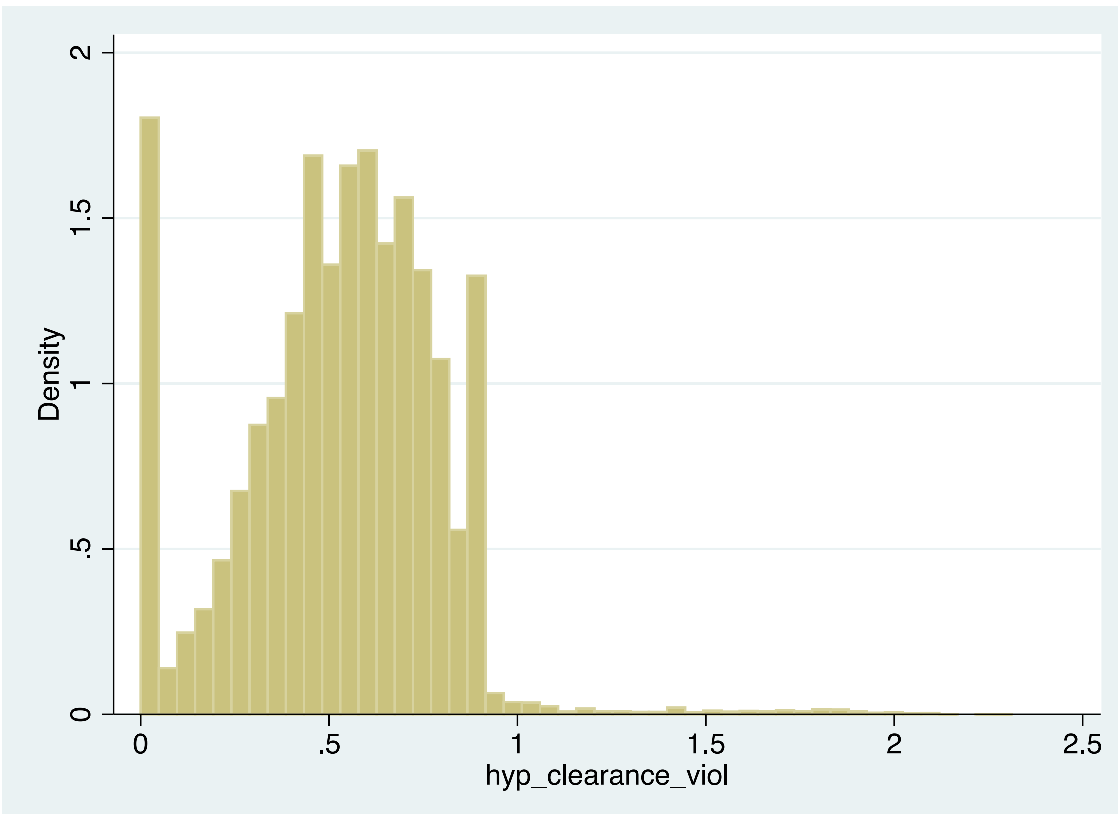
Violent crimes: murder, manslaughter, rape, assault, robbery

Figure B.5: Clearance rate for property crimes



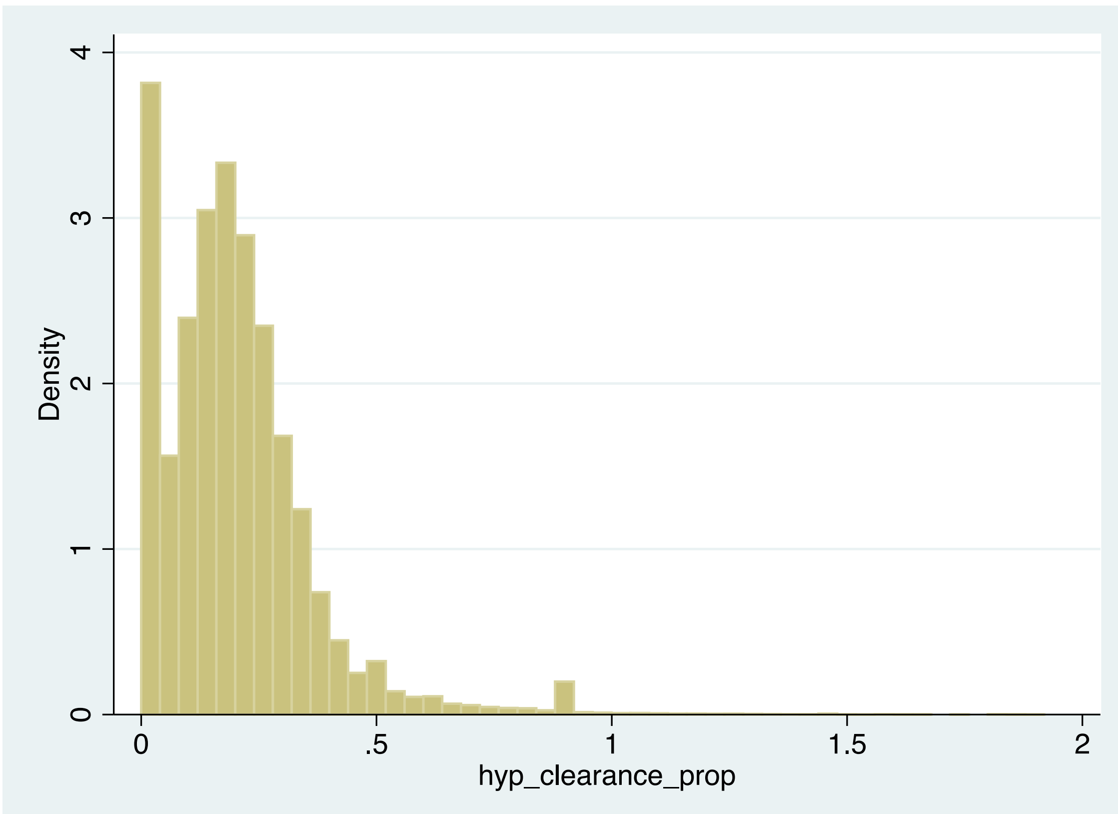
Property crimes: larceny, burglary, motor vehicle theft

Figure B.6: Clearance rate for violent crimes. Inverse hyperbolic sine transformation



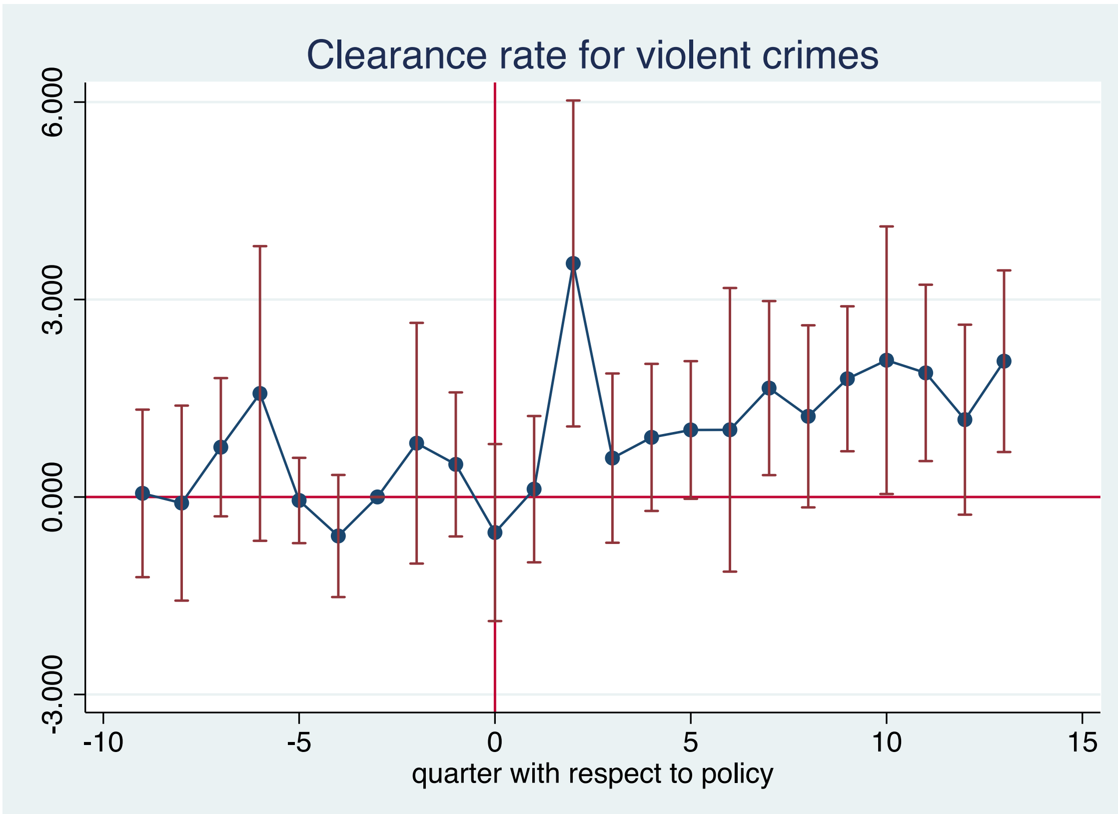
Violent crimes: murder, manslaughter, rape, assault, robbery

Figure B.7: Clearance rate for property crimes. Inverse hyperbolic sine transformation



Property crimes: larceny, burglary, motor vehicle theft

Figure B.8: Event Studies Estimates for Clearance Rate of Violent Crimes



Notes: The graph shows coefficients (95% confidence intervals) on interactions between the non citizen share, democratic share and the Obama guidelines. The specification includes county fixed effects, district and state dummies intereacted with time fixed effects. Interaction with one quarter before the policy is omitted to identify the model. Specification is weighted by 2010 county population. The vertical line is the the quarter right before the policy change, April-June 2011.

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