Can Immigration Enforcement Policies Induce Labor Market Discrimination? Evidence from Secure Communities

by

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Abstract

Immigration policies have been increasingly targeting the undocumented population in the United States, potentially elevating employer prejudice against this group and inducing labor market discrimination. Using ACS data and reports from ICE, I test whether Secure Communities (SC), a deportation program initiated in 2008, had spillover effects on labor market outcomes of Hispanic male citizens and legal immigrants. Using a difference-in-difference regression, I estimate the effect of SC between the pre-treatment period (2007) and the post-treatment period (2010) on annual earnings of my treatment group. The latter consists of Hispanic male citizens and legal immigrants working in SC counties, and my control group consists of Hispanic male citizens and legal immigrants working in non-SC counties. I consider any negative change in wages or annual earnings as evidence in support of labor market discrimination. I find that Hispanic men working in treated counties witnessed a decline in their annual earnings of 3.4% in magnitude as a result of the implementation of SC. I find no significant effect of SC on employment of Hispanic men, and no significant gender or border (states that border Mexico versus those that do not) differences in my treatment group.
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1. Introduction

Public debate around immigration enforcement policies has been increasingly centered around the effect of such policies on labor market consequences for different groups in the US. In recent years, the number of undocumented immigrants in the United States has decreased from an all-time high of 12.2 million people in 2007 to 10.5 million people as of 2017, which is the lowest figure since 2004 (Pew Research Center, 2018). The past couple of decades have also witnessed increasing efforts in the US aimed at reducing the number of undocumented immigrants through the implementation of many immigration enforcement and deportation programs as well as through greater control of US borders. These programs have targeted Hispanic undocumented immigrants in particular because this demographic group constitutes the vast majority of the undocumented population, given the U.S./Mexico border and the U.S.’s geographical proximity to Latin America.

The response of the general public to immigration enforcement and deportation programs and policies has varied. While these programs and policies have been met with some backlash and have sparked a great deal of public debate around their human rights implications, their efficacy and the extent of their benefits to American society have also been met with support and enthusiasm from certain population segments. Such programs and policies have seen a notable rise in support within the past four years under the Trump administration, which has been devoted to implementing especially strict immigration agendas (ADL, 2020). Many argue that this has fueled anti-immigrant sentiments across the United States (ADL, 2020) and has led to a rise of discriminatory behavior against Hispanics.

To the extent that they invoke xenophobic sentiments, deportation policies may also impact the lives of Hispanics that legally live and work in the United States. About 40% of Hispanics surveyed by the Pew Research Center in 2018 reported one of the following incidents: being called offensive names, being told to go back to their home country, being
criticized for speaking Spanish in public, or experiencing discrimination or unfair treatment because they are Hispanic (Pew Research Center, 2018). The survey also revealed that a majority of Hispanics worry about getting deported, regardless of their legal status. These responses suggest the possibility of discrimination against US Hispanics, including citizens and legal residents, within the realm of the labor market. Given that documented Hispanic immigrants made up 17.5% of the US labor force as of 2018 (US BLS, 2018), there is a clear need for a study of the effects of recent immigration enforcement policies on labor market outcomes, in particular the manifestation (or lack thereof) of labor discrimination in the context of implementing deportation programs.

This paper aims to investigate the effects of Secure Communities (SC), a deportation program implemented from 2008-2012, on the annual earnings of Hispanic men in the United States. Secure Communities, which relies on partnerships with local law enforcement agencies and the federal government to remove undocumented immigrants from US territory, provides a unique natural experiment for my study. I define ‘Hispanic men’ in the US to be citizens and legal immigrants who identify as male and who are of Hispanic or Latino ethnicity. I focus on men because there is reason to believe that women’s participation in the workforce can be heterogeneous and can substantially vary due to factors such as pregnancy and maternity leave. As for my main labor outcome, I focus on annual earnings solely from salary and wage income because doing so facilitates an examination of employer-employee relationships that are the most susceptible to discrimination in the context of this thesis.

I use the 2007 and 2010 American Community Survey (ACS) public micro-data drawn from the IPUMS website for my empirical strategy, as well as a report published by ICE containing the SC activation dates by county. My methodology consists of a difference-in-difference model that tests the effect of the treatment (SC) on my treatment group, composed of Hispanic men in the US who work in counties where SC was implemented by 2010, versus
my control group, which is composed of Hispanic men in the US who work in counties where SC had not been implemented. I first examine the effects of this program on my main outcome, which is annual earnings of Hispanic men. I then support my findings by investigating whether SC had an effect on Hispanic men’s employment. This not only accounts for the unemployed that were not taken into consideration in my main outcome (since they could not indicate a place of work) but also helps me elucidate the effect observed on men’s annual earnings. I also run the same main regression but on women’s annual earnings to test for robustness and gender differences. Finally, I check for geographical heterogeneity by dividing my treatment group into counties in states that border Mexico and counties in states that do not to test whether the program had a more pronounced effect in the South, where the concentration of Hispanics is high relative to the rest of the U.S.

For my analysis, I choose to focus on spillover effects on Hispanic-Americans and legal Hispanic immigrants, despite the fact that the SC program deports undocumented Hispanics. This is because the SC program may have fueled existing prejudice against Hispanics in certain employers, who might have felt justified to act from such prejudice because the US immigration enforcement authorities are mandated to detain and remove undocumented Hispanics. It is important to underscore that the notion of perception plays a central role in cultivating and sustaining the stereotypical appearance of being of Hispanic ethnicity and that as such, perception-based discrimination may importantly contribute to the phenomenon of labor discrimination towards Hispanics and Hispanic-Americans in the US.

Perception may play a central role in the phenomenon of labor discrimination against undocumented Hispanics, Hispanic-Americans, and legal Hispanic immigrants because, to an important extent, these demographic group all possess similar physical appearances as well as Hispanic or Latino-originating or -sounding names. These resemblances may put documented and undocumented Hispanics in the US at a comparatively similar risk of ethnic profiling. The
assumption I make above is supported by multiple psychology papers that have explored the phenomenon of perceived discrimination. Perceived discrimination occurs when an individual is discriminated against based on the perception that they are a member of a certain religious, ethnic, social, or political group. Ahluwalia and Pelletiere (2010) investigate the phenomenon of perceived discrimination by analyzing the experience of Sikh men post 9/11. This group usually possesses certain physical characteristics that are stereotyped as quintessentially Muslim or Islamic, such as the pervasive maintenance by male Sikhs of their beards and their near-constant adornment of turbans. Ahluwalia and Pelletiere’s findings indicate that Sikhs were discriminated against post 9/11 indeed because they were perceived as belonging to the Islamic faith. My analysis investigates whether Hispanic men legally in the US have been similarly negatively perceived and discriminated against in the labor market, and whether such perception and discrimination is causally connected to the implementation and development of Secure Communities, a program that deals with individuals, in many cases Hispanics, that have illegally entered the country.

I find that Secure Communities is associated with a 3.4% decrease in annual earnings of Hispanic men who work in treated counties. I also find that SC had a negative effect on Hispanic women’s annual earnings but to a lesser extent than the earnings of men (-2.8%). As hypothesized, I find that Hispanic men who work in treated countries in border states witness a stronger negative effect on their annual earnings compared to those who work in treated counties in non-border states. However, I find no effect of SC on employment, and no evidence to support the significance of gender or border differences.

The remainder of this paper is structured as follows. Section 2 is a literature review on the topics of immigration enforcement, Secure Communities, and labor market discrimination in general. Section 3 provides a conceptual framework for my analysis, based on simple demand and supply dynamics in the labor market as well as insights from the Becker model.
(Becker, 1957). Section 4 presents a policy background on Secure Communities to better understand why and how this program was implemented. Section 5 describes the key data sources and main outcomes of this paper. Section 6 outlines the empirical strategy with a focus on assumptions needed for the validity of the Difference-in-Difference methodology. Section 7 presents the main results and robustness checks, and Section 8 interprets these results and touches on policy implications. Lastly, Section 9 concludes the main purpose and work of this paper.
2. Literature Review

My work contributes to the literature in various ways. First, my paper aims to contribute to evaluating an important and timely political debate on immigration policies in the US. This is particularly significant as ICE and the Trump administration have received backlash on reactivating the SC program in 2017, three years after its discontinuation (The Atlantic, 2017). My analysis could help supply an important economic perspective for assessing the costs and benefits of a prominent and politically controversial immigration policy.

Second, my paper adds to an existing body of literature on the effects of Secure Communities on various outcomes such as crime and welfare benefits. Alsan and Yang (2018) examine the effects of SC on the demand for safety nets in the United States. They arrive at the conclusion that Hispanic-Americans are themselves sensitive to this immigration enforcement given the demographic, ethnic, and cultural connections between their communities and those of undocumented Hispanics. Alsan and Yang find a negative effect on the demand for safety nets from Hispanic citizens in the US, explained by a fear on the part of Hispanic-Americans of losing their legal status or an undocumented family member or friend. Another paper by Cox and Miles (2015) investigates whether immigration enforcement reduces crime by specifically looking at the SC deportation program, which claims to only remove undocumented immigrants who have a criminal record or who have been arrested. Their results show that the SC program leads to no meaningful reduction in the FBI crime rate index. Although these two papers provide valuable insight about the ramifications and lack thereof of the SC immigration enforcement program, literature that focuses solely on the connection between Secure Communities and its effects on labor markets remains limited.

Third, my thesis complements a body of work on labor discrimination against minorities in the US. Bertrand and Mullanaithan (2004) conduct an experiment by randomly assigning African-American or White sounding names to resumes that were sent to help-
wanted ads in Boston and Chicago. They find that White names receive 50% more callbacks than African-American names, and that this racial gap is uniform across occupation, industry and employer size. Papers like that of Bertrand and Mullanaithan (2004) have established the existence of labor market discrimination in the US. As a result, scholars have therefore attempted to study policies or events that fuel and manifest such behavior by employers in the workplace. Davila and Mora (2005), for instance, provide insight on how 9/11 decreased not only Arab-American earnings, but also the earnings of members of other ethnicities that were perceived as Arab such as Pakistanis and Afghanis, due to a rise in labor market discrimination.

Finally, my thesis is unique in that it explores whether and how immigration policies can induce labor market discrimination. In particular, it looks specifically at changes in annual earnings of Hispanic men after Secure Communities, which has not been studied before.
3. Conceptual Framework

3.1. Dynamics of the Labor Market in the Absence of Discrimination

In the absence of labor market discrimination, I expect the removal of undocumented workers through the Secure Communities program to induce a decrease in the labor supply of undocumented Hispanics in the US. This decrease encompasses many more layers beyond the actual deported undocumented workers, as explored in a paper by East and Velasquez (2018). These scholars detail the following three points. First, Secure Communities might have lowered the availability of undocumented workers through fear of detention. It is plausible, for instance, that many undocumented Hispanics stopped participating in the workforce to minimize interactions with local law enforcement. Second, SC might have influenced many undocumented workers to voluntarily leave the U.S., perhaps because they did not feel secure in their communities given the widespread reach of this program. Third, SC might have also disincentivized prospective undocumented workers, who recognized that their demography was at especially high risk of being targeted by the program, from crossing the US-Mexico border. This leftward shift of the labor supply curve, depicted in Figure 1a, will put upward pressure on the wage rate received by undocumented workers and reduce employment. As a result of this reduction on the supply of undocumented workers, I expect a rightward shift in the labor demand curve of Hispanic citizens and legal immigrants, as shown in Figure 1a, as more employment opportunities become available in the aftermath of the deportation of undocumented Hispanics.

This analysis assumes that undocumented workers act as imperfect substitutes for Hispanic citizens and legal immigrants. Such an assumption is supported by empirical studies that have examined the effects of migration on native wages and employment. Borjas and Katz (2005), for instance, examine the rise of Mexican immigration and its effects on the US labor market. They conclude that the large influx of Mexican migrants to the U.S. in recent decades
has had a sizable adverse effect on the wages of low-skilled Americans under the assumption of labor substitution between the two groups. It is important, however, to note that Ottaviano and Peri (2012) have observed some complementarity between undocumented immigrants and high-skilled American workers during the period of 1990-2004. Note that the mean years of schooling in my Hispanic citizens and legal immigrants sample is around 11 years, which is lower than the national average of 13.7 years of schooling in the U.S. (US Census Bureau, 2019). This is indicative of the fact that a high share of the Hispanic workers in my data are low-skilled, which makes them substitutable for undocumented Hispanic workers. I therefore believe it is safe to assume that Ottaviano and Peri’s conclusion is not prominent in my analysis, and that the complementarity between undocumented Hispanics and Hispanic citizens will play little to no role in the labor supply shifts.

3.2 Dynamics of the Labor Market in the Presence of Discrimination: Becker Model

Labor market discrimination would manifest after the implementation of SC and the removal of undocumented Hispanics in the following way. Acting on their taste-based preferences and prejudice, employers might either fire, not hire, or reduce the wages of Hispanic citizens and legal immigrants. Perception plays a significant role in explaining the thread connecting a policy targeted towards undocumented Hispanics to a potential effect on Hispanic Americans and legal immigrants. Given the similar physical traits, appearances, and names shared by members of the Hispanic population regardless of their legal status, one might attribute stereotypes of criminality and illegal activity fueled by the implementation of SC to citizens and legal immigrants of Hispanic origin as well.

I use the neoclassical Becker “employer taste” model to explain discrimination in this context. Becker (1957) treats tastes or preferences for or against certain ethnic or racial groups similarly to how economists generally analyze individuals’ preferences of goods and services. If an employer does not want to interact or associate with Hispanics, he or she will not employ
any workers from this group (assuming the firm has to pay all workers the same wage). Employers will choose to avoid this population segment, and will perceive their marginal revenue product of labor (the labor demand curve) to be lower than that of other ethnic groups, such as white Americans for instance. Becker notes that not only will the disadvantaged group suffer from lower wages and unemployment, but the employers will suffer consequences too. The increase in discrimination and the availability of relatively cheap Hispanic labor will encourage non-discriminatory firms to enter the market and take advantage of this labor demography. Such firms will be able to produce more output than discriminatory firms because their costs of labor will be relatively cheaper. This will in turn negatively impact the competitiveness of the discriminatory firms, which will bear the brunt of their discrimination and suffer economic consequences such as profits losses and potentially bankruptcies. Therefore, the important conclusion to take away from this model is that in the long run discrimination can only persist if firms operate in non-competitive labor and product markets.

Discriminating against Hispanic labor in the context of this study will therefore translate into a decrease in labor demand as represented by a leftwards shift of the labor demand curve for Hispanic citizens and legal immigrants, as shown in Figure 1b. Instead of only witnessing a shift (arrow 1) from D₁ to D₂, the demand curve will also move to D₃ (arrow 2) as a result of discriminating against Hispanics in accordance with the Becker model. It is important to acknowledge that both effects (D₁ to D₂ and D₂ to D₃) might be at play at the same time and might be of different magnitudes. Whether the employment level will be lower than E₁ remains ambiguous, as that will depend on the magnitude of the shift to D₃. In this paper, I will consider a lower wage, as a result of a leftward shift in the demand curve, as evidence in favor of labor market discrimination against Hispanics.
3.3 The Elasticity of the Hispanic Supply Curve

Thus far, this analysis has assumed that the labor supply curve of Hispanics in the US is fairly elastic because this group is composed of a high percentage of low-skilled workers. These workers tend to be more responsive to changes in wages as compared to high-skilled workers. When the minimum skill of labor needed is relatively low, the pool of worker availability becomes large, which in turn makes the supply curve elastic. However, this might not hold for my treatment group for the following reasons. First, labor supply tends to be less elastic in the short run as workers need time to retrain or respond to changes in the labor market. Since my post-treatment sample year is only 2 years after the first official implementation of Secure Communities in 2008, I believe that this does not leave much room to Hispanic citizens and legal immigrants for a heightened sensitivity to wage changes, especially because it might take time for discrimination to fully manifest in the labor market. Second, given the tense political climate surrounding immigration policies, as well as the rise of discriminatory incidents against Hispanics (Pew Research Center, 2018), workers of this ethnic group might not be willing to change or resign from their jobs even when faced with a lower wage. The lack of security in their communities may paralyze their ability to freely participate in the workforce. And finally, the recession of 2008 has created a tense economic climate where Hispanic men feel obliged, though not necessarily begrudingly, to accept wage cuts because the alternative could be worse (i.e. losing one’s job). For these reasons, Hispanic male citizens and legal immigrants might face an inelastic supply curve. If this is the case, I will observe large changes in wages or earnings but not in employment levels.
4. Policy Background

Secure Communities is one of the largest immigration enforcement policies implemented in the last two decades, removing over 688,600 undocumented immigrants, out of which 96% are of Hispanic origin (TRAC, 2019). Initiated and managed by the US Immigration and Customs Enforcement Agency (ICE), it relies on partnerships with federal and local law enforcement agencies and aims to identify jails detainees who are deportable under US immigration law. According to the ICE website, this program aims to improve national security by removing public safety threats from local communities (ICE Government Website, 2018). The program states that its objectives are to remove individuals who have violated the nation’s immigration laws, who have failed to comply with a final order of removal, and who have engaged in fraud or willful misrepresentation in connection with official government matters.

This program was implemented on a county-by-county basis due to technological constraints, starting in 2008 and achieving full activation in all 3007 counties in January of 2013. The chronology of the program’s implementation depended on whether the county’s jurisdictions had live scan fingerprint devices (Alysan and Yang, 2018). I discuss other non-technological reasons that influenced which counties received the program first in the methodology section.

In an SC county, local law enforcement runs fingerprints of arrested individuals, and sends them not only to FBI criminal databases but also to ICE, which in turn verifies in their database whether an arrested individual matches an alien profile or a record indicating a previous immigration violation. The authorities in most cases issue a detainer, otherwise known as an immigration hold, to keep the individual for an additional 48 hours in order to decide on whether or not to initiate deporting procedures as the Department of Homeland Security (DHS) sees fit. A Secure Communities review in 2011 revealed that many removals were on a basis
unrelated to the arrest, such as overstaying visas (Alysan and Yang, 2018). SC therefore increases the likelihood of an undocumented immigrant getting deported, conditional on their arrest.

The program continued to run until it was initially discontinued in 2014. This was mainly due to the backlash that it has received from the media and many NGOs, as well as the resistance of sanctuary cities (Alysan and Yang, 2018). California legislators, for example, argue that the program does not provide enough legal protection for the detainees, and that this has discouraged Hispanic victims and witnesses from reporting crimes (USA Today, 2013). The “detainer” component of the program has been especially controversial. Immigrant advocates claim that this practice leads to racial profiling and the detention of many immigrants with no criminal record. Per an executive order signed by President Trump, the program was restarted in January 2017.
5. Data

5.1 Data Sources

In order to measure the program’s effects on the labor market for Hispanic men in the U.S., I merged information extracted from both ICE’s published reports (taken down in the last few years, extracted from online archives) and the American Census Survey (ACS). The report that I rely on for my data contains information about the activation date of SC by county from the start date in 2008 to nationwide activation coverage in 2013. I use these SC activation dates to construct my treatment and control group, which are composed of Hispanic men who work in SC counties and those who work in non-SC counties respectively. As for the ACS survey, I extract data from ACS sample years 2008 and 2011, sample years which correspond to the years 2007 (pre-SC) and 2010 (post-SC) respectively. This county-level data includes labor market variables for the Hispanic population such as annual earnings and employment levels as well as demographic and personal characteristics such as gender and age.

I choose to analyze the year 2010 as my post-treatment year, as opposed to 2009 or 2011 for example, for two reasons. First, analyzing 2010 as my post-treatment year increases the chances of observing the possible causal effects of Secure Communities, since it allows sufficient time for such possible effects of the program to manifest in the labor market. Second, choosing 2010 as a sample year ensures my being able to obtain a large enough sample size to conduct the investigation, since not enough counties joined in 2008 and 2009.

I construct my final sample in the following way: I first limit the age of a Hispanic worker to a range between 16 and 60, as this is the subpopulation that I am most interested in investigating with regards to labor market outcomes. I exclude any individuals who do not identify as Hispanic or Latino, and I ensure that those who are left are composed of only citizens and legal immigrants (Green Card, H-1B visas, etc…). I then omit women from my sample as I am only interested in analyzing the effects of SC on the annual earnings of Hispanic
men as my main outcome. I also mark any N/A or unrecorded earnings as missing. As for counties, I accord every 3-digit county code to its 2-digit state code, as there are similar codes for counties across different states.

5.2 Main Outcomes

My main outcome for my analysis is the log of annual earnings for Hispanic men in the US, defined as male citizens and legal immigrants who identify as Hispanic. I choose to focus solely on annual earnings from wage and salary income, as opposed to Social Security or rental income, because I am interested in investigating employer-employee relationships, which are most susceptible to discrimination in the labor market. I also choose to omit women from my main regression for two reasons. First, their participation in the workforce could be very heterogeneous as it might be dependent on factors such as pregnancy and child care. I am not able to distinguish between changes in hours worked and employment due to said factors and changes due to potential discrimination from an employer. It is therefore best to focus solely on men for more accurate results. Second, the program relies on arrests in local jails and state prisons. Since the arrested are mainly composed of men, SC arrestee detention rates are mainly targeted towards Hispanic men. 96% of SC deportations were of Hispanics, and 95% of these were of Hispanic males (TRAC, 2019). The program may therefore have a more prominent impact on men, who, regardless of their legal status, may fear their own deportation or that of a family member or friend. Similarly, employers might act on their prejudice primarily towards Hispanic men, as Hispanic men represent the face of the county deportations that have been taking place.

I also use employment as a secondary independent variable to support or refute hypotheses about results obtained by looking at annual earnings. This outcome enables me to dissect any observed change in annual earnings by looking at whether such change was primarily due to Hispanic men losing or finding more jobs. Similarly, I also run the main
regression using place of residence counties instead of place of work counties, and check for
gender differences and geographical heterogeneity to add more depth to and confirm the
validity and robustness of my main outcome results.

Summary statistics are presented in table 1 and include the main dependent and
independent variables in my regression. The table only includes the Hispanic male legal
population in the US, as that constitutes my main group of interest.
6. Empirical Strategy

For my analysis, I use a Difference-in-Difference (DID) method to explore the effect of SC on annual earnings of Hispanic male citizens and legal immigrants. DID is a method used to study the effect of a treatment, in this case the implementation of the Secure Communities deportation program, on trends in outcomes in a treatment group relative to a control group. SC was activated on a county-by-county basis, starting in 2008, eventually covering all counties by January of 2013. This meant that by 2010, some counties had implemented the program while others had not for reasons which I will present below. This variation proves pertinent to the construction of my treatment and control group, making DID an appropriate method to utilize. My treatment group therefore consists of Hispanic men who work in SC-activated counties, which leaves Hispanic men who work in non-SC counties as my control group. Figure 2 is a series of maps indicating the geographical location of counties that were treated from 2008 to 2013. A total of 400 counties had implemented Secure Communities by December 31st, 2010 (TRAC, 2019) and are included in my treatment group.

6.1 Effects on Annual Earnings

Annual earnings represent the pre-tax income that an individual receives as an employee. This income includes wages, salaries, commissions, cash bonuses, tips, and any other income received from an employer. Using a linear regression (OLS), I compare the average outcome between my treatment and control group. That is to say, I estimate whether Hispanic men who work in SC counties have witnessed an increase or a decrease in the average of their annual earnings over time between the pre-treatment period in 2007 and the post-treatment period in 2010, compared to Hispanic men who work in untreated counties. I estimate this using the following regression:
where \(i\) indexes individual observations, \(c\) indexes the county, and \(t\) indexes the time period (years 2007 and 2010). \(\ln(AE)\) represents the natural logarithm of annual earnings of Hispanic men in the US. \(HispanicSC\) is a dummy variable that equals one if the individual is exposed to SC, i.e. works in a SC county, and equals zero otherwise. This variable serves as a group or county effect in my regression, where it controls for any differences in annual earnings between my treatment and control group, i.e. a group of SC counties and a group of non-SC counties, that are constant over time. This includes level differences driven by education, urbanization, and income for example. \(PostSC\) is a time effect, in this case a dummy variable which equals one if the year corresponds to the post-treatment period (2010) and equals zero if the year corresponds to the pre-treatment period (2007). I also controlled for individual characteristics that might affect one’s annual earnings or income. These are (i) continuous variables: years in school (or the educational level), the experience level, experience squared, the number of years lived in the US, and (ii) fixed effects for categorical variables: the English fluency level, the citizenship status, occupation and industry. Each of these variables can contribute to differences in annual earnings amongst Hispanic men in the US. Adding them to the regression ensures that their effect on the dependent variable can be estimated and separated from the effect of the treatment.

\[ \ln(AE)_{ict} = \beta_0 + \beta_1(\text{HispanicSC})_c + \beta_2(\text{PostSC})_t \\
+ \beta_3(\text{HispanicSC} \times \text{PostSC})_{ct} + \beta_4X_{ict} + \epsilon_{ict} \]
My treatment effect is estimated by the coefficient of interest 3, which shows the percentage change in annual earnings of Hispanic men in the US across different groups (SC counties and non-SC counties) and time (pre and post treatment). The DID estimate is:

\[ \beta_3 = (\ln(AE)_{treatment, t_1} - \ln(AE)_{treatment, t_0}) - (\ln(AE)_{control, t_1} - \ln(AE)_{control, t_0}) \]

where \( t_0 \) and \( t_1 \) refer to the years 2007 and 2010 respectively.

In order to better control for county characteristics that might alter my dependent variable, I also estimate a regression similar to (1) but with place of work county fixed effects. The latter will replace my dummy variable HispanicSC, which only controls for differences in characteristics across the treatment group and control group. There might in fact be differences within the treatment group, for instance across SC-activated counties that might be contributing to our observed coefficient of interest. This replacement will help adjust for any potential confounder that is constant within each county. The regression is constructed as:

\[
\ln(AE)_{ict} = \beta_0 + \beta_1 (WorkCountyFE)_{ict} + \beta_2 (PostSC)_{t} \\
+ \beta_3 (HispanicSC \times PostSC)_{ict} + \beta_4 X_{ict} + \epsilon_{ict}
\]

where WorkCountyFE is a place of work county fixed effect.

For robustness, I also estimate different versions of regression (2) with (i) place of residence county fixed effects instead of place of work fixed effects to support the direction of my coefficient of interest, (ii) employment (by also using place of residence) as the dependent variable instead of annual earnings to dissect any observed effect on my main outcome (i.e. to test whether it might be due to a significant loss of jobs) and capture individuals that were not accounted for by using place of work counties (the unemployed indicated N/A), (iii) women’s annual earnings instead of men’s to check for any gender differences, and (iv) another subdivision of border (with Mexico) and non-border states to check for geographical heterogeneity to test whether the issue of immigration is more prominent and therefore whether
the effect is more pronounced in geographical areas that are closer to Mexico and Latin America.

6.2 Threats to Internal Validity

6.2.1 Potential Selectivity in Early Adopters of SC

While I assume in using DID that the assignment of SC to counties from 2008 to 2010 is plausibly exogenous, it is important to acknowledge some valid elements that might undermine this assumption. The implementation of SC depended on the availability of technological apparatus in local jurisdictions, but it has also followed a geographical pattern. Figure 2 shows that the first counties to implement the program are situated primarily in the Southern U.S. This constitutes two threats to identification. First, Southern states such as Texas are heavily populated with Hispanics as compared to Northern states. Hispanics alone constitute around 40% of the population of Texas (IPUMS, 2019). Figure 3 is a map of the United States showing the intensity of the Hispanic population in each state as of 2017. It is clear that more Hispanics reside in the South, probably for its geographical proximity to Mexico and Latin America. Hispanics in these geographical regions, or in SC counties (by 2010) might possess certain characteristics that distinguish them from their counterparts in the North (or in non-SC counties). Hispanics could, on average, occupy different industries, could have different levels of education, or could even have a different gender composition. Cox and Miles (2013) reveal that early activation of the program in a given county correlates heavily with the intensity of the Hispanic population as a percentage of the total population in that county. Hence, it is important to investigate whether the Hispanics working in SC counties exhibit different demographic characteristics than Hispanics working in non-SC counties. Table 2 displays a number of key demographic characteristics for the two populations prior to the treatment in 2007. The first Column (1) corresponds to the Hispanic population working in
SC counties (which is more likely to be in the South), and the second Column (2) corresponds to the Hispanic population working in non-SC counties.

Second, different regions of the United States might have been hit differently by the global financial crisis (GFC) of 2008. If a significant number of early-adopting counties are situated in the Southern U.S., which was significantly more impacted than the Northern U.S. by the GFC, then I will wrongly attribute some of the decline in annual earnings for Hispanic men in SC counties to labor market discrimination. This is an important limitation to acknowledge, and while I cannot accurately test it, I take it into consideration in interpreting my results.

Cox and Miles (2013) rule out two possibilities that might have threatened the internal validity of the DID model. First, they discuss at great length the possibility that the early assignment of SC to counties was dependent on the crime rate, i.e. that the first counties to receive SC had a high crime rate given the mission of the program. Their data finds that high-crime areas were not first targeted as part of the SC program, which in the scholars’ views “has undermined the government’s claim that Secure Communities is principally about making communities more secure from crime”. Second, Cox and Miles investigate whether the rollout of SC was correlated with local authorities’ cooperation or desire for such a program and find no significant evidence to support this hypothesis. This is crucial as it could have reflected trends in local/regional immigration sentiments and attitudes instead of compliance with federal priorities. My effect could be biased upwards in magnitude if SC counties possessed negative attitudes about immigration and therefore displayed a more prominent labor market discrimination.

6.2.2 The Parallel Trend Assumption

One of the most crucial assumptions about DID consists of ensuring a constant difference between the annual earnings of the treatment and the control group in the absence
of the treatment. This parallel trend assumption can be violated in the presence of one or more omitted variables that are correlated with both changes in annual earnings and with treatment status. This violation will lead to biased estimation of the causal effect of SC on the annual earnings of Hispanic men in the US.

The previous subsection covers several specific threats that might invalidate the parallel trends assumption. And while this assumption cannot be fundamentally tested, it can be backed up by visual inspection of the pre-treatment trends in annual earnings of Hispanic men in the US in both groups. I extract observations over four time points: 2005, 2006, 2007, and 2008. These observations represent the mean log of annual earnings for both my treatment and control group and are plotted on the y-axis against the time points on the x-axis. The graph in Figure 4 indicates a pre-treatment (pre-2008) parallel trend between Hispanics who work in a county where SC was implemented and Hispanics who work in non-SC counties. Visually, the annual earnings of these two groups seem to fluctuate together and their difference seems to stay constant throughout the years. This is a strong indication that the parallel trend assumption holds, and that my control group is indeed valid for analysis.
7. Results

7.1 Annual Earnings of Hispanic Men in the US

Table 1 shows the result of regression (1) and (2), where the Column (1) is simply the raw correlation between the log of earnings and the activation status of Secure Communities, Column (2) is the difference-in-difference regression (1), absent of controls, Column (3) is the second regression and main specification using county fixed effects instead of HispanicSC, and Column (5) is the end result with the full set of controls as shown in the Empirical Strategy section.

In the first column, I observe a positive raw correlation between the log of annual earnings and the treatment status of 0.15 in magnitude. This indicates that, in the absence of DID, I find that the treated counties have on average higher earnings than non-treated counties. In the absence of group and time effects, we cannot attribute this change to Secure Communities. The second column, the basic DID shown in regression (1), indicates a negative effect on the earnings of Hispanic men working in treated counties of 3.1%. This column is more revealing of the causal relationship between SC and annual earnings of my treatment group, as I control for time and group effects by introducing the HispanicSC dummy variable (equals one if individual works in SC county) and the PostSC dummy variable (equals one if the year corresponds to the pre-treatment period of 2007). However, there are many confounding variables correlated with one’s earnings that I do not control for in this column. The third and main specification column yields a negative coefficient of 4.8%. Here, I use place of work county fixed effects instead of merely controlling for differences across the treatment and control group. This betters the precision of the results by minimizing the standard error as shown in the table (from 0.0099 to 0.0095). After controlling for some confounding variables that are determinants of one’s income such as education and experience in Column (4), I find that SC has lowered the earnings of Hispanic men working in treated counties by 3.9%, a figure
significant at the 1% level. Only after controlling for an additional set of controls in Column (5), such as English fluency and industry, do I get the most precise estimate of all my results in the table thus far, which is also significant at the 1% level. This negative coefficient of 3.4% in magnitude signifies that Hispanic men who worked in SC counties have experienced a significant decline in their annual earnings due to the activation of the program, relative to their counterparts who worked in non-treated counties.

As shown in Table 1, my coefficient of interest is stable across different specifications. Regardless of the variable I use to control for group effects (HispanicSC or WorkCountyFE), or the controls I add, the estimate always yields a negative change with slight differences in magnitude.

According to my conceptual framework, I consider a significant decrease in annual earnings of my treatment group as evidence in favor of labor market discrimination. Under the assumption that Hispanic labor in the US is substitutable between undocumented workers and citizens and legal immigrants in the U.S., Secure Communities should have had a positive effect on the treatment group. Yet my findings thus far have indicated that the discrimination effect has in fact dominated any potential substitution effect. This is to say that a significant leftward shift in the demand curve due to discrimination has counteracted any potential rightward shift due to substitution as shown in Figure 1b. Hispanics working in SC counties may have therefore faced acts of discrimination in the workplace more so than those working in a county that has not been exposed to the treatment.

7.2 Robustness Checks

7.2.1 A Different Treatment Group Definition: Place of Residence

In this section, I explore if the estimated effects of SC (a 3.4% decline in annual earnings of Hispanic men who work in SC counties) are robust to a slightly different definition of the treatment variable. Instead of constructing my treatment and control group using place
of work counties, i.e. separating Hispanics who work in SC counties from those who work in non-SC counties, I now define the treatment group as Hispanics who live in SC counties and the control group as those who live in non-SC counties. I make this change to test whether the negative effect of the treatment will hold, given the new definition of the treatment group. I find that Hispanic men whose residence and work counties are identical constitute only about 33.6% of all the sample population. Given that the majority of Hispanic men do not work in the same county they live in, I expect my treatment group to be randomly mismeasured when using place of residence counties to construct it. That is to say that a share of people who are supposed to be treated end up in the control group, and vice versa. I therefore expect to see an effect in the same direction but perhaps of less magnitude because of attenuation bias due to classical measurement error. This mismeasurement lowers the annual earnings gap between the treatment and control group observed when using place of work counties. I run the same regression as equation (2) but with my treatment and control group defined using residence counties, and fixed effects for residence counties instead of work counties.

Note that for all robustness checks, I only use county fixed effects instead of the treatment group dummy variable ($HispanicSC$) to better control for differences across counties.

Table 2 presents the results of the modified regression, which integrates the newly defined treatment. The coefficient of interest in column 5 indeed supports the hypothesis made above, demonstrating a negative percentage change that is smaller in magnitude than the main result obtained (0.015 compared to 0.034). Under this treatment group definition, Hispanic men who live in SC counties have witnessed a decline in their annual earnings of around 1.5%, relative to Hispanic men who live in non-SC counties. This effect is only significant at the 10% level.
7.2.2 A Different Outcome: Employment

As another robustness check measure, I test whether SC had any effect on the employment of Hispanics in the US. The regression below uses, similar to the subsection above, place of residence to define the treatment and control group, simply because place of work county is not reported for the unemployed. This new outcome accounts for those who might have lost their jobs prior to 2010 due to SC (i.e. 2008 and 2009) that are not accounted for in the results of the main outcome. By testing employment changes between 2007 and 2010, I can also infer reasons behind the observed decline in annual earnings. I estimate this effect by slightly modifying regression (2). Instead of using the log of annual earnings, I use a dummy variable equal to one if the individual is employed, and equal to zero if not. Therefore, I attribute a positive and significant coefficient (of interest) to a rise in employment, and a negative and significant coefficient to a fall in employment.

I note an insignificant decrease in employment of 0.18% as observed in Column (4) of Table 3. I conclude that the employment of Hispanic men who live in SC counties was not impacted by the implementation of the program. I attribute this result to attenuation bias, which biases the regression slope towards zero due to classical measurement errors in the independent variable. Another explanation could be due to the elasticity of labor supply as explained in my conceptual framework. If the labor supply of Hispanic men is inelastic, I expect to see little to no change in the employment levels as the demand curve moves.

7.2.3 Gender Differences: Women’s Annual Earnings

Although women’s participation in the labor force might be heterogeneous, it is important to test whether Hispanic women witnessed an impact similar to that of Hispanic men. I construct my treatment and control group similarly to regression (2), and I omit all Hispanics who identify as male. I hypothesize that the deportation program had a lesser effect on women,
given that they may be generally viewed as more trustworthy and less prone to racial profiling or association with crime and illegal activity.

I find that Hispanic women who work in SC counties earn less than those who work in non-SC counties by 2.9%. Column (1) of Table 4 refers to the main result obtained for men, which is a 3.4% decline in their annual earnings. For comparison, Column (2) shows the same specification estimated on women rather than men. The coefficient of interest in Column (2) is significant at the 1% level and is lower than that observed for men in Column (1) by 0.5%.

This decline in women’s annual earnings is consistent with the results I have obtained thus far. I test whether the gender differences in annual earnings are statistically significant, i.e. whether the program has affected Hispanic men and women who work in SC counties differently because of the nature of their gender. To do so, I interact every term in regression (2) with a dummy variable for gender which equals one if the individual is male and equals zero if the individual is female.

I find an insignificant difference between the annual earnings of Hispanic men and women working in SC counties, as indicated in the first coefficient in Table A of the Appendix. It is nonetheless important to explore the implications of the economically small and statistically insignificant heterogeneity by gender in the discussion section.

### 7.2.3 Geographical Heterogeneity: Border and Non-Border States

I divide my treatment and control group into border (with Mexico) and non-border states to check whether the effect of the program is more prominent in some geographical regions than others and, if so, to elucidate what explains any differences. Geographical heterogeneity can reveal a great deal about how and why this program fuels prejudice against Hispanics in employers. For example, discrimination could be related to the concentration of Hispanics in a state’s population, or its proximity to Mexico. With these findings, I aim to
introduce a deeper discussion about how immigration policies might induce discrimination in the workplace.

I find that Hispanic men who work in SC counties in border states witness a larger decline in their annual earnings (5.4%) than those who work in SC counties in non-border states (2.6%). Column (1) in Table 6 corresponds to the border states regression while Column (2) corresponds to the non-border state regression. My findings suggest that labor market discrimination might be pronounced in border states, perhaps because that is where the issue of immigration is most prominent in the US.

I test whether this difference of 2.8% is significant by employing the same method used to assess the significance of gender differences in annual earnings. I interact all the variables of regression (2) with a “Border” dummy variable equal to one if the individual works in a state that borders Mexico (i.e. Arizona, New Mexico, Texas, and California) and equal to zero if the individual works in a state that does not border Mexico. I find that the difference between the annual earnings of Hispanic men working in SC counties in border states and those working in SC counties in non-border states is insignificant. My regression in Table 7 recovers the difference of -0.028 log points in the coefficient of interest. However, a t-test against the null hypothesis stating that there is no difference between the two coefficients (men’s annual earnings and women’s annual earnings) yields a t-value smaller than 1.96, indicating lack of evidence to reject the null hypothesis. The gender differences in annual earnings are therefore insignificant.

7.3 Summary of Effects

The results obtained thus far suggest that the Secure Communities deportation program had a significant effect on the annual earnings (comprised of wages, salary, tips, and any compensation received from employers) of Hispanic citizens and legal immigrants working in SC counties. I find a decline of 3.4% in the treatment group’s annual earnings after controlling
for (i) any confounding effects that might affect one’s income such as experience and education, and (ii) time and county fixed effects.

Evidence also seems to suggest that Hispanic women working in SC counties also witness a decline in their annual earnings of 2.8%, which is relatively lower than that of men. This validates my results’ robustness by showing that the direction of results is similar across gender. I detect no evidence that supports the significance of these annual earning gender differences.

I observe a more pronounced effect of SC on annual earnings of Hispanic men in states that border Mexico, where the coefficient of interest (-5.4%) is higher than the one in non-border states (-2.6%). I elaborate on why this might be the case in the discussion section. Again, I find that the difference between the percentage change in annual earnings of Hispanic men in border and non-border states is insignificant, as shown in Table B of the Appendix.

Overall, my results suggest that given the negative percentage change in their earnings, Hispanic men in SC counties may have faced labor market discrimination. Under the assumption that Hispanic undocumented workers are substitutable for Hispanic citizens and legal immigrants, in the absence of discrimination, I expect to see a rise in wages and employment for Hispanic legal workers due to a rightward shift in their labor demand. However, and in the presence of discrimination, I expect to see a leftward shift in the labor demand either after the workers have been substituted or instead of this phenomenon. Either way, this decline in labor demand for my treatment group either partially or fully counteracts the rise in wages and employment from substitution. As detailed in my conceptual framework, I consider a decrease in annual earnings to count as strong evidence in support of my labor market discrimination hypothesis.
8. Discussion

The headline result obtained thus far is a significant decline of 3.4% in the annual earnings of Hispanic men working in SC counties. This suggests that the implementation of the Secure Communities program, which was targeted mainly towards undocumented Hispanic immigrants, had a negative spillover effect on Hispanic citizens and legal immigrants due to labor market discrimination. As shown in my conceptual framework section, Hispanic citizens and legal immigrants should have benefited from the rise in employment opportunities after the departure of Hispanics who were deported from the US homeland together with those who voluntarily left for fear of being detained by local authorities. This would have appeared as an increase in the wage and employment rate of my treatment group, given higher demand for their labor. A decline in earnings is, therefore, an indication that this substitution either did not take place or was overpowered by the effects of discrimination, and that net demand for Hispanic (citizens and legal immigrants) labor has in fact declined. This decline testifies to employment discrimination towards job-seekers of Hispanic ethnicity. This paper does not delve too deeply into the psychology of discriminating employers, though I do acknowledge that their employment and administrative decisions regarding Hispanic workers could stem from a variety of reasons such as ignorance, groupthink, prejudice, a desire to comply with authorities, etc… Rather than examining particular psychological reasons behind employment decisions, this paper is more invested in furnishing an understanding of the way in which immigration policies incite different responses from employers, such as decisions to cut back/fire Hispanic labor or lower the wages of Hispanic workers.

A decline in earnings could stem from either a decline in hours worked, a decline in the wage rate (or compensation and tips), or a decline in both. The sample years I have chosen in my data set contain data points on hours worked per week but fail to contain information about weeks worked per year, making it difficult to measure the total hours worked per year and, by
extension, to calculate the hourly wage rate from one’s annual earnings. Such a lack of information constitutes a limitation because I would have otherwise been able to unpack this decrease in the dependent variable. Nevertheless, I provide a hypothetical discussion about the factors that might have played into the negative effect observed on my main labor market outcome of annual earnings.

8.1 Decomposing Annual Earnings

8.1.1 Hours Worked

My dependent variable is defined in terms of wages, salaries, tips, and/or compensation given by the employer. Accordingly, I describe annual earnings as the wage rate (including tips and compensation) multiplied by the hours worked per year. A decrease in the hours worked of a worker will therefore be negatively reflected in their annual earnings. A decline in hours worked could in turn be due to two reasons.

First, and as a result of Secure Communities, a Hispanic worker could be working fewer hours, voluntarily or involuntarily. On the one hand, the discriminatory employer might impose a schedule with fewer hours on a Hispanic employee, perhaps in order to avoid any significant association with and exposure to them. On the other hand, the Hispanic employee himself might choose to work fewer hours than usual out of fear either of being discriminated against in the work place or of exposing an undocumented family member or friend. Economic literature on labor market discrimination supports the latter claim, stating that a policy or an event that negatively portrays a minority group in a negative light might constrain members of that minority group to their homes, which in turn might result in a reduction of the labor supply of that particular group. Such a phenomenon is evinced by the findings of Kerwin Charles (2017) on taste-based discrimination and labor market outcomes of Arab and Muslim men in the U.S. The paper finds that each time a US soldier dies in an Arab country, Arab-Americans living in that soldier’s state or county of origin supply less labor and confine themselves to
their homes for longer periods of time for fear of needing protection from potential discrimination in public areas and/or in the workplace.

Second, a Hispanic employee could be supplying fewer hours of labor simply because he was fired from his job, an act that could be traced back to a discriminatory employer. I test whether SC had any effect on the employment of Hispanic men in SC counties, to account for any job gains or losses that happened between 2007 and 2010 (which are not necessarily reflected in the decline of annual earnings). As shown in Table 5, I find no significant evidence in support of this hypothesis. This indicates that employers did not fire their Hispanic employees after and in response to the implementation of the SC program. It could also mean that the supply curve of Hispanic labor is relatively steep (inelastic), or even vertical (perfectly inelastic), which means that Hispanic men were not very sensitive to potential decreases in wages that could have resulted in a decline in employment. Given the tense political and economic climate that took place after 2008 due to immigration enforcement and the global financial crisis, Hispanic men in the US might be more inclined to preserve their employment regardless of any negative employer treatment or wage cut they incur.

8.1.2 The Wage Rate

By the definition of my dependent variable, a decline in annual earnings could also be due to a decline in the wage rate received by Hispanic men working in SC counties. Employers might simply act out of spite and decrease the reward that their Hispanic employees receive in exchange for their labor. They might justify this action by considering the Hispanic marginal revenue product of labor to be lower than that of non-Hispanics, yielding a lower wage. This action on the part of an employer is particularly feasible given the low-paying jobs that most

Note that this only applies to Hispanic men who lost their job mid-2010 because they were able to indicate their place of work county, which suggests that they worked either all of 2010 or only for parts of it. The decline in annual earnings does not reflect the job losses that took place in 2008 and 2009 as those people could not have indicated their place of county in 2010.
Hispanics hold. I highlight this channel in light of the fact that most Hispanic employees do not subscribe to a union (BLS, 2020), which might have otherwise constituted a barrier to such treatment from the employer. Moreover, and in light of the fact that the two most occupied industries by Hispanic workers are construction and restaurants, my treatment group could have faced lower demand for their construction services or received less tips from dining customers, whose prejudice against Hispanics might have augmented in light of the implementation of the SC program.

It is important to understand not only how hours worked and the wage rate separately respond to SC, but also how they interact with each other under the implementation of the program. The correlation of these two variables is dependent on whether the substitution or income effect dominates the individual’s actions. One the one hand, a Hispanic worker faced with a lower wage could decide to work more hours (on the assumption that he could) to compensate for the loss of income; this illustrates the income effect. On the other hand, due to a substitution effect, he could be discouraged and work fewer hours because the marginal hour’s worth of his labor is not as rewarding as it used to be. While I cannot test for either of these effects given the limitations on my data set variables (as discussed above), I can acknowledge that these distinctions are very important and could both be operating in this context.

8.1.3 Assumptions

I make several crucial assumptions to support my attempts at explaining and elucidating the multiple reasons for the decline in my treatment group’s annual earnings.

First, I assume that employers have a great deal of bargaining power over their employees, thus enabling them to avoid the consequences of dictating the wage rate and of discriminating against Hispanic workers. This assumption is plausible for two reasons, the first being the vulnerability of Hispanic people in SC communities, given that the program targets
mainly Hispanic undocumented immigrants. The second and perhaps most important reason is less concerned with the ethnicity of these workers and more related to economic conditions at the time, namely the global financial crisis and its recessionary consequences on the job market. I also make the assumption that the labor demand side in its entirety is discriminatory towards Hispanic men in SC-activated counties. This is crucial because, as predicted by the Becker taste-based discrimination model (Becker, 1957), if the labor market was comprised of non-discriminatory employers, or allowed for easy entry of non-discriminatory employers, then Hispanic employees could easily find new jobs working under a more “welcoming” employer, thereby leaving discriminatory firms and employers with relatively high labor costs or in some cases no labor at all.

Second, in my conceptual framework I advance reasons to believe that Hispanic labor is mainly substitutable between undocumented workers and workers with citizenship and legal immigration status. This assumption is crucial in understanding the market dynamics and what I hypothesize to be labor market discrimination. If these two labor groups were in fact of a complementary nature, then the decline in annual earnings merely indicates a loss of Hispanic jobs due to the deportation and flight of undocumented workers. For example, a Hispanic citizen who is reliant on an undocumented Hispanic nanny is now suddenly obliged to reduce their work hours in order to assume the responsibilities of the nanny (looking after their own children). This is due not only to the nanny’s deportation, but also to the deportation of other undocumented housekeeping laborers. If this example is scaled up and the complementarity of the two working groups is in fact prevalent and relevant, then a reassessment of my findings is needed.

Lastly, I note that the discussion that has taken place thus far is conditional on the presence of labor market discrimination. That is to say that the explanations given in the subsection above are all reliant on the premise that the decline in earnings is in fact due to labor
market discrimination. Such explanations are by no means exhaustive, so it is worth noting alternative reasons for the observed decline that do not prescribe to the hypothesis of labor market discrimination.

I briefly mention in this paper the notion of perception and its relationship to discrimination in order to explain the potential spillover effect of the SC program on Hispanic workers with legal status in the U.S. It is worth noting that perception can manifest in a variety of ways, one of which is related to ethnic prejudice and is central to my paper. In particular, one possible reason for a sudden discriminatory wave against Hispanic citizens and legal immigrants could be that employers or customers misperceive them as undocumented and act on their prejudice against the Hispanic undocumented population.

Another way in which perception can manifest is through pure economic exploitation, in a manner that is unrelated to the particular ethnicity of the affected workers. For instance, as a result of SC, employers and customers who pay less or tip less could simply be taking advantage of those whom they perceive to be members of a vulnerable group (undocumented workers) but who are in fact citizens and workers with legal status in the US. Such employers and customers might believe that they can exploit certain individuals, whom they misperceive as undocumented, given elevated anti-illegal-immigration sentiments which these employers and customers themselves might not necessarily share.

Note that a third way in which perception might play a role in negatively impacting Hispanic citizens and legal immigrants is compliance with the local authorities: employers misperceiving their legal Hispanic workers as undocumented might, in their view, simply be complying with the policy implemented by their local jurisdictions by firing their “undocumented” workers or reporting them to the authorities. Economic exploitation and compliance with the law are both reliant on misperception but fall outside the realm of labor
market discrimination, as they do not assume prejudice against the Hispanic population but rather a situation that could occur with any other vulnerable ethnic group.

8.2 Explaining Heterogeneous Effects on Earnings

8.2.1 Gender Differences

I find that SC had a lower effect on women’s annual earnings than men, though they both point in the same direction. Hispanic women and men faced a decline of 2.8% and 3.4% respectively as a result of the implementation of Secure Communities in the counties they work in. And while I find that this difference is not statistically significant, it is worthwhile to briefly discuss the different employer treatment that these two groups might face.

Around 95% of all deported undocumented immigrants through Secure Communities are male (TRAC, 2019). I explain this number by looking at the following three possibilities. First, the danger associated with crossing the US-Mexico border may disincentivize some women from taking the risk of migrating to the U.S. Second, Hispanic men are often the first to cross the border in order to establish a living and ensure accommodation prior to the arrival of the rest of their family members. Third, the SC program claims to target undocumented immigrants with criminal records or arrest files, a group which tends to be male-dominated. Given that SC has deported more Hispanic men than women, there is reason to believe that Hispanic men are more targeted for employer discrimination as a result. Moreover, many psychology scholars have run experiments that show that women are on average more trustworthy than men (Bulchan and Solnick, 2008), which may help to explain why women might not be as targeted or as affected by this SC program. Hispanic women might as a result not be as afraid or as cautious as Hispanic men with respect to going out in public, or alternatively may not cut back on hours worked. Similarly, an employer might not be as willing to discriminate against Hispanic women, perhaps because they are more prone to receiving sympathy or appear to be more trustworthy than the average Hispanic man.
8.2.2 Border Differences

I also find that Hispanic men who work in SC counties situated in states that border Mexico are more affected by the implementation of the program than those who work in SC counties situated in non-border states (X.X% decline in earnings in border states vs. Y.Y% in non-border states). I do not find evidence in support of the statistical significance of these geographic differences, but I nonetheless elaborate on border differences that could potentially affect Hispanic men’s annual earnings in different ways.

About 63% of all deportations under SC (2008-present) were conducted in the four states that border Mexico (Texas, California, New Mexico, and Arizona). This could either be attributed to a more intense and thorough search conducted in border states given the proximity of Mexico and Latin America, or to the fact that Hispanic immigrants are more concentrated in this geographical region than elsewhere. Regardless, the frequency of deportations in border states may be reflected in the intensity of anti-immigration or anti-undocumented immigration sentiments. It is plausible that the more deportations are conducted in one’s county, the more prominent the issue of immigration might become, and so the more justified one might feel in acting on one’s prejudice. The effect of SC might therefore be more pronounced in border states, where the issue of deportation is more prominent.

8.3 Policy Implications

My findings suggest that the implementation of Secure Communities has affected Hispanic citizens and legal immigrants negatively, by a reduction of 3.4% in their annual earnings. This testifies to economic consequences that should be taken into consideration when evaluating an immigration enforcement policy. As shown in my conceptual framework, though theoretically Hispanic citizens and legal immigrants should have benefited from the departure of Hispanic undocumented workers (assuming labor substitution and not complementarity), my results attest to the contrary. A policy aimed at reducing the number of undocumented
immigrants risks elevating the phenomenon of labor market discrimination and thus exposing citizens and legal immigrants to a decrease in their labor demand. My findings are not specific to Hispanic immigrants. Though this paper focuses on SC which is mainly targeted towards this ethnic group, it aims to serve a greater purpose. This paper could be generalized to all immigration enforcement that works similarly to SC, and to all minority groups in the U.S. at risk of deportation.
9. Conclusion

The implementation of immigration enforcement policies such as Secure Communities risks negatively impacting the legal population (citizens and legal immigrants) in the U.S. This paper investigates closely the SC program and its effect on the Hispanic labor market. In particular, I test if this program had a negative effect on the annual earnings of Hispanic male citizens and legal immigrants through misperception and labor market discrimination. This investigation is pertinent as it looks at whether and in what ways immigration enforcement policies can induce potential prejudice and discrimination towards immigrants and/or minority groups.

I use a difference-in-difference regression to utilize the county variations from 2008 to 2012. The program was only fully activated in all U.S. counties in 2013. By looking at the effects of Secure Communities between the pre-treatment period (2007) and the post-treatment period (2010), I construct a treatment group of Hispanic men working in SC counties and a control group of Hispanic men working in non-SC counties (who after 2010 eventually also implemented the program). I check for parallel trends between the treatment and control group in the pre-treatment period, evinced by Figure 4.

I find that Secure Communities is associated with a 3.4% decline in annual earnings of Hispanic men working in SC counties, relative to those who work in untreated counties. I find no effect of this program on the employment of my treatment group. I also find that female Hispanic labor is also affected negatively by SC (-2.8%) but to a lesser extent than men. I obtain no evidence in support of the significance of these gender differences in annual earnings. I test whether Hispanic men who work in states that border with Mexico are more or less impacted than those who work in non-border states. I find that the Hispanic men working in treated counties in border states witness a decline of 5.4% in annual earnings while those
working in treated counties in non-border states only witness a 2.8% decline. Again, this difference is not statistically significant.

I discuss several hypotheses in order to explain this decline in annual earnings (constant over many treatment variable definitions and subgroups). I explore the possibility of a lower wage dictated by the employer, voluntary or involuntary fewer hours worked, all stemming from a place of prejudice against Hispanics or Hispanic undocumented workers. However, I also explore other explanations that fall outside the realm of labor market discrimination, such as economic exploitation and compliance with local authorities.

My findings are important in shaping how policy makers evaluate immigration enforcement policies such as that of Secure Communities. This paper serves as evidence for a deportation program that might have heightened prejudice in employers and induced labor market discrimination. I believe that my work could be generalized to not only other deportation programs that work similarly to SC but also to other minority groups at risk of being discriminated against in the U.S.
10. Bibliography

Journal Articles, Books, and Reports


Websites & News Articles


11. Tables and Figures

Table 1: Summary Statistics

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<td>Age</td>
<td>186,293</td>
<td>35.9</td>
<td>11.6</td>
<td>16</td>
<td>60</td>
</tr>
<tr>
<td>Education (years)</td>
<td>186,293</td>
<td>11.5</td>
<td>5.9</td>
<td>0</td>
<td>21+</td>
</tr>
<tr>
<td>Time in the US (years)</td>
<td>186,293</td>
<td>10.1</td>
<td>12.3</td>
<td>0</td>
<td>60</td>
</tr>
</tbody>
</table>

Data Sources: ACS sample years of 2008 and 2011 and ICE Secure Communities Activation Report
Notes: Sample includes years 2007 (ACS 2008) and 2010 (ACS 2011) as well as male Hispanic citizens and legal immigrants only. Note that Annual Earnings do not include the unemployed population, this is because I use place-of-work counties to define my treatment variable. A total of 430 place-of-work counties were used to construct the treatment group.
Table 2: Demographic Differences between Hispanics working in SC Counties and Hispanics working in non-SC Counties

<table>
<thead>
<tr>
<th></th>
<th>Hispanics in SC counties</th>
<th>Hispanics in non-SC counties</th>
</tr>
</thead>
<tbody>
<tr>
<td>Share of male population</td>
<td>53.6 (0.5)</td>
<td>46.6 (0.5)</td>
</tr>
<tr>
<td>Mean Education (years)</td>
<td>11.8 (6.1)</td>
<td>11.4 (5.8)</td>
</tr>
<tr>
<td>Mean Annual Earnings (US Dollar)</td>
<td>31932.4 (32577.2)</td>
<td>27736.6 (33876.6)</td>
</tr>
<tr>
<td>Mean Age</td>
<td>36.5 (11.4)</td>
<td>34.8 (12.8)</td>
</tr>
</tbody>
</table>

Data Sources: ACS sample years of 2008 and 2011 and ICE Secure Communities Activation Report
Notes: Table 2 details the demographic differences between my treatment (Hispanic workers in SC counties) and control group (Hispanic workers in non-SC counties). Standard deviations are in parentheses.
Table 3: DID Effects of Secure Communities on Annual Earnings of Hispanic Male Workers

<table>
<thead>
<tr>
<th>Log Annual Earnings</th>
<th>(1) Raw Relationship</th>
<th>(2) Basic DID</th>
<th>(3) With County FE</th>
<th>(4) Basic Controls</th>
<th>(5) All Controls</th>
</tr>
</thead>
<tbody>
<tr>
<td>(=1 if 2010)* (=1 if SC county)</td>
<td>0.15*** (-0.0054)</td>
<td>-0.031*** (0.0099)</td>
<td>-0.048*** (0.0095)</td>
<td>-0.039*** (0.0080)</td>
<td>-0.034*** (0.0073)</td>
</tr>
<tr>
<td>(=1 if post-treatment year 2010)</td>
<td>-0.053*** (0.0077)</td>
<td>-0.042*** (0.0075)</td>
<td>-0.069*** (0.0064)</td>
<td>-0.045*** (0.0064)</td>
<td></td>
</tr>
<tr>
<td>(=1 if works in SC county)</td>
<td>0.028*** (0.0075)</td>
<td>(0.0064)</td>
<td>(0.0059)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Experience (years)</td>
<td>0.13*** (0.0092)</td>
<td>0.10*** (0.0088)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Experience Squared</td>
<td>-0.0018*** (1.63e-05)</td>
<td>-0.0015*** (1.54e-05)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Education (years)</td>
<td>0.12*** (0.0080)</td>
<td>0.053*** (0.0098)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Time in the US (years)</td>
<td>0.00201*** (0.00018)</td>
<td>0.0015*** (0.00024)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>9.89*** (0.0031)</td>
<td>10.1*** (0.0061)</td>
<td>10.1*** (0.0033)</td>
<td>7.63*** (0.013)</td>
<td>8.40*** (0.013)</td>
</tr>
<tr>
<td>Observations</td>
<td>186,293</td>
<td>186,293</td>
<td>186,293</td>
<td>186,293</td>
<td>186,293</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.003</td>
<td>0.008</td>
<td>0.034</td>
<td>0.295</td>
<td>0.422</td>
</tr>
<tr>
<td>Place of Work County FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Speaks English FE</td>
<td>YES</td>
<td>YES</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Citizenship FE</td>
<td>YES</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Occupation FE</td>
<td>YES</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Industry FE</td>
<td>YES</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Data Sources: ACS sample years of 2008 and 2011 and ICE Secure Communities Activation Report
Notes: Table 3 shows DID regressions of the effect of Secure Communities deportation program on log annual earnings. Time effects control for the pre- and post-treatment years of 2007 (2008 ACS) and 2010 (2011 ACS) respectively, and place-of-work county fixed effects account for county characteristics. The treatment group includes all male citizens and legal immigrants of Hispanic or Latino origin who work in treated counties. In Column (5) SC is associated with a significant 3.4% decline in annual earnings.
Table 4: DID Effects of Secure Communities on Annual Earnings of Hispanic Male Workers

(Using place-of-residence counties as a different definition of the treatment variable)

<table>
<thead>
<tr>
<th>Log Annual Earnings</th>
<th>(1) DID with County FE</th>
<th>(2) All Controls</th>
</tr>
</thead>
<tbody>
<tr>
<td>(=1 if 2010)* (=1 if SC county)</td>
<td>0.019* (0.010)</td>
<td>0.015* (0.0079)</td>
</tr>
<tr>
<td>=1 if post-treatment year (2010)</td>
<td>0.092*** (0.0081)</td>
<td>0.083*** (0.0064)</td>
</tr>
<tr>
<td>Experience (years)</td>
<td>0.12*** (0.00092)</td>
<td>0.060*** (0.0011)</td>
</tr>
<tr>
<td>Experience Squared</td>
<td>-0.0017*** (1.62e-05)</td>
<td>0.0034 (0.00026)</td>
</tr>
<tr>
<td>Education (years)</td>
<td>0.060*** (0.0011)</td>
<td>0.0034 (0.00026)</td>
</tr>
<tr>
<td>Time in the US (years)</td>
<td>0.00034 (0.000092)</td>
<td>0.00034 (0.000092)</td>
</tr>
<tr>
<td>Constant</td>
<td>9.98*** (0.0036)</td>
<td>8.04*** (0.014)</td>
</tr>
</tbody>
</table>

Observations | 186,293 | 186,293 |
R-squared | 0.013 | 0.41 |
Place of Residence County FE | YES | YES |
Speaks English FE | YES | YES |
Citizenship FE | YES | YES |
Occupation FE | YES | YES |
Industry FE | YES | YES |

Robust standard errors in parentheses

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

Data Sources: ACS sample years of 2008 and 2011 and ICE Secure Communities Activation Report
Notes: Table 4 shows DID regressions of the effect of Secure Communities deportation program on log annual earnings, using a different definition for my treatment variable (place of residence). Time effects control for the pre- and post-treatment years of 2007 (2008 ACS) and 2010 (2011 ACS) respectively, and place-of-residence county fixed effects account for county characteristics. Place-of-residence treatment variable is used to check for robustness of main result. The treatment group includes all male citizens and legal immigrants of Hispanic or Latino origin who live in treated counties. In Column (2) SC is associated with a 1.5% decline in annual earnings, significant at the 10% level. This decline is lower than that of my main result (-3.4%).
Table 5: DID Effects of Secure Communities on the Employment of Hispanic Male Workers

<table>
<thead>
<tr>
<th>DID with County FE</th>
<th>All Controls</th>
</tr>
</thead>
<tbody>
<tr>
<td>=1 if Employed</td>
<td></td>
</tr>
<tr>
<td>(=1 if 2010)* (=1 if SC county)</td>
<td>-0.0041</td>
</tr>
<tr>
<td></td>
<td>(0.0026)</td>
</tr>
<tr>
<td>= 1 if post-treatment year (2010)</td>
<td>-0.048***</td>
</tr>
<tr>
<td></td>
<td>(0.0020)</td>
</tr>
<tr>
<td>Experience (years)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.0059***</td>
</tr>
<tr>
<td>Experience Squared</td>
<td></td>
</tr>
<tr>
<td></td>
<td>-9.23e-05***</td>
</tr>
<tr>
<td>Education (years)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.0026***</td>
</tr>
<tr>
<td>Time in the US (years)</td>
<td>-0.00081***</td>
</tr>
<tr>
<td>Constant</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.93***</td>
</tr>
<tr>
<td>Observations</td>
<td></td>
</tr>
<tr>
<td></td>
<td>195,704</td>
</tr>
<tr>
<td>R-squared</td>
<td></td>
</tr>
<tr>
<td></td>
<td>0.014</td>
</tr>
<tr>
<td>Place of Residence County FE</td>
<td>YES</td>
</tr>
<tr>
<td>Speaks English FE</td>
<td></td>
</tr>
<tr>
<td></td>
<td>YES</td>
</tr>
<tr>
<td>Citizenship FE</td>
<td></td>
</tr>
<tr>
<td></td>
<td>YES</td>
</tr>
<tr>
<td>Occupation FE</td>
<td></td>
</tr>
<tr>
<td></td>
<td>YES</td>
</tr>
<tr>
<td>Industry FE</td>
<td></td>
</tr>
<tr>
<td></td>
<td>YES</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Data Sources: ACS sample years of 2008 and 2011 and ICE Secure Communities Activation Report
Notes: Table 5 shows DID regressions of the effect of Secure Communities deportation program on employment (a dummy variable equal to one if employed), using place-of-residence (instead of place-of-work) counties to account for the unemployed. Time effects control for the pre- and post-treatment years of 2007 (2008 ACS) and 2010 (2011 ACS) respectively, and place-of-residence county fixed effects account for county characteristics. The treatment group includes all male citizens and legal immigrants of Hispanic or Latino origin who live in treated counties. Effects on employment are used to test for robustness of main outcome (annual earnings) and to dissect the reasons behind the decline in said outcome. In Column (2) SC does not have a significant effect on employment levels of Hispanic men in SC counties. The coefficient of interest is of 0.18% in magnitude and is insignificant.
Table 6: Comparing DID Effects of Secure Communities on Annual Earnings of Hispanic Female and Male workers, and of Hispanic Male Workers in Border and Non-Border States

<table>
<thead>
<tr>
<th>Log Annual Earnings</th>
<th>(1) Men</th>
<th>(2) Women</th>
<th>(3) Men in Border States</th>
<th>(4) Men in Non-Border States</th>
</tr>
</thead>
<tbody>
<tr>
<td>(=1 if 2010)* (=1 if SC county)</td>
<td>-0.034***</td>
<td>-0.029***</td>
<td>-0.054***</td>
<td>-0.026**</td>
</tr>
<tr>
<td>(0.0073)</td>
<td>(0.0089)</td>
<td>(0.014)</td>
<td>(0.012)</td>
<td></td>
</tr>
<tr>
<td>(=1 if post-treatment year (2010)</td>
<td>-0.045***</td>
<td>0.0022</td>
<td>-0.020</td>
<td>-0.055***</td>
</tr>
<tr>
<td>(0.0059)</td>
<td>(0.0073)</td>
<td>(0.013)</td>
<td>(0.0068)</td>
<td></td>
</tr>
<tr>
<td>Experience (years)</td>
<td>0.10***</td>
<td>0.087***</td>
<td>0.10***</td>
<td>0.10***</td>
</tr>
<tr>
<td>(0.00088)</td>
<td>(0.0010)</td>
<td>(0.0012)</td>
<td>(0.0013)</td>
<td></td>
</tr>
<tr>
<td>Experience Squared</td>
<td>-0.0015***</td>
<td>-0.0012***</td>
<td>-0.0014***</td>
<td>-0.0015***</td>
</tr>
<tr>
<td>(1.54e-05)</td>
<td>(1.81e-05)</td>
<td>(2.04e-05)</td>
<td>(2.36e-05)</td>
<td></td>
</tr>
<tr>
<td>Education (years)</td>
<td>0.053***</td>
<td>0.067***</td>
<td>0.053***</td>
<td>0.052***</td>
</tr>
<tr>
<td>(0.00098)</td>
<td>(0.0013)</td>
<td>(0.0014)</td>
<td>(0.0014)</td>
<td></td>
</tr>
<tr>
<td>Time in the US (years)</td>
<td>0.0016***</td>
<td>0.0018***</td>
<td>0.0010***</td>
<td>0.0023***</td>
</tr>
<tr>
<td>(0.00024)</td>
<td>(0.00028)</td>
<td>(0.00037)</td>
<td>(0.00032)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>8.4***</td>
<td>8.1***</td>
<td>8.41***</td>
<td>8.39***</td>
</tr>
<tr>
<td>(0.013)</td>
<td>(0.016)</td>
<td>(0.018)</td>
<td>(0.020)</td>
<td></td>
</tr>
<tr>
<td>Observations</td>
<td>186,293</td>
<td>150,330</td>
<td>98,595</td>
<td>87,698</td>
</tr>
<tr>
<td>R-squared</td>
<td>0.42</td>
<td>0.41</td>
<td>0.42</td>
<td>0.44</td>
</tr>
<tr>
<td>Speaks English FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Citizenship FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Occupation FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
<tr>
<td>Industry FE</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
<td>YES</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses

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

Data Sources: ACS sample years of 2008 and 2011 and ICE Secure Communities Activation Report
Notes: Table 6 shows DID regressions of the effect of Secure Communities deportation program on log annual earnings. Time effects control for the pre- and post-treatment years of 2007 (2008 ACS) and 2010 (2011 ACS) respectively, and place-of-work county fixed effects account for county characteristics. The first two columns correspond to the treatment group that was divided into female and male legal workers of Hispanic or Latino origin in treated counties. Column (1) and (2) show that Secure Communities is associated with a 3.4% decline in men’s annual earnings and a 2.8% decline in women’s annual earnings, both significant at the 1% level. The third and fourth column correspond to the treatment group that was divided into states that border Mexico and states that do not. Column (3) and (4) show that SC had a negative effect on annual earnings of Hispanic men who work in treated counties in border states (-5.4%) and a similar effect but to a lesser extent on annual earnings of Hispanic men who work in treated counties in non-border states (-2.6%).
Figure 1: Labor Market Discrimination Against Hispanics in the US: A Conceptual Framework

Figure 1a

In the absence of labor market discrimination

Labor market for Hispanic undocumented immigrants

Labor market for Hispanic citizens and legal immigrants

*Assuming imperfect labor substitution
In the presence of labor market discrimination

![Diagram showing labor market for Hispanic citizens and legal immigrants](image)

Source: Yousra Zerouali Boukhal
Notes: Figure 1 helps illustrate my conceptual framework, the backbone of my analysis and discussion. In these graphs, I demonstrate how discrimination manifests in the labor market. I assume imperfect substitution between the labor of undocumented workers and that of citizens and legal immigrants (both of Hispanic origin). I consider an observed decrease in wage or annual earnings as evidence in favor of labor market discrimination against Hispanic workers (depicted as a decrease in labor demand).
Figure 2: The Chronological Spread of Secure Communities in US Counties (2008-2012)

Source: Cox and Miles (2015)
Note: This series of map details the geographical locations of SC-activated counties from 2008 to 2012.
Figure 3: Hispanic Concentration in the South of the US

U.S. Hispanic Population by County
2017

Source: the US Census Bureau
Note: This map details the percentage of Hispanic population by county in the US as of 2017.
Figure 4: Parallel Trends in Annual Earnings Between the Treatment and Control Group in the Pre-SC Period

Notes: This graph depicts a visually inspected parallel trend in the log of annual earnings during the pre-SC period between my treatment and control group. My treatment group consists of Hispanic men who work in treated (where Secure Communities was implemented) counties (red line), and my control group consists of Hispanic men who work in non-treated counties (blue line). Note that treated counties earn on average a higher income than non-treated counties.
### Table A: Testing the Significance of Gender Differences in the Effects of Secure Communities on Annual Earnings

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Log Annual Earnings</td>
<td>Gender Differences</td>
</tr>
<tr>
<td>(1 if 2010)*(=1 if SC county)*Gender</td>
<td>-0.0039 (0.012)</td>
</tr>
<tr>
<td>(1 if 2010)*(=1 if SC county)</td>
<td>-0.029*** (0.0090)</td>
</tr>
<tr>
<td>Gender</td>
<td></td>
</tr>
<tr>
<td>=1 if post-treatment year (2010)*Gender</td>
<td>-0.047*** (0.0094)</td>
</tr>
<tr>
<td>=1 if post-treatment year (2010)</td>
<td>0.0022 (0.0073)</td>
</tr>
<tr>
<td>Experience*Gender</td>
<td>0.014*** (0.0013)</td>
</tr>
<tr>
<td>Experience (years)</td>
<td>0.087*** (0.0010)</td>
</tr>
<tr>
<td>(Experience Squared)*Gender</td>
<td>-0.00024*** (2.37e-05)</td>
</tr>
<tr>
<td>Experience Squared</td>
<td>-0.0012*** (1.81e-05)</td>
</tr>
<tr>
<td>Education*Gender</td>
<td>-0.014*** (0.0017)</td>
</tr>
<tr>
<td>Education (years)</td>
<td>0.067*** (0.0013)</td>
</tr>
<tr>
<td>(Time in the US)*Gender</td>
<td>-0.00023 (0.00036)</td>
</tr>
<tr>
<td>Time in the US (years)</td>
<td>0.0018*** (0.00028)</td>
</tr>
<tr>
<td>Constant</td>
<td>8.3*** (0.010)</td>
</tr>
</tbody>
</table>

| Observations                     | 336,623                                  |
| R-squared                        | 0.43                                    |

Place of Work County FE  YES  
Place of Work County X Gender  YES  
Speaks English FE  YES  
Speaks English FE X Gender  YES  
Occupation FE  YES  
Occupation FE X Gender  YES  
Industry FE  YES  
Industry FE X Gender  YES  
Citizenship FE  YES  
Citizenship FE X Gender  YES

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Table B: Testing the Significance of Border Differences in the Effects of Secure Communities on Annual Earnings

<table>
<thead>
<tr>
<th>Log Annual Earnings</th>
<th>(1) Border Differences</th>
</tr>
</thead>
<tbody>
<tr>
<td>(=1 if 2010)* (=1 if SC county)*Border</td>
<td>-0.028 (0.018)</td>
</tr>
<tr>
<td>(=1 if 2010)* (=1 if SC county)*Border</td>
<td>-0.026** (0.011)</td>
</tr>
<tr>
<td>Border</td>
<td>-</td>
</tr>
<tr>
<td>(=1 if post-treatment year (2010))*Border</td>
<td>0.035** (0.015)</td>
</tr>
<tr>
<td>(=1 if post-treatment year (2010))</td>
<td>-0.055*** (0.0068)</td>
</tr>
<tr>
<td>Experience*Border</td>
<td>-0.0017 (0.0018)</td>
</tr>
<tr>
<td>Experience (years)</td>
<td>0.10*** (0.0013)</td>
</tr>
<tr>
<td>(Experience Squared)*Border</td>
<td>6.43e-05** (3.12e-05)</td>
</tr>
<tr>
<td>Experience Squared</td>
<td>-0.0015*** (2.36e-05)</td>
</tr>
<tr>
<td>Education*Border</td>
<td>0.00032 (0.0020)</td>
</tr>
<tr>
<td>Education (years)</td>
<td>0.052*** (0.0014)</td>
</tr>
<tr>
<td>(Years in the US)*Border</td>
<td>-0.0013** (0.00049)</td>
</tr>
<tr>
<td>Time in the US (years)</td>
<td>0.0023*** (0.00032)</td>
</tr>
<tr>
<td>Constant</td>
<td>8.4*** (0.014)</td>
</tr>
</tbody>
</table>

Observations: 186,293
R-squared: 0.43

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.
PLEDGE

This paper represents my own work in accordance with University regulations.

Yousra Zerouali Boukhal
04/30/20