The Labor Market Effects of Immigration Enforcement*

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Abstract

We examine the labor market effects of Secure Communities (SC)–an immigration enforcement policy which led to over 454,000 deportations between 2008-2015. Using a difference-in-differences model that takes advantage of the staggered rollout of SC, we find that SC significantly decreased the employment share of likely undocumented male immigrants. Importantly, the policy also led to a decrease in the employment rate of citizens. The employment effects are concentrated among male citizens working in higher-skilled occupations, particularly in sectors that traditionally rely on likely undocumented workers. This is consistent with complementarities in production between low-skilled immigrants and higher-skilled citizens.

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1 Introduction

Approximately 8 million undocumented immigrants participated in the U.S. labor market in 2015, constituting about five percent of the total U.S. labor force (Passel and Cohn, 2016). An increasing number of policies aimed at reducing the number of undocumented immigrants through deportations have been implemented in the past two decades, but it is still largely unknown how such policies have impacted the U.S. labor market and to what extent they have been costly or beneficial to U.S. citizen workers (Chassamboulli and Peri, 2015).[1]

This paper makes an important contribution to the literature by providing the first causal evidence of the local labor market impacts of a nationwide immigration enforcement policy on the employment outcomes of likely undocumented immigrants and its potential spillover effects onto citizen's labor outcomes. Specifically, we analyze the employment effects of one of the largest immigration enforcement policies in the U.S.: Secure Communities (SC). SC increased information sharing between local law enforcement agencies and the federal government in an attempt to detect and remove undocumented immigrants. The policy was ultimately adopted by all U.S. counties, and more than 454,000 individuals, 96% of whom were male, were removed under SC during 2008-2014.[2] As a result, SC caused a significant reduction in the availability of undocumented male immigrants through its direct impact on deportations and may have also reduced the supply of immigrant labor through "chilling effects" caused by the increased perceived risk of deportation among immigrants. These chilling effects of SC may have led to voluntary outmigration, reduced the number of incoming undocumented immigrants, and impacted the willingness of immigrants to work

[1] A large body of literature has focused on analyzing the effect of migration *inflows* on native wages and employment. See for example, Card (2001), Borjas (2003), Boustan et al. (2010a), and Dustmann et al. (2017). For excellent reviews of the literature see Friedberg and Hunt (1995), Longhi et al. (2005), and Longhi et al. (2006). Previous studies on the labor market impacts of recent immigration enforcement policies in the U.S. have mostly focused on the direct effects on the migrant population. See Phillips and Massey (1999), Bansak and Raphael (2001), Orrenius and Zavodny (2009), Amuedo-Dorantes and Bansak (2014), and Orrenius and Zavodny (2015).

[2] Statistics on removals under SC come from the Transactional Records Access Clearinghouse (TRAC). Other immigration enforcement policies in this time period differ from SC in their implementation and design. See Karoly and Perez-Arce (2016) for a summary of the literature on state immigration policies.

outside the home in order to limit interactions with the local police (Kohli et al., 2011; Valdivia, 2019).[3]

The implementation of SC provides an ideal natural experiment to estimate the effects of deportation policies on the labor market. First, because the Department of Homeland Security (DHS) was unable to simultaneously implement SC nationwide, the program was rolled out on a county-by-county basis over 4 years. Cox and Miles (2013) provide evidence that, after controlling for geographic and year fixed effects, the rollout of SC was largely exogenous to county characteristics such as crime or unemployment rates. We provide additional evidence on the exogeneity of the rollout of SC by showing that the adoption year of SC is, at best, very weakly predicted based on a large set of local-area pre-period demographic and economic characteristics. We also conduct an event-study analysis that shows no significant differences in trends in labor market outcomes before implementation. Thus, the timing of SC implementation can be thought of as plausibly exogenous and employment impacts are identified off of the differential timing of SC implementation across local areas. Second, the relative speed of the rollout, and the fact that all U.S. counties eventually adopted SC, limits the scope of cross-county mobility by immigrants and natives alike, and thus concerns about spatial arbitrage of employment should be minimal (Borjas, 2003; Borjas and Katz, 2007; Cadena and Kovak, 2016).

We use data from the 2005-2014 American Community Survey (ACS) and conduct the analysis at the commuting zone (CZ) level, a geographic unit frequently used to define a local labor market (Tolbert and Sizer, 1996; Autor et al., 2013; David and Dorn, 2013). We merge in annual SC exposure based on the population-weighted share of counties in the CZ that implemented the policy, and include this measure of SC exposure, along with CZ and year fixed effects, in a difference-in-differences model. Additionally, we include several CZ-by-year covariates and CZ linear trends to account for other changes in local immigration

[3]Additionally, Wang and Kaushal (2018) found that the implementation of SC and 287(g) agreements increased the share of Latino immigrants with mental distress.

policy and economic conditions over this period.

We first analyze the impact of SC on the employment share of likely undocumented immigrants, which we measure as the number of employed likely undocumented immigrants in the CZ-year, divided by base year total CZ population in 2005. We focus on the employment share of the population most affected by deportations: likely undocumented male immigrants. Because documentation status is not available in the ACS, we follow the literature (e.g. Orrenius and Zavodny (2009) and Amuedo-Dorantes and Bansak (2012)) and define our sample of likely undocumented male immigrants as male non-citizens with a high-school degree or less, and call this sample "low-educated non-citizens" ("LENC").[4] Given that most undocumented immigrants have low levels of education, this group captures a large portion of the undocumented population that will be directly affected by SC.

We find that, consistent with the policy's objectives, the implementation of SC substantially increased the number of detentions of immigrants. In addition, we find that SC led to a 7 percent decline in the employment share of likely undocumented male immigrants, relative to the local labor market's population in 2005. This "direct effect" is concentrated in sectors that traditionally rely on the labor of likely undocumented workers and is robust to alternative definitions of likely undocumented immigrants, based on year of arrival and country of origin (Passel and Cohn, 2014). Moreover, we document that the direct effect is primarily driven by Hispanic LENC who recently arrived in the U.S., who are not only very likely to be undocumented (Passel and Cohn, 2014), but also one of the groups most likely to be affected by SC, since 92% of individuals deported under SC were Latin American.

The implementation of SC also provides a unique opportunity to study the effect of large-scale removal of immigrant labor supply on the local labor market outcomes for *citizens*: the "indirect effect". We show that SC is associated with a 0.71 percent decline in the employment rate of all citizen workers. Moreover, consistent with the fact that SC primarily

[4]Non-citizens refer to foreign-born individuals who report not holding U.S. citizenship. In what follows we use "LENC" and "likely undocumented" interchangeably.

affected likely undocumented male immigrants, and the tendency of likely undocumented male immigrants to sort into male-dominated industries and occupations, we find that SC is associated with a larger decline (0.84 percent) in the employment rate of male citizens.[5] Thus, based on these estimates, we calculate that a 1 percent decline in the employment share of likely undocumented male immigrants is associated with a 0.12 percent decline in the employment rate of male citizens.

In order to shed light on the mechanisms through which this immigration enforcement policy impacts the employment of male citizens, we further divide the male citizen sample by Hispanic ethnicity, education, and occupational skill. This is motivated in part by recent theoretical and empirical research indicating that the effect of immigrants on citizens' labor market outcomes depends crucially on the skill composition of immigrants, and their degree of substitutability or complementarity with citizen workers across the skill distribution (Borjas, 2003; Ottaviano and Peri, 2012; Chassamboulli and Peri, 2015; Dustmann et al., 2017; Lee et al., 2019). In particular, low-skilled citizens are more likely to be substitutes for the low-skilled immigrants directly affected by SC, and therefore may experience an increase in employment after its implementation. On the other hand, high-skilled citizens are more likely to be complements, so they may experience a decrease in employment after SC.

We find a positive (though imprecisely estimated) effect on the employment rate of Hispanic low-educated citizen men, who are plausibly the closest substitutes for immigrants directly affect by SC. Interestingly, when we do not condition on Hispanic ethnicity, we find negative effects on both low-educated and high-educated male citizens. Splitting the sample into four skill groups containing occupations based on the share of workers with at least a college degree reveals important heterogeneity in the direct and indirect effects of SC.[6]

[5] Using data from the ACS in 2005, 76 percent of LENC males worked in a male-dominated industry, and 80 percent worked in a male-dominated occupation; where "male-dominated" is defined as industries or occupations with more than 50 percent of male workers.

[6] To construct occupational skill specific employment rates for citizens, the numerator measures the number of workers in each demographic and occupational skill group, and the denominator measures the number of people in each demographic group, irrespective of whether they work or their occupation.

The decline in the employment rate of male citizens is concentrated in the second and third quartiles of the occupational skill distribution, in occupations such as construction managers and food service managers. In contrast, SC reduced the employment share of LENCs in the first and second quartiles of the occupational skill distribution, such as construction and food preparation workers.[7] Thus, the results indicate that while some citizens may act as substitutes to undocumented immigrants, many citizen workers (both low- and high-educated) in higher-skilled occupations act as complements. To provide further support for complementarities in production between male citizens and likely undocumented male immigrants, we show that the negative indirect effect is driven by sectors with a higher share of LENC workers pre-SC.

This paper makes several contributions to the literature of the effect of immigration on citizen outcomes. First, we contribute to a small but growing literature that uses policy-driven changes to immigrant labor supply, which rely on testable assumptions about the exogeneity of the policy changes. This is in contrast to a large literature that has used a shift-share approach in the spirit of Card (2001), which is important because recent work has questioned the identifying assumptions in this approach for studying immigration (Jaeger et al., 2018). Second, unlike previous studies which examine the labor market effects of immigration inflows, we estimate the impact of reducing the supply of undocumented immigrants on labor market outcomes. This is an important distinction because reducing the supply of a more assimilated group of immigrants is likely to generate different short-run adjustments compared to adjustments in response to an inflow of newly arrived immigrants (Acemoglu, 2010). We are aware of only two papers that examine the labor market impacts of migrant outflows. Clemens et al. (2018) provide historical evidence that reducing the supply of Mexican Bracero farm workers at the end of 1964 did not impact the employment or wages of domestic farm workers because firms absorbed the decrease in the availability of low-skilled

[7] Appendix Tables (A4) and (A5) reports the 10 most common occupations by quartiles of the occupational skill distribution for the sample of LENCs men and for different groups of citizen men.

labor by changing their crops and adopting new technologies. In another historical context, Lee et al. (2019) study the effect of the repatriation of Mexican-born migrants living in the U.S. between 1930 and 1940 and find this led to a decrease in the probability of employment for native workers; they argue that the likely mechanism is related to an increase in firms exits, which reduced demand for native workers.[8] Our findings build upon those papers by examining the effects of a *contemporary* deportation policy, which affected a wide range of industries, on the labor market outcomes of both low- and high-skilled citizen workers. Our results also highlight the importance of a different mechanism from these historical papers–production complementarities between low-skilled immigrants and higher-skilled citizens–in explaining how a contemporary immigration enforcement policy impacts the employment of citizens.

Finally, the paper contributes to an important policy debate on the effects of deporting undocumented immigrants on the labor market. This is particularly relevant since SC was reactivated in January of 2017 (SC was replaced by the Priority Enforcement Program at the end of 2014) and President Trump has recently proposed expanding other similar enforcement programs (Alvarez, 2017; Sakuma, 2017). To the best of our knowledge, the only existing evidence on the spillover effects of SC on the labor market outcomes of citizen workers is provided by East and Velasquez (2019) in which the authors document a negative spillover effect of SC on the labor supply of high-skilled mothers with young children, which is operating through an entirely different mechanism than that studied here. In particular, the effect on high-skilled mothers is due to a decrease in the labor supply of low-educated Hispanic immigrant women working in household and childcare related occupations, which is expected to increase the price of outsourcing home production (Cortes, 2008). In contrast, our results indicate that the mechanism for the indirect effect on citizen men operates through

[8]Ager and Hansen (2018) study the effects of immigration quotas in the 1920s, which restricted new inflows of immigrants. They find negative effects of the quotas on native wages and Abramitzky et al. (2019) provide evidence that this may be due to effects on native migration within the U.S., and firm capital investment decisions. Importantly, we find no evidence that SC caused citizens to migrate within the U.S.

direct substitution and complementarities in *market* production.[9]

The paper proceeds as follows. Section 2 describes the SC program. Section 3 describes our data sources and the construction of the analysis sample. Section 4 outlines the empirical strategy, and we discuss the results in section 5. We conclude in section 6.

2 Policy Background

Secure Communities (SC) is one of the largest interior immigration enforcement programs and is administered by the U.S. Immigration and Customs Enforcement (ICE).[10] SC's main objectives were to identify undocumented immigrants arrested by local law enforcement agencies, and to prioritize their deportation. In practice, SC facilitated information sharing between local and state law enforcement agencies, the Federal Bureau of Investigation (FBI), and the Department of Homeland Security (DHS). Usually, local law enforcement agencies conduct a criminal background investigation after a person is arrested by sending their fingerprints to the FBI. Prior to SC, fingerprints received by the FBI were not used to check the legal status of a person or their eligibility for removal.[11] Under SC, the fingerprints were also automatically sent to ICE, who subsequently ran the fingerprints against their biometric database, known as the Automated Biometric Identification System (IDENT) to determine an individual's immigration status.[12]

At this point, "detainers could be issued when an immigration officer had reason to

[9]Previous research indicates the effect of a change in the cost of outsourcing home production is unlikely to affect men (Cortes and Tessada, 2011).

[10]For excellent reviews of the Secure Communities program's implementation see Cox and Miles (2013), Miles and Cox (2014), and Alsan and Yang (2018). The information in this section comes primarily from these reviews.

[11]Instead, violators of immigration law were identified via interviews conducted by federal agents under a program called the Criminal Alien Program (CAP), or by local agents authorized to act as immigration agents under written voluntary agreements with the DHS: 287(g) agreements.

[12]IDENT includes biometric and biographical information on non-U.S. citizens who have violated immigration law, or are lawfully present in the U.S., but have been convicted of a crime and are therefore subject to removal, as well as naturalized citizens whose fingerprints were previously included in the database. In addition, the IDENT system includes biometric information on all travelers who enter or leave the U.S. through an official port, and when applying for visas at U.S. consulates.

believe the individual was removable," which could be for criminal reasons or for immigration-crime-related reasons. A detainer (or deportation) did not have to be preceded by a conviction.[13] The detainer required state or local law enforcement agencies to hold an arrested individual for up to 48 hours until ICE could obtain custody and start the deportation process. Thus, a detainer prevented the release of individuals whose cases were dismissed and, for those who were charged with a crime, did not provide them the opportunity for a pre-trial release through bail. As a result, conditional on being arrested, the administration of SC substantially increased the probability of apprehension and deportation of non-citizens by ICE.

Unlike previous voluntary information sharing programs, SC is a federal program, and local and state law agencies could not "opt in" or "opt out" of SC. For empirical purposes, this is important for two reasons. First, local agencies have much more limited discretion in the usage of the program, compared to other interior immigration enforcement polices (Miles and Cox, 2014).[14] Second, despite being a federal program, SC was rolled out on a county-by-county basis between 2008 and 2013, until the entire country was covered. We gathered information on the rollout dates of SC from the U.S. Immigration and Customs Enforcement (ICE). Our empirical strategy, described in more detail below, relies on the piecemeal implementation of SC across counties. Therefore, it is important that the timing of the rollout across counties not be related to time-varying county characteristics. Cox and Miles (2013) show that the earliest activations were related to the fraction of the county's Hispanic population, distance from the U.S.-Mexico border, and presence of local 287(g) agreements. Importantly for the purpose of our study, their results also show that early adopters were not selected in terms of the county's economic performance, crime rates and

[13]This policy language taken from the ICE website, is available here: `https://www.ice.gov/pep`.

[14]After the activation of SC, some jurisdictions known as "sanctuary cities" started refusing to cooperate with ICE detainer requests by claiming that the policy was unconstitutional under the Fourth Amendment. Using information from ICE on locations that were sanctuary cities under this definition, almost all became sanctuary cities in 2014, which in part motivate the end of SC. We explore whether the results are robust to dropping sanctuary cities in Section 5.

potential political support to SC. In addition, the timing of adoption in subsequent counties was more "random" because the government shifted to mass activations, and this was based on resource constraints and waiting lists (Cox and Miles, 2013). This pattern can be seen in Figure (1) which plots the rollout of SC across counties and over time.[15] In our main sample we include the whole country, but the main results are robust to excluding early-adopter areas.

We also examine whether changes in pre-SC demographic and economic characteristics between 2005 and 2007 at the CZ level predict the year when SC was adopted. The first column of Appendix Table (A1) report average changes in CZ characteristics while the second column reports the standard deviation of these changes. In column 3 we report estimates of the relationship between changes in CZ characteristics (such as the change in the share of non-citizens, the change in the share of low-educated male non-citizens, and a measure of changes in housing prices) and the year of SC adoption.[16] Out of 11 pre-SC characteristics, the only two statistically significant variables are the change in 287(g) Jail agreements and the 2000-2006 change in housing prices. However, although significant, the effects are small: an increase of one standard deviation in exposure to 287(g) Jail agreements is associated with a 3.57 months earlier adoption of SC.[17] Likewise, an increase of one standard deviation in the change of housing prices is associated with a 2.5 months earlier adoption of SC.[18] In our main model, we control for the presence of 287(g) agreements and for trends in pre-SC housing prices.

[15] Alsan and Yang (2018) provide additional evidence on the selectivity of earlier adopters by testing whether differences in demographic characteristics between Hispanics and other ethnic groups before the activation of SC were significantly different in early versus later adopters. Relevant for their study, they find that differences in food stamp take-up between different ethnic groups are not related with the timing of SC activation.

[16] In order to test whether the housing price boom predicts the timing of the rollout of SC, we follow Charles et al. (2018) and define the housing price boom as the change in housing prices between 2000 and 2006 divided by prices in 2000. Because housing price information is missing for some CZs, we also report estimates in column 4 of Table Appendix Table (A1) where we exclude changes in housing prices.

[17] This is calculated as follows: -2.13*0.14*12=-3.57.

[18] This is calculated as follows: -0.512*0.41*12=-2.51.

We expect SC to have affected the immigrant employment share of the population through two main channels. First, SC reduced the number of low-skilled workers by removing undocumented immigrants through detainers and eventual deportations.[19] As shown in Appendix Table (A2), over the period 2008-2014, 20 percent of deported individuals were not convicted of a crime, and among those who were convicted, it was often not a serious crime. Among the deportees, 7 percent had a traffic violation, 11 percent had a DUI, 2 percent had a crime related to marijuana, and 7 percent had illegal entry or re-entry as their most serious criminal conviction. Thus, a broad swath of the undocumented population may have been affected, and not just the most serious criminals (Amuedo-Dorantes et al., forthcoming). Second, fear of detentions and deportations may have reduced the labor supply of undocumented immigrants and impacted their job search efforts. Anecdotal evidence suggests that immigrant communities believed that SC allowed police officers to act as ICE agents, and advocacy groups suggested that SC provided a way for law enforcement to use minor violations to target the Hispanic population (Kohli et al., 2011). Consequently, fear of driving a car, interacting with law enforcement, or having to present forms of identification, may have limited the participation of immigrants in the labor market (Valdivia, 2019).[20] Moreover, increased immigration enforcement could have changed the number of undocumented immigrants by increasing voluntary out-migration from the U.S., or by reducing in-migration to the U.S. Finally, SC may have also impacted the labor supply of documented immigrants because the documented and undocumented populations are heavily integrated (Alsan and Yang, 2018).[21]

[19] At the end of 2014, the SC program was replaced by the Priority Enforcement Program (PEP). Under PEP, the same screening process occurred as did under SC, but PEP focused more on individuals convicted of serious crimes or those who were deemed to pose a threat to public safety. We use restricted-access data on deportations and detentions under SC from the Transactional Records Access Clearinghouse (TRAC) at Syracuse University, to provide context for understanding the potential effects of SC. Details about this data can be found in Appendix A.

[20] SC could have also directly increased the uncertainty of hiring an undocumented immigrant and hence increased their labor costs.

[21] The screening process by ICE is subject to error, and roughly 2% of individuals who were identified for deportation by ICE under SC turned out to be citizens, thus SC may result in fear of being held in custody or detained among documented individuals (Kohli et al., 2011).

In addition to East and Velasquez (2019), a few papers have analyzed other impacts of SC. Cox and Miles (2013) examine the characteristics of counties in relation to their date of SC implementation which we rely on for some of the information provided above. Miles and Cox (2014) and Hines and Peri (2019) show that SC did not lead to a decline in the crime rate. In support of spillover effects on the documented immigrant population, Alsan and Yang (2018) find that SC reduced participation in the Supplemental Nutrition Assistance Program (SNAP) and the Supplemental Security Income program (SSI) among Hispanic citizens. Finally, Bellows (2018) provides evidence that the implementation of SC was associated with a decline in the achievement of Hispanic, although this was also accompanied by a decline for non-Hispanic black students who are not expected to be similarly affected.

A related literature has examined the effects of other immigration policies on employment, and these analyses are informative for thinking about the potential effects of SC.[22] A number of studies have examined the effects of the 287(g) agreements, which deputize local law enforcement agencies to enforce immigration law. Like SC, 287(g) agreements act as a mechanism to check the immigration status of individuals interacting with the criminal justice system and as a pathway for initiating deportations. These papers find that the presence of a 287(g) agreement in a local area reduces total employment in that area, with mixed effects in industries in which undocumented immigrants are overrepresented. However, this effect is not disaggregated across immigrants and natives, or across low- and high-skill occupations, so it is unclear what is the direct effect of enforcement on immigrants' employment and what may be spillover effects due to substitution or complementarities in production (Pham and Van, 2010; Bohn and Santillano, 2017).[23]

[22] Several papers include SC as part of a summary index of interior immigration enforcement; see for example Amuedo-Dorantes and Lopez (2017).

[23] Watson (2013) examines the effect of 287(g)s on migration and finds they do not cause immigrants to leave the United States, but they do increase migration to a new region within the United States. These migratory effects are concentrated in Maricopa County, AZ and among the college-educated foreign-born, who are unlikely to be undocumented. Moreover, the effect of 287(g)s on migration is likely different than the effect of SC, since 287(g)s were optional and not all locations had an agreement.

3 Data

To measure the labor market effects of SC, we merge information on the rollout dates of SC with data on local-level employment drawn from the 2005-2014 American Community Survey (ACS) Integrated Public Use Microdata Series (IPUMS) (Ruggles et al., 2017). The ACS is a repeated cross-sectional dataset covering a 1% random sample of the U.S. We begin our sample in 2005, as this is the first year we can identify the Public-Use Microdata Area (PUMA) geographic level in the public-use data, and end in 2014 when SC was replaced by the Priority Enforcement Program. We conduct our analysis at the Commuting Zone (CZ) level.[24] The main advantage of using CZs as our unit of analysis is that they are designed to provide a measure of local labor markets, while representing both metropolitan and rural areas (Dorn, 2009). We concord the PUMA-level data to the CZ-level following Dorn (2009) and David and Dorn (2013). From the enforcement data, we observe the month and year SC was activated in each county. CZs include several counties, so we calculate the population-weighted average of the county values of the SC variable within each CZ, similar to the approach taken by Watson (2013).[25] In addition, since the ACS data only includes the year in which the survey was conducted, we create a variable that indicates the fraction of the survey year SC was in place in each CZ.

To estimate the direct effect of SC on the employment share of likely undocumented male immigrants, we count the number of male working-aged (20-64) LENC in each CZ-year who report working at the time of the survey, then divide this by the CZ's total working-age population in the base year (2005), and multiply these employment shares by 100 to ease the presentation: $\frac{ImmigEmp_{jt}}{Pop_{j2005}} * 100$. Thus, this outcome variable captures changes in the employment share of likely undocumented male workers due to deportations, voluntary

[24] We use 1990 CZ definitions and exclude workers in the military and public administration sectors from the main sample because of the potential direct impact of SC on the employment of citizens in these sectors. Our results are robust to including these sectors and also conducting our estimation at the PUMA level rather than CZ level.

[25] We weight the value of the SC variable for each county by the fraction of the total CZ population that each county represents.

migration, or chilling effects. Fixing total working-age population in 2005 allows us to isolate changes from employment from changes in population size.[26] To calculate both the numerator and the denominator, we use the ACS-provided person-level weights. We also report results from using more restrictive definitions of "likely undocumented" immigrants. For instance, we restrict the sample to foreign-born non-citizens with a high school education or less, who were born in Mexico or Central America and entered the U.S. after 1980, and Hispanic foreign-born non-citizens with a high school education or less who entered the U.S. after 1980 (Passel and Cohn, 2014).[27]

Similar to Dustmann et al. (2005) and Boustan et al. (2010b), the employment rate of citizens is calculated as the number of working aged (20-64) citizens in each CZ-year who report working at the time of the survey divided by the population of citizens in the CZ-year and multiplied by 100 to ease presentation: $\frac{EmpCitizen_{jt}}{PopCitizen_{jt}} * 100$.[28] We construct corresponding employment rate outcomes for demographic subgroups split by gender, Hispanic ethnicity, and education, where the numerator is the number of employed individuals in the subgroup and the denominator is the population of the subgroup.

To better understand the effects of SC on the labor market across the skill distribution, we calculate corresponding measures of our main outcome variables across 3-digit SOC occupations classified based on the fraction of workers that have at least a college degree in each occupation in 2005 (the base year of our sample). Figure (A2) shows the distribution of this measure across occupations. The median occupation has roughly 13 percent of workers with a college degree, and the cutoffs for the 25th and 75th percentiles are 5 and 42 percent, respectively. We generate four skill groups of occupations, based on the four quartiles of the distribution, and calculate the employment shares for likely undocumented keeping the

[26]We expect changes in the LENC population size due to deportations, which we test for in section 5.3.

[27]Results are very similar if we use 1986 as the year of entry cutoff instead of 1980 as in (Amuedo-Dorantes and Bansak, 2012, 2014; Orrenius and Zavodny, 2015). Results available upon request.

[28]SC's impact on the employment rate as defined here can be the result of changes in the number of employed citizens or by changes in a CZ's citizen population at time t. We provide evidence in section 5 that SC had no impact on the CZ-level citizen population size.

denominator the same as before (total CZ population in 2005).

To calculate these variables for employment rates for the different citizen groups, we divide the number of employed individuals in each occupational skill group by the total number of individuals in the corresponding demographic group, unconditional on occupation since not all are working. Similarly, to calculate these variables for employment shares for the non-citizen groups, we divide the number of employed individuals in each occupational skill group by the total CZ population in 2005.[29]

Since our sample period spans the Great Recession, we account for changes in economic conditions that may influence employment by including "Bartik-style" measures of labor demand (Bartik, 1992) and a control for a housing boom trend (Charles et al., 2018). We also control for the presence of 287(g) agreements across CZs in our sample period. These controls are described in detail in Appendix A. We show summary statistics for all main variables in Table (1).

4 Empirical Strategy

Our empirical strategy uses both the geographic and temporal variation in the implementation of the SC program to identify its effect on CZ-level employment of non-citizen and citizen workers. In order to identify the causal effect of adopting SC on local employment, we estimate the following model:

$$Y_{jt} = \alpha + \beta SC_{jt} + X'_{jt}\gamma + \nu_j + \lambda_t + t\delta_j + \epsilon_{jt} \tag{1}$$

[29]For example, when we look at the employment shares of low-educated citizen males in the first occupational skill quartile, the numerator is the number of low-educated citizen males working in the first occupational skill quartile, and the denominator is the total CZ population in 2005. Splitting our sample by occupations, rather than simply by education, enables us to identify whether changes in the labor demand for citizens and non-citizens is occurring within versus across occupations, providing a better understanding of the interaction between these two types of labor in production. Results are very similar if we instead stratify occupations by average wages, or the percent of the occupation with less than a high school degree.

As described in the data section, SC_{jt} is a continuous variable indicating CZ-level exposure to SC and ranges between zero and one. Once SC has been implemented by January of year t in all counties in a CZ j, the variable SC_{jt} takes a value of one for the remainder of the sample. Therefore, β measures the effect of 100 percent of the CZ population being covered by SC for the entire survey year. The baseline model is weighted by the CZ population in 2000.[30] The model includes year fixed effects, λ_t, to account for national economic shocks, and fixed effects at the CZ level, ν_j, to control for time-invariant unobserved heterogeneity, such as the pre-SC share of Hispanics and proximity to the border. To account for differential trends in employment within CZs over time, we first include a parametric control defined as the CZ-level change in housing prices between 2000-2006 interacted with a linear trend, following Charles et al. (2018) and Appendix Table (A1). We then explore more flexible controls for economic conditions including CZ-by-year linear trends, $t\delta_j$, and Bartik-style measures of labor demand.[31] Finally, we also examine the sensitivity of the results to including controls for 287(g) agreements.

The underlying identification assumption is that there were no time-varying CZ-specific factors which were correlated with the timing of the adoption of SC across local areas. To provide support for this assumption, we test for parallel pre-trends by estimating the effect of SC on employment before and after the implementation of SC through an "event study" model as follows:

$$Y_{jt} = \alpha + \sum_{\substack{k=-3 \\ k\neq -1}}^{4} \beta_k 1_{jk} + X'_{jt}\gamma + \nu_j + \lambda_t + t\delta_j + \epsilon_{jt} \tag{2}$$

[30] The results are robust and are very similar when estimating a model without weights, providing evidence that measurement error and heterogeneous treatment effects are not a large concern (Solon et al., 2015). We do not include state by year fixed effects because 10 states and the District of Columbia implemented SC on a state-wide basis. These states are Alaska, Delaware, DC, Main Minnesota, New Hampshire, New Jersey, North Dakota, Rhode Island, Vermont, West Virginia, Wyoming. Appendix Figure (A1) plots the share of counties within each state that had SC over time. However, results are very similar when we include Census region by year fixed effects.

[31] The results are similar if we instead only model pre-trends and use this to predict post-treatment trends, which is preferred if there are dynamic treatment effects (Wolfers, 2006; Lee and Solon, 2011; Goodman-Bacon, 2016; Borusyak and Jaravel, 2017).

We classify a CZ j as treated if SC covers 50 percent or more of its population. β_k identifies the effect of SC on the employment share of likely undocumented immigrants or the employment rate of citizens in CZ j and year k, where k indicates how far each CZ-year observation is from SC implementation in that CZ. So, for example, β_1 estimates the effect in the *year of SC implementation*. The excluded group is $k = 0$ (the year before SC implementation) and all marginal effects should be interpreted as relative to the year before implementation. In order for our identification strategy to be valid, there should be no discernible differential trends present before SC's implementation. However, we note that the approach in the event study design is not exactly the same is in the difference in difference model in equation (1), because we use a continuous measure of SC treatment in equation (1), whereas the event study assigns dichotomous treatment status. However, we still view this as an informative test of our identification strategy.

We report the results of this analysis with the fully saturated model including CZ and year fixed effects, Bartik controls, 287(g) controls, and CZ linear trends in Figure (2). In Panel A we estimate equation (2) on the sample of low-educated non-citizen men, where the blue dots show the effect of SC, and dashed lines represent 95 percent confidence intervals. Panel B plots the coefficients from estimating the event study on the employment rate of all working age citizens while Panel C limits the sample to male citizens. The results across the three panels provide no evidence that the immigrant employment share or the employment rate of citizens (or male citizens) were following a differential trend across locations prior to the adoption of SC. There is, however, clear evidence of a decline in the number of employed immigrants, and in the employment rate of citizens following the implementation of SC. The increasing magnitude of the effect after SC implementation could be due to dynamic treatment effects, or to the fact that SC phases in over time across CZs, or to the fact that, because our sample ends in 2014, the post period coefficients in this event study are not all estimated on a balanced sample of CZs. In order to ensure this unbalanced sample is not driving the post-period pattern of results, Appendix Figure (A4) plots the event study estimates focusing on a sample of CZs that adopted SC before 2013 for which we can observe for post period years. This pattern of results in both the pre and post period is very similar.

5 Results

5.1 Direct Effect of SC on Likely Undocumented Immigrants

We begin by presenting the effects of SC on the employment share of likely undocumented immigrants ("LENC") in Table (2). Recall that changes in the employment share of likely undocumented male immigrants with the fixed base year population denominator can occur because of changes in the presence of workers in the U.S. or changes in the likelihood of working among those who remain in the U.S. Moving across columns we estimate the effect of SC using different definitions of likely undocumented male immigrants. The first column of Table (2) shows the results for our main sample of likely undocumented immigrants–male LENC. The results in Panel A, where we only control for CZ and year fixed effects, indicate that SC led to a decline in the employment share of LENC men of 0.30 percentage points, significant at the one percent level. In Columns 2-4 we use more restrictive definitions of likely undocumented immigrants to focus on groups most likely to have been affected by SC. In Column 2, we restrict the sample to Hispanic LENC. In Column 3, we restrict the sample to Hispanic LENC who entered the U.S. after 1980, and in Column 4 we restrict the sample to LENC who were born in Mexico or Central America and entered the U.S. after 1980. The estimated decline in the employment share across these alternative samples of likely undocumented immigrants ranges between 0.198 and 0.276 percentage points, and they are all statistically significant at conventional levels. Because the denominator in all four columns is total CZ population in 2005, we can compare the magnitude of the estimates across the columns, and infer that 92 percent of the decline of male LENC employment is coming from changes in the employment of Hispanic LENC, who are more likely to be undocumented ($.92 = -2.76/-0.3$).

We test the robustness of these results by adding different sets of controls across the different panels. In Panel B, we add a measure of the housing boom interacted with a linear trend.[32] This is a parametric way to control for the differential impact of the Great Recession across CZs. The addition of this control has little impact on the estimated effect of SC. In Panel C we replace the housing trend with a more flexible CZ-specific linear trend. Again, the results are slightly smaller compared to the estimates in Panel A but are very similar to those reported in Panel B, and remain statistically significant. The addition of Bartik-style controls (in Panel D) or controlling for the presence of 287(g) agreements (in Panel E) reduces the size of the coefficients slightly but does not affect statistical significance. The results based on our preferred model, in Panel E, indicate that SC is associated with about a 6.8 percent (0.197/2.90) decline in the employment share of LENC men, and this effect size is similar when using alternative samples of undocumented immigrants.

Using our preferred specification in Panel E, we next explore heterogeneous effects across the *occupational* skill distribution in Table (3). We focus on the effects for male LENC, since this includes all of the other subgroups from Table (2), although the results are very similar for all the other subgroups. For convenience, we report the estimate from Panel E of Table (2) in the first column of Panel A. In Columns 2-5, we report the impact of SC by quartiles of the occupational skill distribution. Note that across all panels and columns the denominator is the same–total CZ population in 2005 divided by 100–however, the numerator changes across columns depending on the occupational skill group of interest. Focusing on the sample of all LENC in Panel A, moving across columns, the results indicate that the decline in the employment share of likely undocumented immigrants is concentrated in the lowest two quartiles of the skill distribution. Specifically, SC is associated with a 0.104 and 0.073 percentage points decline in the employment share of LENC men, both

[32] Note, the sample size shrinks slightly because housing price information is missing for some CZs.

significant at the 5 percent level. In proportional terms, the estimates imply that SC led to a 6 percent (0.104/1.73) decrease in the employment share of likely undocumented workers in occupations in the first quartile of the distribution, and about 8 percent (0.073/0.87) decline in the employment share in occupations in the second quartile. To give context to these findings, we report the top 10 most common occupations by quartile for LENC men in Appendix Table (A4). The first and second quartiles of the occupational skill distribution include occupations such as construction laborers, chefs and cooks, agricultural workers, carpenters, painters, and food preparation workers.

Importantly, the results are not sensitive to the choice of cutoffs in the skill distribution. Figure (3) plots the estimated coefficients from our main specification for the alternative samples of likely undocumented workers by gradually shifting the occupational skill group to include occupations with a higher share of college educated workers (a "moving window" approach). In addition to plotting the estimated coefficients, we also plot the employment share of each group of workers in 2005 across the same occupational bins. It is clear from Panels A-D in Figure (3) that SC led to significant declines in the employment share of likely undocumented male workers in the bottom half of the occupational skill distribution and that this is consistent across all groups of likely undocumented men. Reassuringly, these occupations appear to be the same ones that include a large share of likely undocumented workers in the pre-SC period.

We next examine the effects on likely undocumented immigrants across industries. Appendix Figure (A3) shows the distribution of the share of LENC workers by industry in 2005. The median industry has about 4 percent LENC workers as a fraction of its total workforce (shown in the black line), but it is clear from this figure that there are many industries that do not employ LENCs, and some industries that very heavily rely on LENC labor.[33] To explore heterogeneity of the direct effect of SC across industries, we estimate the

[33]We have compared the fraction of LENCs across sectors with published statistics on the fraction of undocumented immigrants across sectors released by the PEW Center, and while the levels are slightly

results of equation (1) for sectors with above and below 4 percent share of LENC workers in 2005, and report the results in Panels B and C of Table (3), respectively.[34] The results in Panel B show that the effects of SC are concentrated in sectors with a higher initial share of LENC workers. Specifically, in these sectors, the implementation of SC is associated with a 6 percent (0.094/1.56) and 10 percent (0.082/0.79) decline in the employment share of LENC workers in the first and second quartile of the skill distribution, respectively. In contrast, we find little evidence that SC is associated with a decline in the employment share of likely undocumented immigrants in sectors that do not traditionally rely on LENC labor.

Our primary estimate indicates that, on average, SC is associated with a 0.197 percentage points decline in the employment share of LENCs. To gauge the plausibility of this estimate, we compare it to the estimated number of deportations in the entire U.S. during 2004-2008 (454,000) as a share of U.S. population in 2005, which assumes that deportations were evenly distributed across CZs. The share of deportees at the national level is equal to 0.262 ($\frac{454000}{1.73\times10^8}$) which is about 33 percent higher that our main estimate. This is perhaps not surprising since not all deportees were employed and because, as we have shown, the impact of SC varies across local labor markets along important margins such as the initial share of LENC and the composition of industries across CZs, whereas the estimated effect of 0.197 is the average effect. This exercise provides evidence that the magnitude of our estimate are reasonable, given the scale of the policy, however, it should be noted that a limitation of our research design is that our estimates can only be used to make statements about differential effects across areas, not total levels changes. This is due to the fact that time fixed effects implicitly difference out any general equilibrium effects of the policy (see the discussion in Nakamura and Steinsson (2018)). Therefore our estimates should not be used to estimate

different, the rank is similar (Passel and Cohn, 2016).

[34]The sectors above median are: Agriculture, Forestry and Fisheries; Construction; Manufacturing; Wholesale, Retail; Business and Repair Services; Personal, Entertainment, and Recreational Services. The sectors below median are Mining; Transportation and Utilities; Finance, Insurance, and Real Estate; Education, Health, and Other Services. Recall we drop both Public Administration and Military Sectors from all of our analysis.

the total employment effect of SC nationally, and we refrain from producing such estimates in the remainder of the paper.[35]

To further verify that the implementation of SC affected the likely undocumented population, we directly estimate the effect of SC implementation on detentions using restricted-access data from TRAC.[36] Appendix Table (A3) reports estimates of the impact of SC on the number of detentions at the CZ-level scaled by the total CZ population in 2005. Using the full set of controls as described in equation (1), the results in column 5 indicate that SC is associated with a 0.091 percentage points increase in the detentions per population, or an increase of about 90 percent relative to the mean. This is further evidence that SC led to a significant decline in the pool of likely undocumented labor.

In sum, the results provide strong evidence that the implementation of SC led to a significant decline in the employment share of likely undocumented immigrants, particularly in low-skilled occupations and in industries that relied more on low-skilled immigrant labor pre-SC. In the next section, we explore whether these effects had a spillover effect onto the employment rates of citizens.

5.2 Indirect Effect of SC on Citizens

We start by presenting results on the effects of SC on the employment rates of all citizens in Table (4). Overall, we find that SC implementation resulted in a negative and significant reduction in the employment rate of all citizens. We test the robustness of the model to the same controls we included for the direct effect analysis across the columns. Adding a CZ-specific linear trend reduces the estimated coefficient compared to a model where we control parametrically for a trend in housing prices. However, adding the Bartik variables

[35]In order to make estimate the total effect of SC on employment national, one would need to structurally model the general equilibrium effect of SC on prices as in Waugh (2017). This is outside the scope of this paper.

[36]We are unable to directly estimate the impact of SC on deportations because TRAC did not collect pre-SC data on deportations. The data on detentions is described in Appendix A.2.

or controlling for the presence of 287(g) agreements does not impact the estimates. The results of our preferred specification in column 5 of Table (4) indicate that SC led to a 0.480 percentage points decline in the employment rate of citizens significant at the 5 percent level. Thus, relative to a mean employment rate of 67.48 percent, SC is associated with a 0.71 percent decline in the employment rate of citizens.

Having established a net negative effect on citizen employment, we next split the sample of citizens by demographics and alternative measures of skill to understand the mechanisms through which this effect is operating. In Table (5), we replicate the effect from our preferred specification in the first column and restrict the sample to male citizens in the second column. The results indicate that SC led to a 0.590 percentage points decline in the employment rate of male citizens, a proportional decrease of about 0.8 percent (0.590/70.64). In the third column, we focus on a sample of men with a high school degree or less (low-educated) and in the fourth column, we further restrict the sample to low-educated Hispanic men. In the final column, we focus on the sample of men with more than a high school degree (high-educated). The results indicate that SC is associated with a decrease in the employment rate of both high- and low-educated male citizens (Columns 3 and 5). Interestingly, while SC led to a decline of about 1.1 percent (0.740/65.08) in the employment rate of low-educated male citizens (Column 3); it had a positive, although not precisely estimated, impact on the employment rate of low-educated Hispanic male citizens (Column 4). The increase in the employment rate of low-educated Hispanic men after SC suggests that this group–who are the most similar to the likely undocumented population directly affected by SC along many observable demographics characteristics–may be substituting for the labor of likely undocumented immigrants.

Previous evidence documents complementarities in production between low and high-skilled workers both overall and in the context of immigration (e.g. (Katz and Murphy, 1992; Ottaviano and Peri, 2012)), which may explain the negative effect on high-educated male citizens and we return to this below. However, it is not immediately clear why the effects on low-educated male citizens are also negative. To better understand this, we next explore the effects of SC on male citizens across the occupational skill distribution, as we did with the direct effect above. The results in Panel A of Table (6) suggest that SC increases the employment rate of male citizens in the lowest quartile of the skill distribution–who may be the closest substitutes to LENCs–but this coefficient is not statistically significant.[37] In contrast, SC results in a 0.194 percentage points (1.27 percent) decrease in the employment rate of workers in the second quartile of the skill distribution, significant at the 10 percent level, and a 0.454 percentage points (2.6 percent) decline in the employment rate of workers in the 51-75th percentile of the occupational skill distribution, significant at the 1 percent level. Figure (4) plots the estimated coefficients from our main specification for the sub-groups of male citizens using the same moving window approach across occupational skill as in the direct effect analysis. In these figures, it is clear that the decline in employment rates for both low and high-educated citizen men is coming from the middle to upper-middle part of the occupational skill distribution–above the 20th percentile for low-educated male citizens and above the 40th percentile for high educated male citizens. This suggests that the negative effects on both low-educated citizen men may also be operating through complementarities in market production between high and low-skilled workers. To further examine this, we list the top 10 occupations of male citizens by quartile of the skill distribution in Appendix Table (A5). Focusing on the first column of all citizen men, occupations in the second and third quartile include many positions that could be described as supervisory of workers that are likely to be LENC: First-Line Supervisors of Construction and First-Line Supervisors of Production, Construction Managers, Farmers and Ranchers, and Food Service and Lodging Managers. Across the columns it is clear that the most frequent occupations do not differ much for the low- and high-educated samples. This supports the idea that both *low-* and

[37] Note that, as discussed above, while the numerator of the employment rate changes across columns depending on the occupational skill group, the denominator is fixed across the columns and is equal to the total CZ male citizen population divided by 100.

high-educated citizen men may be complementary to LENCs.[38]

To further test whether these negative effects of SC on citizen employment are operating through complementarities in market production, we explore whether the effects are larger in sectors which have traditionally relied on unskilled immigrant labor. Following the strategy from Table (3), we examine the heterogeneity of the results for citizens in sectors with above and below median share of low-educated non-citizen workers in 2005 in Panels B and C of Table (6), respectively. As expected, the decline in the employment rate of male citizens is larger in industries that employed a higher share of low-educated non-citizen workers before the implementation of SC. For instance, the results in Panel B indicate that SC reduced the employment rate of male citizens in the third quartile of the occupational distribution by about 3.2 percent (0.374/11.67) in sectors with above the median share of low-educated non-citizen workers in 2005, relative to a 1.5 percent decline (0.079/5.20) in below median sectors.

As additional evidence of complementarities between likely undocumented and citizen workers, Figure (5) plots the effect of SC on sector-specific low-educated non-citizen employment shares in the second occupational skill quartile (horizontal axis) against the effect on sector-specific citizens' employment rates in the third occupational skill quartile (vertical axis). To more easily compare the magnitude of the effect across sectors, we scale each β by the sector and demographic group specific mean employment, so the graph plots the percentage effects. This figure indicates a strong relationship between these two groups: in sectors where non-citizens are more affected by SC, citizens also experience larger reduc-

[38] It should be noted that the negative effect of low-educated male citizens is plausible even if low-educated male citizens and non-citizens are substitutes, rather than complements, in production. As articulated in the labor search model developed in Chassamboulli and Peri (2015), if LENCs have a lower reservation wage than low-educated citizens, and citizenship status is not easily observable when firms hire workers, a reduction in the supply of LENCs would increase the expected labor cost for firms, thereby reducing demand for low-educated workers regardless of citizenship status. Alternatively, the effect of removing immigrants on the local labor market could also be driven by changes in demand for local goods. In our context, however, if non-citizen consumption was the main mechanism, we would not expect to find differential effects of enforcement policies across the occupational skill distribution nor would we expect these effects to be concentrated in industries intensive in LENC.

tions in employment. This provides further evidence that the effect on citizens is operating through complementarities in production.

Finally, we explore the extent to which the effect of SC on citizen men varies across CZs based on the CZ's pre-policy share of the likely undocumented population. Effects may be larger in local labor markets that relied more heavily on LENC labor before SC.[39] Panels A and B of Table (7) report results for CZs with below and above median share of LENC workers pre-SC, respectively. The results indicate that the effects of SC on male citizens are generally larger in CZs with above the median share of likely undocumented immigrants. The effect on the total male employment rate (column 1) is twice as big in CZs with above median share LENC (a 0.53 percent decline compared to a 0.27 percent decline relative to the sample means). In above median CZs, we also see evidence of a positive effect among citizen males in the lowest occupation quartile, who may be the closest substitutes to LENC; there is a marginally significant increase of 1.6 percent (0.290/17.80) in their employment rate. Additionally, in the above median CZs, we find a large and significant decline in the third quartile of the skill distribution.

5.3 Robustness Checks

We also evaluate the impact of SC on the *population* of non-citizens and citizens within a CZ. To do this we create measures of population shares, similar to the employment shares used to evaluate the direct effects. Specifically, we sum the number of individuals of each demographic group, divide by the total working-age CZ population in 2005 and multiply by 100. We expect to see negative effects on the population share of LENC because of deportations and voluntary migration decisions. The results in Appendix Tables (A6) indicate that SC led to a decline of about 1-3 percent in the population of likely undocumented immigrants, although the effects are not precisely estimated. Compared to our main results on the

[39]The distribution of the likely undocumented population is calculated by dividing the population low-educated male non-citizens in 2005 by the total population in 2005.

employment share of LENCs (and ignoring the large confidence intervals), the population estimates imply that direct removals of immigrants was an important channel through which the employment of male LENCs declined after SC.[40]

In contrast, there is little evidence, as shown in Appendix Table (A7), that citizens react to the implementation of SC by moving in or out of a CZ. In this case, not only are the effects are statistically insignificant, but the magnitude of the coefficients relative to the population means are also very small. This ensures that our main results on citizens' employment rates are driven by changes in employment and not by changes in population, which is consistent with the evidence of Cadena and Kovak (2016) on internal mobility of natives.

We also check the robustness of the main employment results by excluding CZ's that adopted SC before 2010, since these have been shown to be more highly selected on observable characteristics. For convenience, the results in Panels A and C of Appendix Table (A8) repeat the main estimates for LENCs and for male citizens, respectively. Dropping early adopters of SC from the sample does not change the results substantially; the coefficients in Panel B indicate that SC decreased the employment share of LENC men by about 7 percent (0.159/2.28). The results for male citizens in Panel D are slightly larger and continue to indicate that SC reduced the employment rate of male citizens, particularly those employed on the second and third quartile of the occupational skill distribution.

Next, we test the robustness of the results to dropping CZs that adopted a Sanctuary City policy *before* the implementation of SC. These results are reported in Appendix Table (A9) and indicate that excluding these localities has little impact on the estimated effect of SC on the employment share of LENC or the employment rate of male citizens.

While our main focus is on the employment effects of SC, we also investigate whether

[40]It is very unlikely that SC led to internal migration of LENC across local areas because the entire country was eventually covered by SC. We also directly test for internal migration of LENC across local areas and find no evidence of this.

SC impacted the wages of citizens. The ACS does not include hourly wages, so instead, we calculate hourly wages using each individuals' past year's annual earnings and divide this by hours worked in the previous year. We estimate the effect of SC on log wages employing the same empirical model as in equation (1) and report our results in Table (A10).[41] The results provide little evidence that SC is associated with changes in the wages of male citizens. This is perhaps not surprising given the short-term nature of the analysis and recent evidence on the presence of nominal wage rigidities (Barattieri et al., 2014; Kaur, 2019). The results provide some indication that SC is associated with a decline of about 1.8 percent in the wages of low-educated citizens (Column 3) which is significant at the 10 percent level. This result may seem at first counterintuitive, but additional analysis (not shown) demonstrates these negative wage effects are driven by workers in high-skilled occupations, which as we reported previously, experienced a decline in employment due to SC. Thus, this result is consistent with low-educated male citizens and low educated male non-citizens being complements in production.

Finally, we acknowledge that some undocumented immigrants might choose not to participate in surveys conducted by the U.S. government (Passel and Cohn, 2011; Hoefer et al., 2012; Warren and Warren, 2013; Van Hook et al., 2014; Genoni et al., 2017; Brown et al., 2018).[42] It is important to note, then, that the internal validity of our estimates for low-educated non-citizen workers would be affected if the number or type of undocumented immigrants that respond to the ACS survey is related to the implementation of SC.[43] However, this undercount would not affect our estimates for citizen workers, who we do expect to change survey response behavior in response SC.

[41] Note that because we find changes in the likelihood of employment of citizens, any effects on wages may be influenced by selection into who remains working.

[42] For instance, Genoni et al. (2017) provides evidence that between 2000 and 2005 U.S. surveys (such as the ACS) were more likely to undercount young, single, male, and less educated migrants.

[43] While previous studies estimate an overall 7.5% undercount of undocumented immigrants (Warren, 2014), we are unable to assess how the undercount varies in response to SC.

5.4 Discussion

The results imply that a 1 percent decline in LENCs due to SC is associated with a 0.12 percent decline in the employment rate of male citizens.[44] Although this is the first paper to estimate the differential effect of SC on the labor outcomes of likely undocumented and citizen workers, it is informative to compare our findings to the broader literature on the impact of immigration on native's labor market outcomes.

Comparing our results to the historical literature on policy-driven immigration outflows, our results are consistent with a low degree of substitutability between migrant and native labor as found in Clemens et al. (2018) paper. Moreover, our findings are similar in magnitude to the results reported by Lee et al. (2019); their results suggest that a 1 percent decline in the population of Mexican migrants due to repatriations in 1930 is associated with a 0.2-0.25 percent decline in the probability of natives' to have a job in 1940. Importantly, our findings are also consistent with findings from other studies that look at the impact of more modern policy-driven immigration *inflows*. For example, Beerli and Peri (2015) found that increased high-skilled immigration inflows from opening the border for EU immigrants to work in Switzerland, increased the employment of highly educated native workers and that this was likely due to production complementarities. And, Foged and Peri (2016) found no evidence that policy-driven inflows of low-skilled refugees to local areas negatively affected the employment outcomes of low-skilled natives in Denmark. Likewise, Friedberg (2001) found that lifting emigration restrictions in the Soviet Union had no adverse effects on the employment or wages of native workers in Israel, and, in fact, that these high-skilled immigrants may have actually been complements for natives.

It is also informative to compare our findings to the labor market effects of another enforcement policy: 287(g) agreements. Using a contiguous counties approach, Bohn and

[44]The effect of SC on the employment share of LENCs reported in Table (2) is 6.8 percent (0.197/2.9) and the effect on the employment rate of male citizens reported in column 5 of Table (5) is 0.84 percent, suggesting that the effect on LENCs is about 8 times larger.

Santillano (2017) found that the introduction of 287(g) agreements did not have a significant effect on overall employment, but there was a reduction in some industries that employ many immigrants of similar magnitude to our estimated effects. For instance, they found that 287(g) reduced the employment in administrative services by about 7 percent. Taking a more traditional difference-in-differences approach, Pham and Van (2010) found that 287(g)s reduced overall employment by about 1-2 percent, which is similar to our estimated effects of SC on the overall citizen employment rate. Ours is the first study to estimate the labor market impacts of an immigration enforcement policy by citizenship status and across the skill distribution. As a result, we cannot compare our estimates on these groups with the potential effects of 287(g) on these populations.

6 Conclusion

Secure Communities, one of the largest interior federal immigration enforcement policies over the last decade, resulted in the deportation of almost half a million individuals during 2008-2015. This paper makes an important contribution to the immigration literature by estimating the effects of the SC program on the employment outcomes of both citizens and likely undocumented male non-citizen workers. We find that SC is associated with a significant decrease in the employment share of low-educated non-citizen male workers, who are likely to be undocumented. The decline in the employment share of likely undocumented workers is concentrated in lower-skilled occupations and in sectors that historically rely on low-educated non-citizen labor.

We also use the rollout of the SC program to estimate the effect of an exogenous change in the supply of low-skilled non-citizen workers on the employment rate of citizens. Our findings indicate that SC may have increased the employment rate of low-educated Hispanic citizens, who are the closest substitutes to likely-undocumented migrants. However, we also find that SC led to a decline in the employment rate of both low- and high-educated male citizens. Specifically, a 1 percent decline in the share of LENCs male workers is associated with a 0.12 percent decrease in the employment rate of male citizens. The effects on low and high-educated men are concentrated in middle- to higher-skilled occupations and in sectors that historically rely on low-educated non-citizen labor. The results are consistent with low-skilled migrants acting as complements in production to higher-skilled citizens. Thus, the findings suggest that immigration policies aimed at reducing the number of undocumented immigrants should take into account the potential negative spillover effects on the labor market outcomes for citizens.

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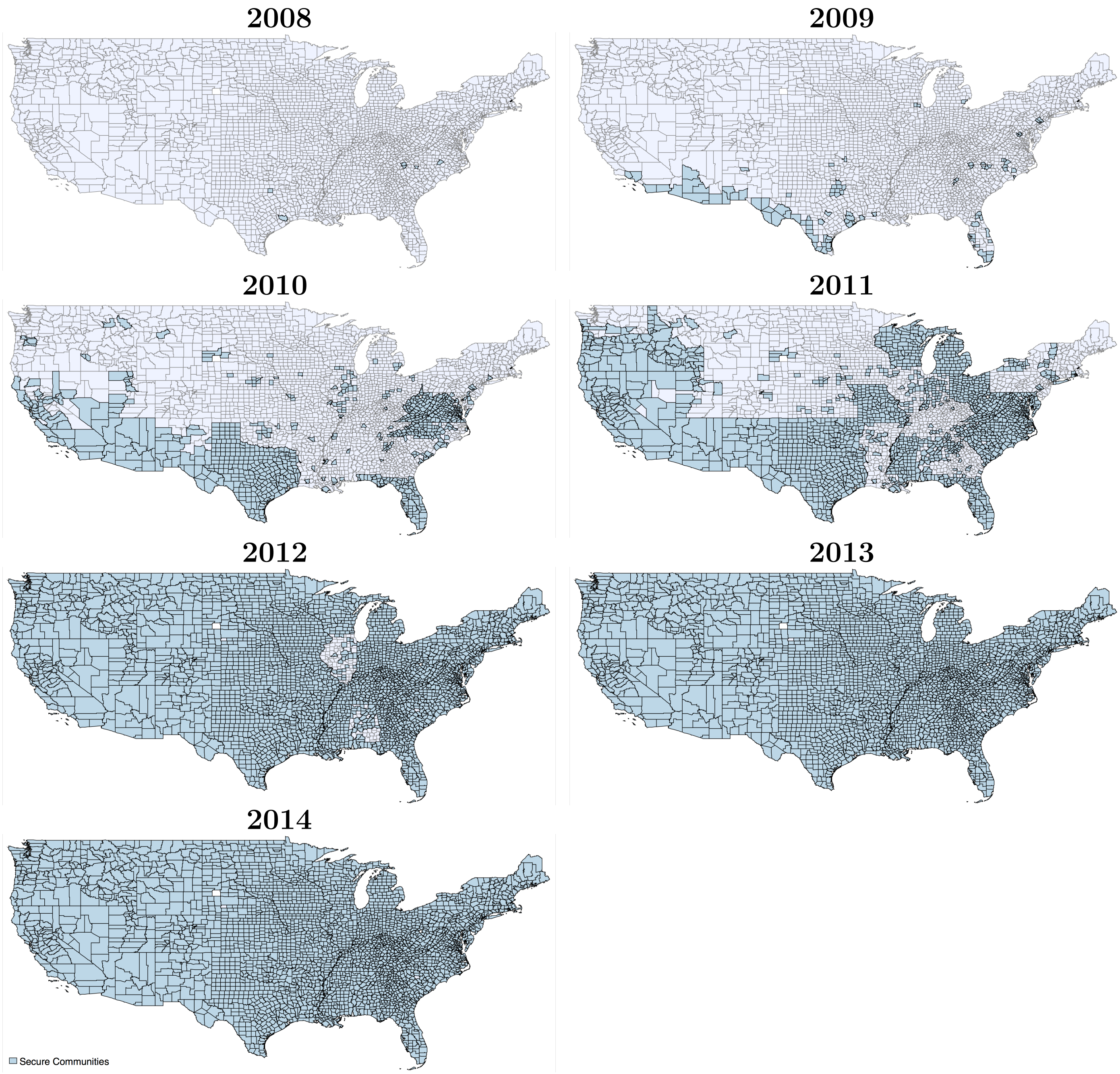
7 Figures

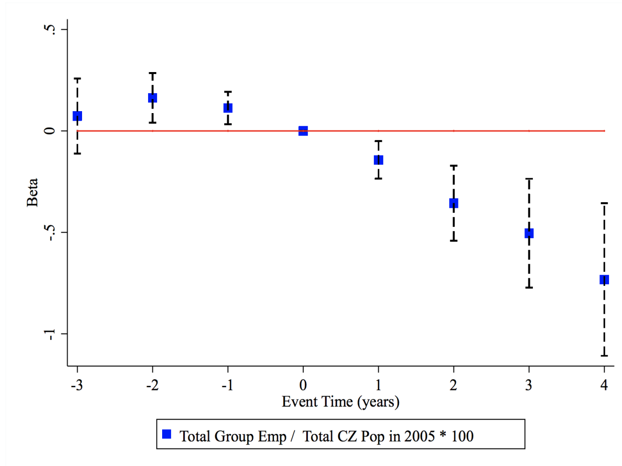
Figure 1: Rollout of Secure Communities by Year



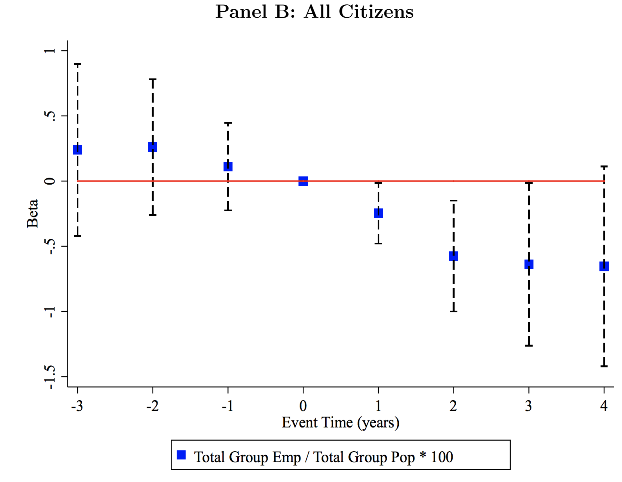
Notes: Counties that had adopted Secure Communities based on December of each year are shaded. See text for sources.

Figure 2: Event Study, Total Effects

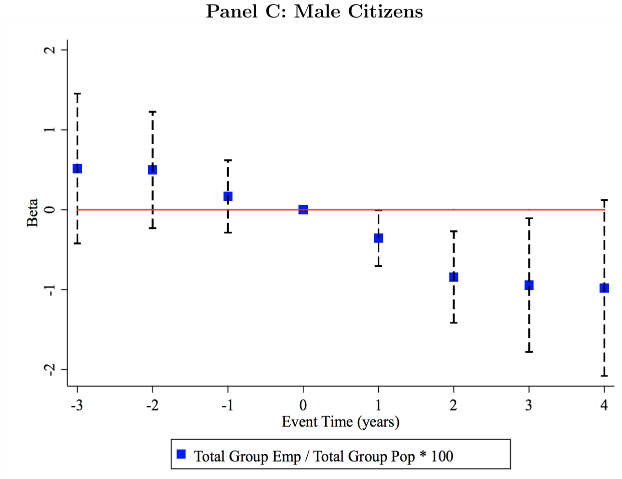
Panel A: Male Low-Educated Non-Citizens



Panel B: All Citizens

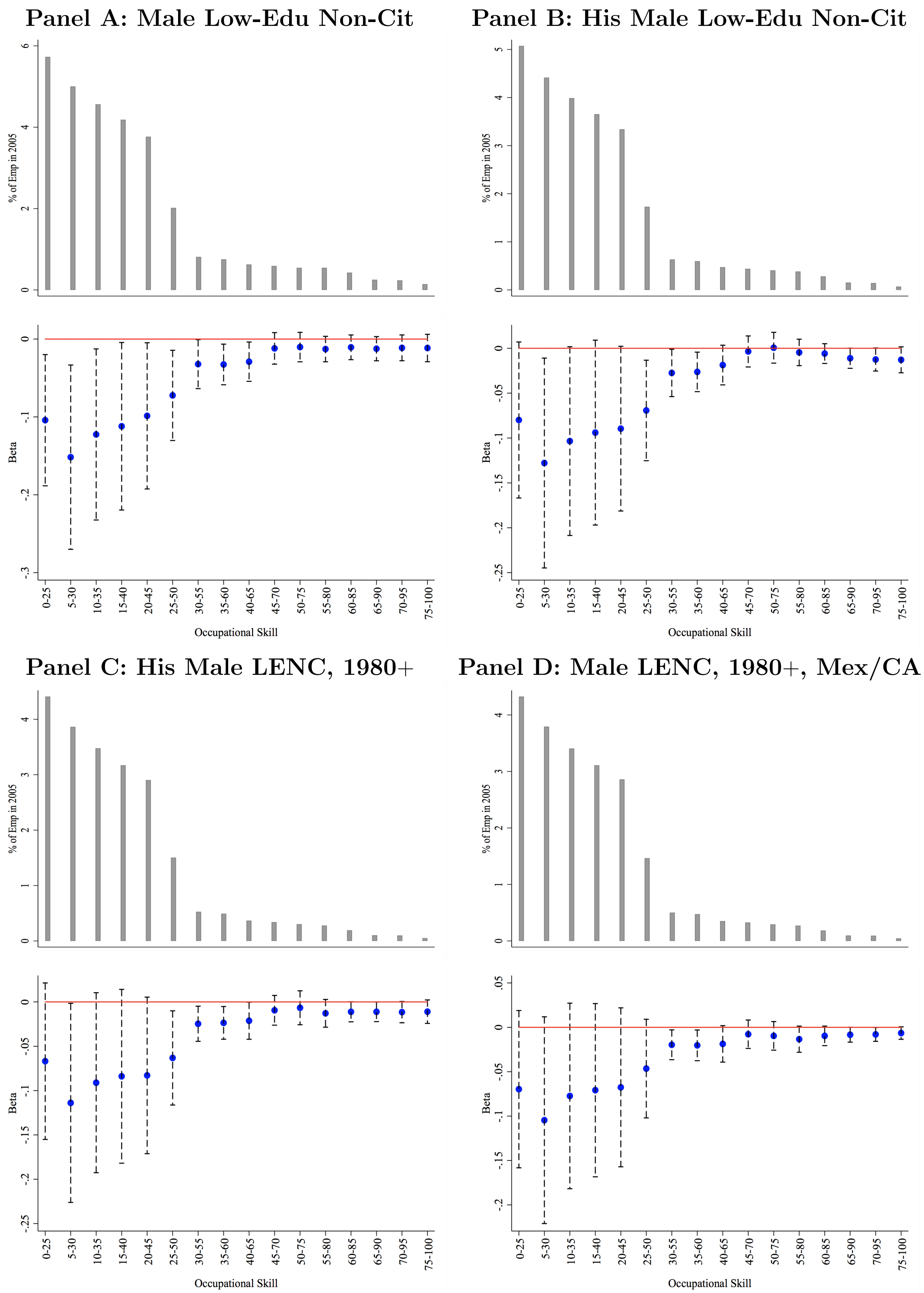


Panel C: Male Citizens



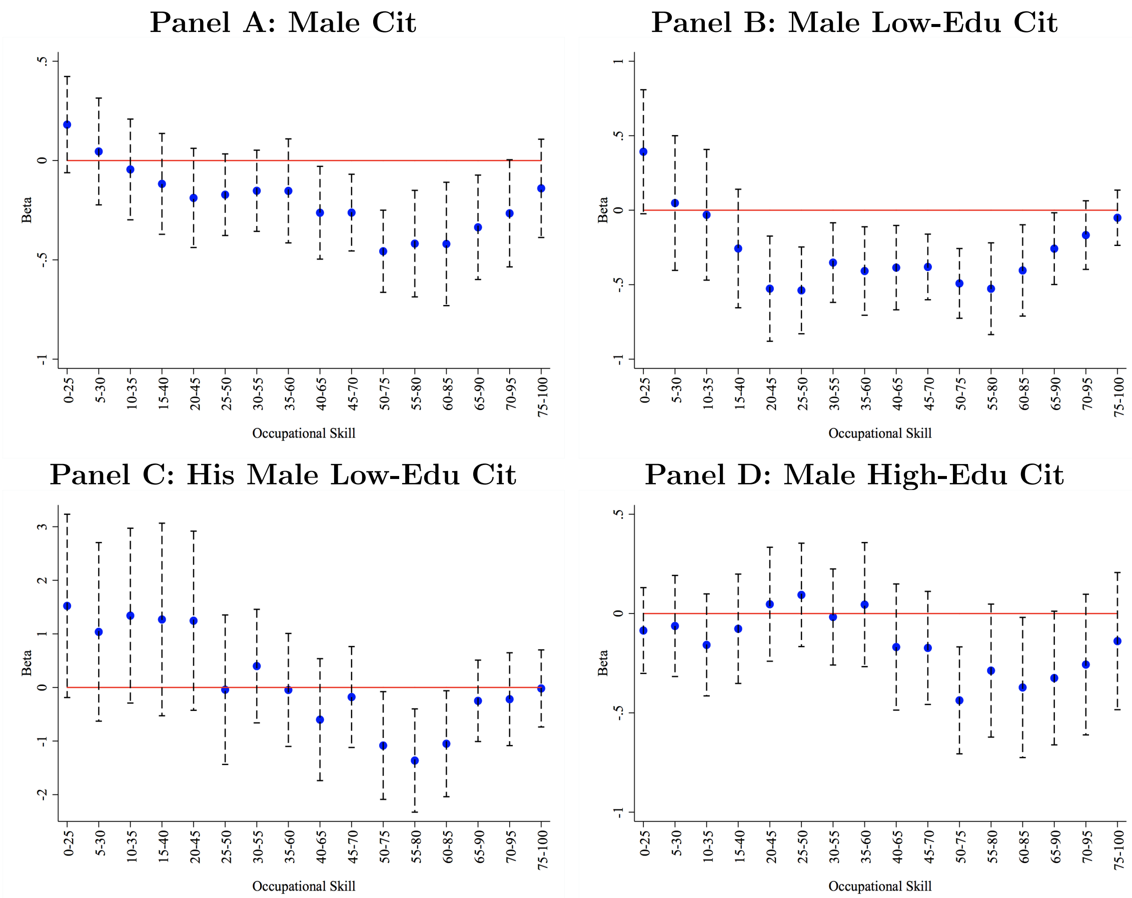
Notes: Data are from the 2005-2014 American Community Survey. The sample in panel (a) is based on all working-aged (20-64) low-educated non-citizen males. The sample in panel (b) is based on all working-aged (20-64) citizens and in panel (c) is male working-aged citizens. Event time is defined relative to the first year 50% of the CZ was covered by SC. The omitted period is the year before 50% of the CZ is covered by SC for the first time. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ.

Figure 3: Rolling Window by Occupational Skill: Direct Effect



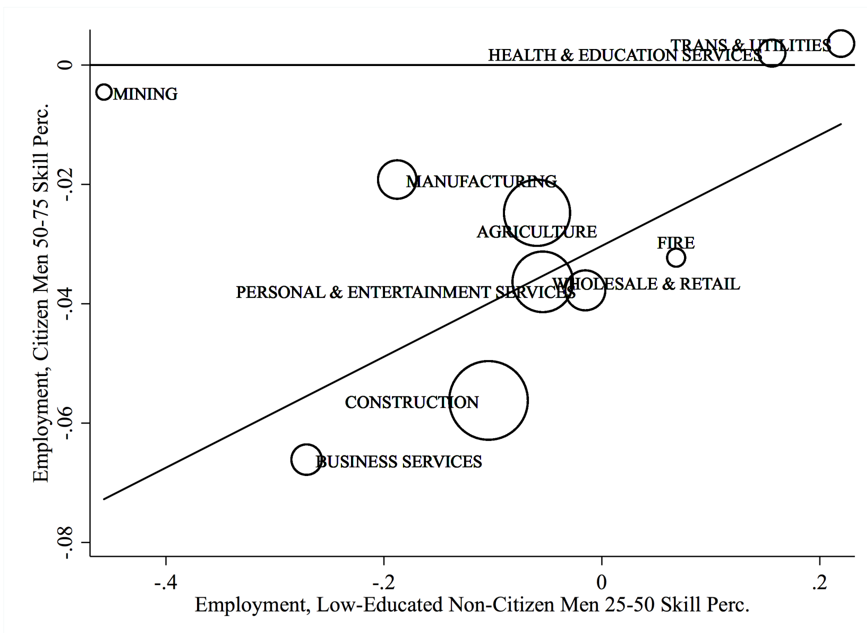
Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) low-educated non-citizen males. The top figure in each panel shows the percent of occupation skill group employment that was made up by each demographic group in 2005. The bottom figure in each panel shows the estimated effect of SC on the demographic-group-specific employment divided by CZ base year population. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000 and standard errors are clustered by CZ. The coefficient is represented by the blue dot, and the 95% confidence intervals are shown in the dashed lines.

Figure 4: Rolling Window by Occupational Skill: Indirect Effect



Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) citizen males. Each panel shows the estimated effect of SC on the demographic-group-specific employment rate. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000 and standard errors are clustered by CZ. The coefficient is represented by the blue dot, and the 95% confidence intervals are shown in the dashed lines.

Figure 5: Heterogeneous Effects by Sector: Effect on Citizen Men in 50-75 Skill Percentiles vs. Effect on Low-Educated Non-Citizen Men in 25-50 Skill Percentiles



Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) men. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ male employment by sector in 2005. Standard errors are clustered by CZ. The size of each circle indicates the number of low-educated non-citizen men in each sector in 2005.

8 Tables

Table 1: Summary Statistics

Non-Citizen Male Employment as a Share of Total Population in 2005 * 100	
Low-Educated	2.90
Hispanic Low-Educated	2.39
Hispanic Low-Educated, enter U.S. after 1980	2.12
Low-Educated from Mexico or other Central American country, enter U.S. after 1980	1.94
Citizen Employment Rates * 100	
All	67.48
All Men	70.64
Low-Educated Men	65.08
Hispanic Low-Educated Men	66.84
High-Educated Men	75.54
Policy Variables	
SC	0.39
Jail 287(g)	0.10
Task 287(g)	0.02

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) individuals. We weight the summary statistics by the CZ population in 2000.

Table 2: Direct Effect on Low-Educated Non-Citizen Men

	Dep. Var: Total Group Emp / Total CZ Pop in 2005 * 100			
	All	His	His, 1980+	Mex/CA, 1980+
A: CZ FE, Year FE only				
β: SC	-0.300***	-0.276***	-0.223***	-0.198**
	(0.100)	(0.099)	(0.083)	(0.083)
CZ-Year Trends				
Bartiks				
287(g)				
Housing Boom * Trend				
Y mean	2.90	2.39	2.12	1.94
Observations	7370	7370	7370	7370
B: Add Housing Boom Trends				
β: SC	-0.264***	-0.251***	-0.211***	-0.183**
	(0.078)	(0.081)	(0.074)	(0.074)
CZ-Year Trends				
Bartiks				
287(g)				
Housing Boom * Trend	X	X	X	X
Y mean	2.91	2.39	2.12	1.94
Observations	6580	6580	6580	6580
C: Add CZ Trends				
β: SC	-0.260***	-0.226***	-0.197***	-0.170**
	(0.067)	(0.070)	(0.066)	(0.068)
CZ-Year Trends	X	X	X	X
Bartiks				
287(g)				
Housing Boom * Trend				
Y mean	2.90	2.39	2.12	1.94
Observations	7370	7370	7370	7370
D: Add Bartiks				
β: SC	-0.210***	-0.173***	-0.158**	-0.140**
	(0.062)	(0.063)	(0.064)	(0.066)
CZ-Year Trends	X	X	X	X
Bartiks	X	X	X	X
287(g)				
Housing Boom * Trend				
Y mean	2.90	2.39	2.12	1.94
Observations	7370	7370	7370	7370
E: Add 287(g)s				
β: SC	-0.197***	-0.161**	-0.148**	-0.133**
	(0.066)	(0.065)	(0.063)	(0.067)
CZ-Year Trends	X	X	X	X
Bartiks	X	X	X	X
287(g)	X	X	X	X
Housing Boom * Trend				
Y mean	2.90	2.39	2.12	1.94
Observations	7370	7370	7370	7370

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) low-educated non-citizen males. All models include CZ fixed effects, and year fixed effects. Panel B adds in the percentage change in CZ-level housing prices from 2000-2006 interacted with a linear trend. Note that some CZs have missing housing price information so the sample size is slightly smaller in Panel B. Panel C instead includes CZ linear trends. Panel D adds to the model in Panel C bartik-style controls for labor demand. Panel E adds to the model in Panel D controls for CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 3: Direct Effect on Low-Educated Non-Citizen Men, Splitting by Occupational Skill and Sector

	Dep. Var: Total Group Emp / Total CZ Pop in 2005 * 100				
	All	Occ Skill <25	25< Occ Skill <50	50< Occ Skill <75	75< Occ Skill
A: All Low-Edu Non-Cit Men					
β: SC	-0.197***	-0.104**	-0.073**	-0.010	-0.010
	(0.066)	(0.043)	(0.030)	(0.010)	(0.009)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	2.90	1.73	0.87	0.24	0.06
Observations	7370	7370	7370	7370	7370
B: LENC share >4%					
β: SC	-0.194***	-0.094**	-0.082***	-0.005	-0.013**
	(0.063)	(0.041)	(0.030)	(0.010)	(0.006)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	2.58	1.56	0.79	0.19	0.03
Observations	7370	7370	7370	7370	7370
C: LENC share <4%					
β: SC	-0.002	-0.010	0.010*	-0.005	0.003
	(0.015)	(0.012)	(0.006)	(0.004)	(0.004)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	0.32	0.17	0.08	0.05	0.03
Observations	7370	7370	7370	7370	7370

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) low-educated non-citizen males. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 4: Indirect Effect on All Citizens

	Dep. Var: Total Group Emp / Total Group Pop * 100				
β: SC	-0.672***	-0.655***	-0.487***	-0.454***	-0.480***
	(0.183)	(0.188)	(0.162)	(0.164)	(0.163)
CZ-Year Trends			X	X	X
Bartiks				X	X
287(g)					X
Housing Boom * Trend		X			
Y mean	67.48	67.49	67.48	67.48	67.48
Observations	7370	6580	7370	7370	7370

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) citizens. All models include CZ fixed effects, and year fixed effects. Column (2) adds in the percentage change in CZ-level housing prices from 2000-2006 interacted with a linear trend. Note that some CZs have missing housing price information so the sample size is slightly smaller in column (2). Column (3) instead includes CZ linear trends. Column (4) adds to the model in column (3) bartik-style controls for labor demand. Column (5) adds to the model in column (4) controls for CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 5: Indirect Effect on Citizens by Demographics

	Dep. Var: Total Group Emp / Total Group Pop * 100				
	All	Men	Low-Edu Men	Low-Edu His Men	High-Edu Men
β: SC	-0.480***	-0.590***	-0.740***	0.308	-0.521**
	(0.163)	(0.226)	(0.268)	(0.749)	(0.225)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	67.48	70.64	65.08	66.84	75.54
Observations	7370	7370	7370	7348	7370

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) citizens. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 6: Indirect Effect on Citizen Men, Splitting by Occupational Skill and Sector

	Dep. Var: Total Group Emp / Total Group Pop * 100				
	All	Occ Skill <25	25< Occ Skill <50	50< Occ Skill <75	75< Occ Skill
A: All Cit Men					
β: SC	-0.590***	0.181	-0.194*	-0.454***	-0.122
	(0.226)	(0.124)	(0.105)	(0.106)	(0.122)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	70.64	19.48	15.26	17.13	18.76
Observations	7370	7370	7370	7370	7370
B: LENC share >4%					
β: SC	-0.383*	0.122	-0.151	-0.374***	0.021
	(0.210)	(0.123)	(0.096)	(0.087)	(0.072)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	44.35	15.01	10.90	11.67	6.77
Observations	7370	7370	7370	7370	7370
C: LENC share <4%					
β: SC	-0.185	0.052	-0.025	-0.079	-0.133
	(0.133)	(0.061)	(0.055)	(0.059)	(0.096)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	25.81	4.45	4.29	5.20	11.87
Observations	7370	7370	7370	7370	7370

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) citizens. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table 7: Indirect Effect on Citizen Men Erate, Splitting by Occupational Skill and CZ LENC Population Share

	Dep. Var: Total Employment in Group / Total Population in Group				
	All	Occ Skill <25	25< Occ Skill <50	50< Occ Skill <75	75< Occ Skill
A: Pop Share LENC Males < Median					
β: SC	-0.189	0.099	-0.179	-0.316	0.207
	(0.356)	(0.247)	(0.253)	(0.234)	(0.237)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	69.65	24.47	15.33	15.18	14.66
Observations	3680	3680	3680	3680	3680
B: Pop Share LENC Males > Median					
β: SC	-0.373	0.290*	-0.163	-0.421***	-0.078
	(0.267)	(0.151)	(0.127)	(0.126)	(0.150)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	70.97	17.80	15.24	17.79	20.14
Observations	3690	3690	3690	3690	3690

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) citizens. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Appendix For Online Publication

A Data Description and Additional Results

A.1 CZ-Year Control Variables

We construct four Bartik-style measures of labor demand to use as controls that correspond to the following four demographic groups: 1) all working-age adults, 2) foreign-born working-age adults, 3) working-age adults with more than a high-school diploma, and 4) working-age adults with a high-school diploma or less. For each group, we calculate the CZ-level employment by industry, as a fraction of total CZ employment in 2005. We then apply to these industry shares the changes in national employment for the full national sample of working age adults for each industry over time, to obtain a measure of predicted changes in local labor demand. The housing price information used in the trend control comes from the Federal Housing Finance Agency and is available at the county by year level, which we aggregate up to the CZ level using a similar weighting process as described in the main text for the SC variable.

We also include controls for the presence of 287(g) Agreements. 287(g) agreements were similar to SC, but 287(g)s were optional agreements law enforcement agencies could choose to enter into with the federal government. Start and end dates for all 287(g) agreements came from reports published by ICE, the Department of Homeland Security, the Migration Policy Institute, as well as Kostandini et al. (2013), and various news articles. There were three types of 287(g) agreements and this information also allowed us to determine which type of agreement was in place. The "Task Force" model permitted trained law enforcement officials to screen individuals regarding their immigration status during policing operations, and arrest individuals due to suspected immigration violations. The "Jail" model allowed screening of immigration status for individuals upon being booked in state prisons or local jails and was more similar to SC. A third "Hybrid" model includes both the Task Force and

Jail models.[45]

A.2 TRAC Data Description

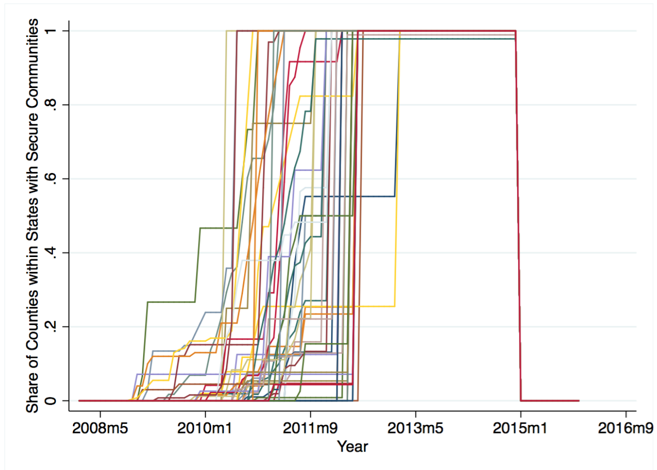
Data on deportations under SC comes from the Transactional Records Access Clearinghouse at Syracuse University. TRAC obtained these data from ICE through a series of Freedom of Information Act requests. The data contain individual-level records of each deportation under SC, beginning in November 2008 and continuing through the end of SC in 2014.[46] The county given in this file is the county of apprehension, the date is the date of removal. Because deportations do not happen immediately upon apprehension, there is a lag between the initial apprehension and the date recorded in our data. For each individual, we have information on the deportation proceedings as well as various demographics, including age, gender, and country of citizenship. The data also contain information on the criminal background of the deportee, including their most serious criminal conviction (MSCC).

TRAC provides a very similar file of records for ICE detainers, which we use to examine the effects of SC on detention intensity. However, it is important to note that we cannot separately identify which detentions were done under SC.

[45] Background information on 287(g)s is obtained from Capps et al. (2011).

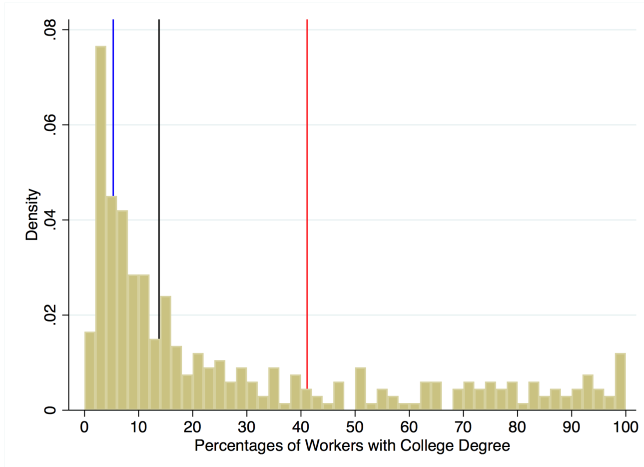
[46] The data also contain information about deportations under PEP, which replaced SC in 2014, as well as under the restoration of SC after January 2017, but we do not use this information.

Figure A1: Rollout of Secure Communities across Counties within States



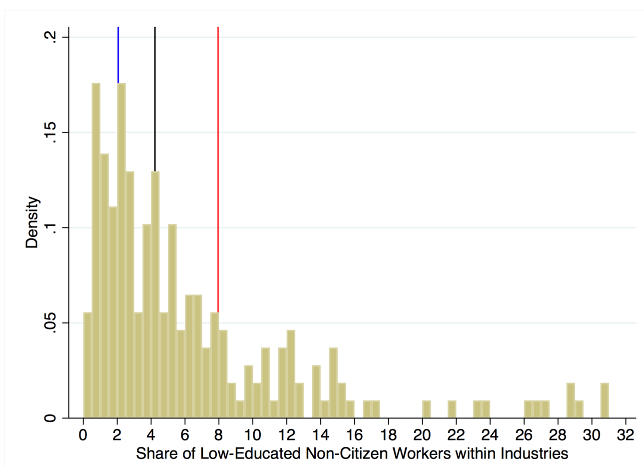
Notes: The above figure plots the phase in of Secure Communities within States. In January of 2015 SC was replaced by the Priority Enforcement Program.

Figure A2: Distribution of Skill Intensity Across Occupations



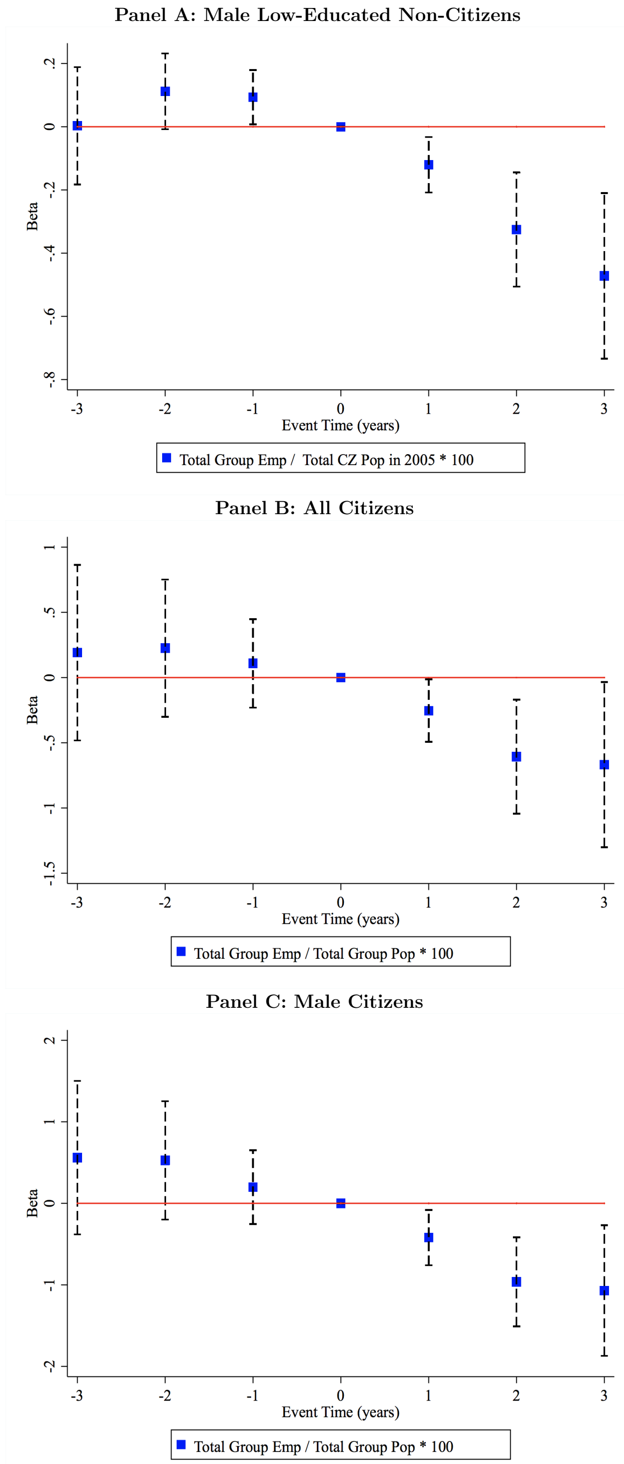
Notes: The above figure plots density of skill intensity across occupations as measured by the share of workers within an occupation with a college degree. This is estimated using the 2005 American Community Survey (ACS). The black bar indicates the occupation with the median skill (12.7) the blue and red bars depict the 25th and 75th percentile skill occupations respectively (4.6 and 42.2).

Figure A3: Distribution of Low-Educated Non-Citizen Across Industries



Notes: The above figure plots density of low-educated non-citizen labor intensity across industries as measured by the 2005 American Community Survey (ACS). The black bar indicates the industry with the median low-educated non-citizen labor intensity (4.16) the blue and red bars depict the 25th and 75th percentile industries, respectively (1.86 and 7.87).

Figure A4: Event Study, Total Effects, Only CZs that adopted before 2013



Notes: Data are from the 2005-2014 American Community Survey. We include only CZs that implemented SC before 2013 in order to have a balanced sample. The sample in panel (a) is based on all working-aged (20-64) low-educated non-citizen males. The sample in panel (b) is based on all working-aged (20-64) citizens and in panel (c) is male working-aged citizens. Event time is defined relative to the first year 50% of the CZ was covered by SC. The omitted period is the year before 50% of the CZ is covered by SC for the first time. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ.

Table A1: Correlation of 2005-2007 Changes in CZ Characteristics and SC Adoption Year

	Mean of Characteristic	Standard Deviation of Characteristic	Regression Estimate 1	Regression Estimate 2
Change % Non-Citizen	0.06	0.21	0.358	0.395
			(0.284)	(0.271)
Change % Male Non-Citizen	0.09	0.32	0.241	0.248
			(0.225)	(0.215)
Change % Low-Edu Male Non-Cit	0.13	0.57	0.098	0.088
			(0.122)	(0.116)
Change % His Low-Edu Male Non-Cit	0.27	1.90	0.021	0.024
			(0.027)	(0.026)
Change Task 287(g)	0.01	0.09	0.658	0.515
			(0.682)	(0.653)
Change Jail 287(g)	0.04	0.14	-2.130***	-2.419***
			(0.516)	(0.489)
Change Citizen Bartik	4733375	7476281	-0.000	-0.000
			(0.000)	(0.000)
Change Non-Cit Bartik	4385099	7050454	0.000	0.000**
			(0.000)	(0.000)
Change Low-Edu Bartik	4423635	6981763	0.000	-0.000
			(0.000)	(0.000)
Change High-Edu Bartik	4938222	7777598	0.000	0.000
			(0.000)	(0.000)
Housing Boom: % Change 2000-2006	0.60	0.41	-0.512***	
			(0.128)	
Mean Y			2010.10	2010.10
R-Squared			0.15	0.13
N			658	737

Notes: Data are from the 2005-2007 American Community Survey and 2000-2006 Federal Housing Finance Agency. The first regression estimate includes the change in housing prices. The second regression estimate drops the change in housing prices from the model since housing price information is missing for some CZs. The regressions are weighted by the CZ population in 2000. * p<0.10, ** p<0.05, *** p<0.01

Table A2: Characteristics of Deportees under SC, 2008-2014

Characteristic	Share of Deportees (percent)
Most Serious Criminal Conviction	
None	20.63
All Non-Violent	60.83
Traffic	7.01
Immigration	5.46
DUI	10.94
Marijuana	2.38
Gender	
Male	95.61
Country of Citizenship	
Latin America	92.22

Notes: Data on deportees comes from individual listings of all deportations under SC from TRAC records described in Appendix A. The most serious criminal conviction may be, but does not have to be, the crime for which the deportee was initially apprehended.

Table A3: Effect of SC on Detentions as a share of Total Population in 2005

	Dep. Var: Total Detainers/Population 2005'				
	(1)	(2)	(3)	(4)	(5)
β: SC	0.135***	0.133***	0.119***	0.102***	0.091***
	(0.021)	(0.021)	(0.021)	(0.019)	(0.016)
CZ-Year Trends			X	X	X
Bartiks				X	X
287(g)					X
Housing Prices		X			
Y mean	0.10	0.10	0.10	0.10	0.10
Observations	7370	6580	7370	7370	7370

Notes: Data are from the 2005-2014 Transactional Records Access Clearinghouse (TRAC). All models include CZ fixed effects, and year fixed effects. Column (2) adds in the percentage change in CZ-level housing prices from 2000-2006 interacted with a linear trend. Note that some CZs have missing housing price information so the sample size is slightly smaller in column (2). Column (3) instead includes CZ linear trends. Column (4) adds to the model in column (3) bartik-style controls for labor demand. Column (5) adds to the model in column (4) controls for CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table A4: Top 10 Most Common Occupations by Skill Quartile for Low-Educated Non-Citizen Men

Occupations in 0-25th Perc.	
Construction Laborers	14.47
Chefs and Cooks	9.20
Agricultural workers, nec	7.16
Driver/Sales Workers and Truck Drivers	6.75
Janitors and Building Cleaners	6.41
Laborers and Freight, Stock, and Materi	4.59
Other production workers including semi	3.58
Drywall Installers, Ceiling Tile Instal	2.94
Automotive Service Technicians and Mech	2.76
Assemblers and Fabricators, nec	2.71
Occupations in 25-50th Perc.	
Grounds Maintenance Workers	20.36
Carpenters	18.02
Painters, Construction and Maintenance	10.97
First-Line Supervisors of Construction	4.01
Stock Clerks and Order Fillers	3.52
Waiters and Waitresses	3.37
Cashiers	3.15
Food Preparation Workers	3.11
Shipping, Receiving, and Traffic Clerks	2.58
Electricians	2.29
Occupations in 50-75th Perc.	
First-Line Supervisors of Sales Workers	18.00
Retail Salespersons	13.59
Food Service and Lodging Managers	8.25
Constructions Managers	5.86
Sales Representatives, Wholesale and Management	5.72
Customer Service Representatives	4.32
First-Line Supervisors of Landscaping,	3.66
Farmers, Ranchers, and Other Agriculture	3.14
First-Line Supervisors of Office and Ad	2.52
Property, Real Estate, and Community As	2.06
Occupations in 75-100th Perc.	
Managers, nec (including Postmasters)	22.16
Designers	8.10
Chief executives and legislators/public	7.18
General and Operations Managers	6.07
Human Resources, Training, and Labor Relations	3.37
Other Teachers and Instructors	3.24
Human Resources Managers	3.16
Managers in Marketing and Advertising	2.89
Computer Scientists and Systems Analyst	2.79
Securities, Commodities, and Financial	2.69

Notes: Data are from the 2005 American Community Survey. The results are weighted using individual survey weights.

Table A5: Top 10 Most Common Occupations by Skill Quartile and Education for Citizen Men

All Cit Men		Low-Edu Cit Men		High-Edu Cit Men	
Occupations in 0-25th Perc.					
Driver/Sales Workers and Truck Drivers	15.66	Driver/Sales Workers and Truck Drivers	16.13	Driver/Sales Workers and Truck Drivers	14.21
Laborers and Freight, Stock, and Materi	7.1	Laborers and Freight, Stock, and Materi	7.06	Laborers and Freight, Stock, and Materi	7.25
Janitors and Building Cleaners	6.59	Janitors and Building Cleaners	6.63	Janitors and Building Cleaners	6.43
Construction Laborers	6.01	Construction Laborers	6.1	Chefs and Cooks	5.83
Automotive Service Technicians and Mech	4.41	Other production workers including semi	4.13	Construction Laborers	5.74
Chefs and Cooks	4.33	Automotive Service Technicians and Mech	4.01	Automotive Service Technicians and Mech	5.65
Other production workers including semi	4.1	Chefs and Cooks	3.85	Other production workers including semi	3.99
Assemblers and Fabricators, nec	3.4	Assemblers and Fabricators, nec	3.33	Assemblers and Fabricators, nec	3.62
Pipelayers, Plumbers, Pipefitters, and	2.98	Welding, Soldering, and Brazing Workers	2.97	Industrial and Refractory Machinery Mec	3.24
Welding, Soldering, and Brazing Workers	2.83	Pipelayers, Plumbers, Pipefitters, and	2.95	Pipelayers, Plumbers, Pipefitters, and	3.07
Occupations in 25-50th Perc.					
Carpenters	9.39	Carpenters	11.34	First-Line Supervisors of Construction	6.96
First-Line Supervisors of Construction	7.91	First-Line Supervisors of Construction	8.53	Carpenters	6.44
First-Line Supervisors of Production an	6.06	First-Line Supervisors of Production an	6.09	First-Line Supervisors of Production an	6.01
Electricians	5.13	Grounds Maintenance Workers	5.41	Electricians	5.21
Stock Clerks and Order Fillers	4.84	Stock Clerks and Order Fillers	5.39	Security Guards and Gaming Surveillance	4.2
Grounds Maintenance Workers	4.51	Electricians	5.09	Stock Clerks and Order Fillers	4
Security Guards and Gaming Surveillance	3.88	Security Guards and Gaming Surveillance	3.68	Cashiers	3.67
Cashiers	3.04	Painters, Construction and Maintenance	3.64	Waiters and Waitresses	3.66
Inspectors, Testers, Sorters, Samplers,	2.94	Maintenance and Repair Workers, General	3.29	Inspectors, Testers, Sorters, Samplers,	3.31
Maintenance and Repair Workers, General	2.92	Shipping, Receiving, and Traffic Clerks	3.2	Grounds Maintenance Workers	3.15
Occupations in 50-75th Perc.					
First-Line Supervisors of Sales Workers	17.7	First-Line Supervisors of Sales Workers	20.14	First-Line Supervisors of Sales Workers	16.32
Retail Salespersons	9.57	Retail Salespersons	11.17	Sales Representatives, Wholesale and Ma	8.88
Sales Representatives, Wholesale and Ma	7.79	Constructions Managers	6.77	Retail Salespersons	8.67
Constructions Managers	5.32	Sales Representatives, Wholesale and Ma	5.86	Constructions Managers	4.5
Customer Service Representatives	3.8	Farmers, Ranchers, and Other Agricultur	5.36	Customer Service Representatives	3.68
Farmers, Ranchers, and Other Agricultur	3.42	Customer Service Representatives	4.01	Real Estate Brokers and Sales Agents	3.27
Food Service and Lodging Managers	3.36	Food Service and Lodging Managers	3.63	Food Service and Lodging Managers	3.2
First-Line Supervisors of Office and Ad	3.13	First-Line Supervisors of Office and Ad	3.18	Sales Representatives, Services, All Ot	3.15
Sales Representatives, Services, All Ot	2.73	Supervisors of Transportation and Mater	2.09	First-Line Supervisors of Office and Ad	3.11
Real Estate Brokers and Sales Agents	2.61	Sales Representatives, Services, All Ot	1.97	Insurance Sales Agents	2.53
Occupations in 75-100th Perc.					
Managers, nec (including Postmasters)	9.85	Managers, nec (including Postmasters)	22.28	Managers, nec (including Postmasters)	8.36
Chief executives and legislators/public	5.12	General and Operations Managers	8.9	Chief executives and legislators/public	4.74
Accountants and Auditors	4.17	Chief executives and legislators/public	8.28	Elementary and Middle School Teachers	4.61
Elementary and Middle School Teachers	4.16	Computer Scientists and Systems Analyst	4.38	Accountants and Auditors	4.54
Computer Scientists and Systems Analyst	3.8	Managers in Marketing, Advertising, and	3.59	Computer Scientists and Systems Analyst	3.73
General and Operations Managers	3.59	Designers	3.52	Lawyers, and judges, magistrates, and o	3.73
Lawyers, and judges, magistrates, and o	3.33	Other Teachers and Instructors	3.3	Postsecondary Teachers	3.58
Postsecondary Teachers	3.23	Financial Managers	2.62	Physicians and Surgeons	3.36
Physicians and Surgeons	3	Human Resources, Training, and Labor Re	2.56	Software Developers, Applications and S	2.98
Managers in Marketing, Advertising, and	2.9	Computer Programmers	2.28	General and Operations Managers	2.95

Notes: Data are from the 2005 American Community Survey. The results are weighted using individual survey weights.

Table A6: Direct Effect on the Population of Non-Citizens as a share of Total Population in 2005

	Dep. Var: Total Group Pop * 100 / Total CZ Pop in 2005 * 100			
	All	His	His, 1980+	Mex/CA, 1980+
β: SC	-0.046	-0.033	-0.045	-0.065
	(0.058)	(0.065)	(0.066)	(0.058)
CZ-Year Trends	X	X	X	X
Bartiks	X	X	X	X
287(g)	X	X	X	X
Housing Boom * Trend				
Y mean	3.59	2.86	2.50	2.27
Observations	7370	7370	7370	7370

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all low-educated non-citizen working-aged (20-64) males. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table A7: Indirect Effect on the Population of Citizens as a share of Total Population in 2005

	Dep. Var: Total Group Pop * 100 / Total CZ Pop in 2005 * 100				
	All	Men	Low-Edu Men	Low-Edu His Men	High-Edu Men
β: SC	-0.031	-0.114	-0.041	-0.006	-0.073
	(0.247)	(0.134)	(0.097)	(0.042)	(0.115)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	95.86	47.29	22.44	3.01	24.85
Observations	7370	7370	7370	7370	7370

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) citizens. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table A8: Examine Robustness to Dropping CZs that Adopted SC Before 2010

	Dep. Var: Total Group Emp / Total CZ Pop in 2005 * 100				
	All	Occ Skill <25	25< Occ Skill <50	50< Occ Skill <75	75< Occ Skill
A: All Low-Edu Non-Cit Men					
β: SC	-0.197***	-0.104**	-0.073**	-0.010	-0.010
	(0.066)	(0.043)	(0.030)	(0.010)	(0.009)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	2.90	1.73	0.87	0.24	0.06
Observations	7370	7370	7370	7370	7370
B: All Low-Edu Non-Cit Men, Drop Early Adopters					
β: SC	-0.159**	-0.063	-0.088***	-0.007	-0.001
	(0.065)	(0.047)	(0.027)	(0.011)	(0.007)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	2.28	1.39	0.66	0.19	0.05
Observations	6930	6930	6930	6930	6930
	Dep. Var: Total Group Emp / Total Group Pop * 100				
	All	Occ Skill <25	25< Occ Skill <50	50< Occ Skill <75	75< Occ Skill
C: All Cit Men					
β: SC	-0.590***	0.181	-0.194*	-0.454***	-0.122
	(0.226)	(0.124)	(0.105)	(0.106)	(0.122)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	70.64	19.48	15.26	17.13	18.76
Observations	7370	7370	7370	7370	7370
D: All Cit Men, Drop Early Adopters					
β: SC	-0.794***	-0.005	-0.289**	-0.490***	-0.010
	(0.247)	(0.121)	(0.139)	(0.135)	(0.183)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	70.52	20.31	15.24	16.70	18.27
Observations	6930	6930	6930	6930	6930

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) males. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table A9: Examine Robustness to Dropping CZs that Adopted a Sanctuary City Policy Before SC was Implemented

	Dep. Var: Total Group Emp / Total CZ Pop in 2005 * 100				
	All	Occ Skill <25	25< Occ Skill <50	50< Occ Skill <75	75< Occ Skill
A: All Low-Edu Non-Cit Men					
β: SC	-0.197***	-0.104**	-0.073**	-0.010	-0.010
	(0.066)	(0.043)	(0.030)	(0.010)	(0.009)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	2.90	1.73	0.87	0.24	0.06
Observations	7370	7370	7370	7370	7370
B: All Low-Edu Non-Cit Men, Drop Sanctuary Cities					
β: SC	-0.176***	-0.099**	-0.053*	-0.010	-0.014
	(0.068)	(0.044)	(0.030)	(0.010)	(0.009)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	2.90	1.73	0.87	0.24	0.06
Observations	7270	7270	7270	7270	7270
	Dep. Var: Total Group Emp / Total Group Pop * 100				
	All	Occ Skill <25	25< Occ Skill <50	50< Occ Skill <75	75< Occ Skill
C: All Cit Men					
β: SC	-0.590***	0.181	-0.194*	-0.454***	-0.122
	(0.226)	(0.124)	(0.105)	(0.106)	(0.122)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	70.64	19.48	15.26	17.13	18.76
Observations	7370	7370	7370	7370	7370
D: All Cit Men, Drop Sanctuary Cities					
β: SC	-0.593***	0.210	-0.159	-0.503***	-0.142
	(0.220)	(0.129)	(0.113)	(0.114)	(0.116)
CZ-Year Trends	X	X	X	X	X
Bartiks	X	X	X	X	X
287(g)	X	X	X	X	X
Y mean	70.76	19.47	15.29	17.18	18.82
Observations	7270	7270	7270	7270	7270

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-aged (20-64) males. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

Table A10: Indirect Effect on Male Citizens Wages

	Dep. Var: Log Wages				
	All	Men	Low-Edu Men	Low-Edu His Men	High-Edu Men
β^1: SC	-0.002	-0.001	-0.018*	-0.025	0.003
	(0.009)	(0.009)	(0.009)	(0.025)	(0.009)
PUMA-Year Trends	X	X	X	X	X
287g	X	X	X	X	X
Labor Demand	X	X	X	X	X
Y mean	3.19	3.32	3.03	2.88	3.47
Observations	7370	7370	7370	7286	7370

Notes: Data are from the 2005-2014 American Community Survey. Average wages are calculated as annual income divided by average hours worked. The sample includes all working-aged (20-64) males. The models include CZ fixed effects, year fixed effects, CZ linear trends, bartik-style controls, CZ-level 287(g) program presence. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parenthesis. * p<0.10, ** p<0.05, *** p<0.01