The Labor Market Effects of Immigration Enforcement*

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April 19, 2022

Abstract

We examine the labor market effects of Secure Communities (SC)–a police-based immigration enforcement policy implemented between 2008-2013. Using variation in implementation across local areas and over time, we find that SC decreased the employment of likely undocumented immigrants. These effects are driven not only by deportations, but also by adjustments among immigrants who remain in the U.S. Importantly, SC also decreased the employment and hourly wages of U.S.-born individuals. We provide support for two mechanisms that could explain this decline in labor demand: an increase in labor costs that decreases job creation, and a reduction in local consumption.

JEL: F22, J2, K37

*We are grateful to Catalina Amuedo-Dorantes, Francisca Antman, Brian Cadena, Hugh Cassidy, Michael Clemens, Brian Duncan, Brian Kovak, Giovanni Peri, Sebastian Tello-Trillo, Barton Willage, seminar participants at the University of California at Irvine, Syracuse University, Northeastern University, the University of Texas at Austin, San Diego State University, the University of Colorado Denver, the Université du Québec à Montréal, the University of Pittsburg, and Ohio State University, as well as session participants at the NBER Spring 2021 Immigrants and the U.S. Economy Conference, the Society of Labor Economists Annual Conference, the Southern Economic Association Annual Conference, the Economic Demography Workshop, the Stanford Institute for Theoretical Economics (SITE) Summer Workshop on Migration, and the University of California Davis alumni conference. We are also grateful to Reid Taylor, Tyler Collinson and Evan Generoli for excellent research assistance. We thank Sue Long at TRAC for assistance with data on ICE deportations, which we obtained from Syracuse University as TRAC Fellows. Chloe East was supported by funding from the Office of Research Services at the University of Colorado Denver. Finally, Annie Hines benefited from support from the Russell Sage Foundation, the UC Mexico Initiative, and the National Institute on Aging, Grant Number T32-AG000186. As always, all errors are our own. *Corresponding author: Hani Mansour, email: hani.mansour@ucdenver.edu. Chloe N. East, email: chloe.east@ucdenver.edu. Annie Laurie Hines, email: ahines@cornerstone.com. Philip Luck, email: philip.luck@ucdenver.edu. Andrea Velásquez, email: andrea.velasquez@ucdenver.edu.

1 Introduction

The share of undocumented immigrants in the U.S. labor force grew rapidly in the 1990s and early 2000s. By 2008, approximately 8 million undocumented immigrants worked in the U.S., making up 5.4 percent of the labor force (Passel and Cohn, 2016b). In response, local, state, and federal policies were implemented to reduce undocumented immigration through detentions and deportations. However, how these policies impacted the labor market outcomes of undocumented immigrants, and whether they were detrimental or beneficial to U.S.-born workers is still largely unknown (Chassamboulli and Peri, 2015).

In this paper, we study how police-based immigration enforcement policies have affected the short-term labor market outcomes of both immigrant and U.S.-born workers. Specifically, we estimate the labor market effects of a federal policy, Secure Communities (SC), which increased information sharing between local law enforcement agencies and the federal government to facilitate the detection and removal of undocumented immigrants. The policy was ultimately adopted by all U.S. counties, and more than 454,000 individuals were removed under SC during our sample period of 2008-2014.[1] In addition to the large number of removals, SC may have further reduced the supply of immigrant workers who remain in the U.S. through "chilling effects". This is because fear of interacting with local police or having to present forms of identification likely increased the cost of working outside the home and the cost of job searching (Kohli et al., 2011; Valdivia, 2019).[2]

SC was rolled out on a county-by-county basis between 2008 and 2013 because the Department of Homeland Security (DHS) was unable to implement it simultaneously across the U.S. Our empirical model exploits this variation in timing of SC implementation across local labor markets. This spatial approach estimates the total local labor market effects

[1]Statistics on removals under SC come from the Transactional Records Access Clearinghouse (TRAC).

[2]Wang and Kaushal (2018) found that exposure to immigration enforcement policies, including SC, increased mental distress among Latino immigrants. Additionally, Alsan and Yang (2018) found that SC reduced the participation of Hispanic *citizens* in safety net programs for which they were eligible, and attributed this to chilling effects.

of SC for likely undocumented immigrants and U.S.-born workers. This is in contrast to models that exploit variation in immigrant flows across education-experience groups and instead identify national effects of one group relative to another (Dustmann et al., 2016).[3] We find no evidence that the timing of SC enactment in a local area can be predicted by pre-SC changes in demographic and economic characteristics, and distributed lag analyses show no significant trends in labor market outcomes before implementation. Thus, the timing of SC implementation can be thought of as plausibly exogenous. Moreover, the relative speed of the rollout, the fact that all U.S. counties eventually adopted SC, and our focus on estimating the short-run effects of the policy, limits the scope of within-U.S. mobility by workers or capital, so concerns about spatial arbitrage 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, which provides a measure of local labor markets representing both metropolitan and rural areas (Tolbert and Sizer, 1996; Dorn, 2009; Autor et al., 2013; Autor and Dorn, 2013). We merge in information about annual exposure to SC based on the population-weighted share of counties in the CZ that implemented the policy, and estimate distributed lag and difference in difference models, which include CZ and year fixed effects, as well as CZ-specific linear trends. The results are robust to adding controls to account for the impacts of the Great Recession and the presence of other immigration enforcement policies. The results are also similar when using the estimator proposed by Callaway and Sant'Anna (2021) that accounts for potential bias when in two-way fixed effect models with a staggered rollout design.

We identify the effects of SC on likely undocumented immigrants by focusing on a sample of foreign-born individuals aged 20-64 with a high-school degree or less. This sam-

[3] Additionally, Dustmann et al. (2012) show immigrants "downgrade" their skills upon arrival, so the education-experience group approach can lead to misclassification and can bias the estimated effects of immigration on the labor market (Dustmann et al., 2016).

ple of "low-educated foreign-born" (LEFB) captures a large portion of the undocumented population that would have been directly affected by SC. This is a common approach used in the literature to proxy for documentation status, given that it is not asked in large-scale U.S. Census Bureau surveys, including the ACS (Van Hook and Bachmeier, 2013; Passel and Cohn, 2014; Borjas and Cassidy, 2019; Albert, 2021).[4] We also study the effects of SC on U.S.-born individuals aged 20-64. For both demographic groups, we focus on two outcome variables: 1) the employment share of the time-fixed population, which we calculate by dividing the number of employed individuals in the demographic group, CZ, and year, by the total working-age CZ population in 2005 (hereafter abbreviated as the "employment share") and 2) the hourly wage, which we calculate by dividing annual income by annual hours worked for full-time workers in the demographic group.

Focusing first on likely undocumented immigrants, we find that SC reduced their employment share by 5.8 percent. This decline is partly driven by a decrease in their share in the local population (by 1.2 percent), and partly by a decrease in employment among those who remain in the U.S. (by 1.3 percent). The effect of SC on the wages of LEFB is also negative (1.3 percent) but marginally insignificant. These negative effects among undocumented immigrants *who were not deported* is consistent with both the presence of chilling effects, and with a decline in the demand for their labor due to their higher risk of deportations (Albert, 2021).

Turning to the effects of SC on the U.S.-born, we find no evidence that, on average, the policy increased their employment share or hourly wages. Instead, the results indicate that SC *decreased* both the employment share and wages of the U.S.-born by about 0.6 percent. These changes are not driven by population changes among the U.S.-born. Taken together, the results imply that a 1 percent decrease in the employment share of LEFB is

[4] We use the terms "low-educated foreign-born" (LEFB) and "likely undocumented" interchangeably. However, we note that LEFB likely also includes some documented immigrants. Our results are similar when using more restrictive samples of likely undocumented immigrants and when conditioning on citizenship status instead of country of birth (Orrenius and Zavodny, 2009; Amuedo-Dorantes and Bansak, 2012).

associated with a 0.1 percent decline in the employment share and hourly wages of U.S.-born workers. This is consistent with the impacts of *historical* removal policies that also led to little improvements in the labor market outcomes of native workers (Clemens et al., 2018), and in some cases to declines in their wages and employment (Lee et al., 2022).[5]

Informed by the past theoretical and empirical literature, we propose and investigate two potential mechanisms through which SC could have affected the labor market outcomes of U.S.-born workers. First, the decline in the labor supply of undocumented immigrants, and the increase in their risk of deportation, may affect firm's labor demand for substitute and complement U.S.-born workers. Based on the job search models developed by Chassamboulli and Peri (2015) and Albert (2021), which assume that U.S.-born workers have higher reservation wages than undocumented immigrants, we posit that the effect on U.S.-born workers who are substitutes in production for undocumented immigrants is ambiguous.[6] This is because while substitute workers face lower competition due to the decline in the employment of LEFB, the rise in the expected wage costs also reduces job creation. In contrast, for workers who act as complements in production to undocumented workers, the decline in job creation is expected to unambiguously decrease labor demand. To examine this empirically, we estimate the heterogeneous effects of SC on U.S.-born workers by education and across different occupational skill categories. The results indicate that while the employment share of low-educated U.S.-born workers increases at the very bottom of the occupational skill distribution, it becomes muted, and eventually starts to decrease among workers in the middle and upper parts of the occupational skill distribution. This is consistent with the job competition effect being more salient in lowest-skilled occupations, where the share of LEFB is highest, and with the job creation effect playing an important role for workers in

[5]Several papers have examined the effects of *emigration* on the labor market outcomes of non-migrants who remain in sending countries, (e.g. Mishra (2007), Elsner (2013), Dustmann et al. (2015), and Anelli et al. (2020)). We do not directly compare our findings to these papers because removals of existing immigrants vs. departures of emigrants are likely to affect natives in the sending country through different mechanisms.

[6]Research shows that undocumented immigrants have lower reservation wages compared to native-born workers with similar skills (Rivera-Batiz, 1999; Kossoudji and Cobb-Clark, 2002; Pan, 2012; Albert, 2021).

higher-skilled occupations.

The second potential mechanism is a reduction in the demand for local goods, which is also expected to reduce labor demand. The impact of immigration-induced consumption on the labor market has received less attention in the existing literature (Dustmann et al., 2017).[7] To examine this possibility, we estimate the effects of SC separately for U.S.-born workers in tradable and non-tradable industries. Consumption effects should have a larger impact on workers in the non-tradable sector (e.g., food services), where demand is local and where prices respond to local shocks (Burstein et al., 2020). Indeed, the negative employment effects of SC on U.S.-born workers are larger in the non-tradable sector. We interpret the results as evidence that reduced local consumption may have also played a role in explaining how SC affected the labor market. Our reduced form approach, however, does not enable us to distinguish the relative importance of each mechanism.

An important contribution of this paper is that it is the first to estimate the short-run labor market effects of a mandatory federal police-based enforcement policy. Prior work has mostly focused on estimating the effects of optional 287(g) agreements, which authorized local law enforcement to act as immigration agents, including by checking immigration status among people who were arrested. While these agreements are similar in design to SC, they were voluntarily adopted by only about 50 counties, raising concerns about endogenous policy adoption and making identification more challenging (Bohn and Santillano, 2017; Pham and Van, 2010). In addition, unlike the prior 287(g) literature, we separately identify the effects of SC on immigrants and natives and examine the role of different mechanisms. The only other paper on the labor market effects of SC focuses on highly educated U.S.-

[7]For example, Greenwood and Hunt (1984) and Altonji and Card (1991) discuss the possibility that immigration inflows can impact the labor market outcomes of native workers through an increase in aggregate demand. Hercowitz and Yashiv (2002) and Örn B. Bodvarsson, Van den Berg and Lewer (2008) conclude that the increase in local demand due to the arrival of immigrants can mask a negative labor market impact due to increased competition. Dustmann et al. (2017) argued that the negative employment effect of Czech migrants on native German workers may be explained by the commuting nature of the migrants, and, thus, a lack of a consumption channel.

born mothers with young children, who are likely affected through an increase in the cost of outsourcing household production (East and Velásquez, Forthcoming). In contrast, we take a broad approach and look at the overall effects of SC on the labor market outcomes of both immigrants and natives in the local area. We also contribute to a small set of papers studying others impacts of SC including on participation in safety net programs among Hispanic citizens (Alsan and Yang, 2018), immigrants' health outcomes (Wang and Kaushal, 2018), immigrants' marriage patterns (Bansak and Pearlman, 2021), and local crime outcomes (Miles and Cox, 2014; Hines and Peri, 2019). Given the volatility of immigration enforcement policies across the last three presidential administrations, understanding their effects is crucially important for policymakers.

We also contribute to the broader immigration literature by providing contemporary evidence about the impact of immigration policy "pull" factors (that increase voluntary departures or forced removals) on labor market outcomes, rather than immigration "push" factors (that increase arrivals), which has been the focus of much of the prior research (Clemens et al., 2018). The effects may not be symmetric because removing immigrants who are already integrated into the labor market is likely different than adding new workers to the labor market, for example because of different degrees of substitution between these groups of immigrants and natives (Lee et al., 2022). This is a key contribution because much of the evidence on the effects of removing immigrants in the U.S. comes from historical policies (Clemens et al., 2018; Lee et al., 2022), and there have been major shifts in the U.S. immigration system and the economy since then. Additionally, we document that removal policies do not impact labor markets outcomes only through deportations, but also through their impact on immigrants who remain in the U.S.

2 Policy Background

2.1 The Secure Communities Program

Secure Communities is one of the largest interior immigration enforcement programs administered by U.S. Immigration and Customs Enforcement (ICE).[8] 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 check after a person is arrested by sending their fingerprints to the FBI. Prior to SC implementation, fingerprints received by the FBI were not used to check the legal status of a person or their eligibility for removal. Instead, violators of immigration law were identified via interviews conducted by federal agents under the Criminal Alien Program (CAP) or by local agents authorized to act as immigration agents under 287(g) agreements. Under SC, fingerprints were 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.

If the fingerprints were matched, "detainers could be issued when an immigration officer had reason to believe the individual was removable", which could be for criminal reasons or for immigration-crime-related reasons.[9] A detainer (or deportation) did not have to be preceded by a conviction. 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, SC substantially increased the probability of apprehension and deportation of undocumented immigrants by ICE.

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

[9]`https://www.ice.gov/pep`.

When estimating the effects of SC on the labor outcomes of both immigrants and natives, it is important to note that this is a police-based enforcement policy. This is in contrast to employment-based enforcement policies, such as E-Verify, which target undocumented immigrants at their place of employment. However, the operation of SC is likely to have affected immigrant labor supply not only through deportations of people interacting with the criminal justice system, but, also through chilling effects among those who were not deported.[10] As we show in Table (1), among the roughly 454,000 individuals removed, 21 percent were not convicted of a crime, and 61 percent had a non-violent crime as the most serious offense, indicating that SC increased the risk of deportations of non-violent and law-abiding undocumented immigrants (Alsan and Yang, 2018; Amuedo-Dorantes et al., 2019; East and Velásquez, Forthcoming).[11] In fact, qualitative 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).[12] Thus, in addition to deportations, SC is likely to have increased the cost of interacting with local police, commuting to work, and the cost of searching for a job for a broad swath of the immigrant community.

The broad reach of SC and the disruption it caused to immigrant communities led several local jurisdictions, known as sanctuary cities, to refuse to cooperate with ICE detainer requests, arguing that they were unconstitutional under the Fourth Amendment (Alsan and Yang, 2018). At the end of 2014, the SC program was replaced by the Priority Enforcement

[10]It is also possible that SC 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.

[11]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. To classify violent crimes, we followed the definitions of the FBI's Uniform Crime Reporting (UCR) Program (`https://ucr.fbi.gov/crime-in-the-u.s/2010/crime-in-the-u.s.-2010/violent-crime`). The top violent crimes leading to a deportation in this period were assault, burglary, domestic violence, and robbery.

[12]Immigrants from Latin American countries were overrepresented among those deported under SC; in 2007, undocumented immigrants from Latin American countries accounted for 70 percent of the undocumented population of immigrants in the U.S. (Passel and Cohn, 2011), but Table (1) shows that 92 percent of those deported were from a Latin American country.

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.[13]

2.2 Implementation of Secure Communities

Unlike previous police-based enforcement 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 because local agencies had much more limited discretion in the usage of the program, compared to other interior immigration enforcement polices like 287(g) agreements (Miles and Cox, 2014). Moreover, despite being a federal program, due to resource and technological constraints, SC was rolled out on a county-by-county basis between 2008-2013, until the entire country was covered (Miles and Cox, 2014; Alsan and Yang, 2018).

Our identification 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 choice of the earliest activations of SC 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, however, they show that early adopters were not selected in terms of the county's economic performance, crime rates and potential political support for SC. They also explain that while early adoptions were done on a county by county basis, and therefore more targeted, the timing of adoption as the rollout progressed was more "random" since it became common for multiple inactive counties within the same state to be simultaneously activated on the same date ("mass activations"). This pattern can be seen in Figure (1) which plots the rollout of SC across counties and over time. In our main sample, we include the whole country, but the results are robust to excluding early-adopter areas.

[13]Secure Communities was re-activated in 2017 and suspended again in 2021.

We gathered information on the rollout dates of SC from ICE, and 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 two columns of Table (2) report the average and standard deviation of changes in CZ characteristics, respectively. In column 3, we report estimates of the relationship between changes in CZ characteristics between 2005-2007 and the year of SC adoption in the CZ. The dependent variable is the calendar year of CZ adoption, and we include all changes in characteristics together in one regression. These characteristics include the CZ immigrant population, local 287(g) agreements, and Bartik-style measures of labor demand. We construct four Bartik-style measures of labor demand that correspond to the following four demographic groups: 1) U.S.-born 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 group-specific CZ-level employment by industry, as a fraction of total group-specific CZ employment in 2005. We then multiply these industry shares by the industry-specific national (not group-specific) employment for all working age adults in each year. This allows us to have a measure of predicted labor demand for each group by CZ, based on the industries that they work in the base period, and the overall changes in employment in these industries over time. See Appendix A for additional detail on the CZ-year level control variables.

Out of 10 pre-SC characteristics, there are only two statistically significant relationships: the change in 287(g) Jail agreements and the change in the male non-citizen population. However, although significant, the magnitude for 287(g) agreements is small: an increase of one standard deviation in exposure to 287(g) Jail agreements is associated with a 3.6 months earlier adoption of SC (-2.189*0.14*12). For the male non-citizen population, the magnitude is also small: a one standard deviation is associated with a 1.4 month later adoption of SC (0.359*0.32*12). This further supports the idea that the timing of the rollout can be thought of as good as random.

3 Data

3.1 Employment and Wage Outcomes

To construct the analysis sample, we merge information on the rollout dates of SC with local-level employment and wage data drawn from the 2005-2014 American Community Survey (Ruggles et al., 2017). The ACS is a repeated cross-sectional dataset covering a 1 percent random sample of the U.S., and it is particularly well-suited for our purposes because it is nationally representative and includes local labor market identifiers across the U.S. In addition, it includes a rich set of demographic characteristics, including country of birth, enabling us to calculate labor market outcomes for different groups of foreign-born and U.S.-born individuals.

We begin our sample in 2005, as this is the first year the detailed geographic identifiers used to define the Commuting Zones (CZ) are in the public-use data. Our sample ends in 2014 when SC was replaced by the Priority Enforcement Program. We chose not to extend the analysis to later years in order to exclusively focus on the effects of SC. We conduct our analysis at the CZ level, but the ACS data only identify Public Use Microdata Areas (PUMAs), so we concord PUMAs to CZs using 1990 CZ definitions and the crosswalks in Dorn (2009) and Autor and Dorn (2013). Similarly, we concord the county-level SC data to the CZ-level by weighting the SC value for each county by the county's population share in the CZ, similar to the approach taken by Watson (2013) and Alsan and Yang (2018) (most CZs include multiple counties). 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. 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), but our results are robust to conducting our estimation at the PUMA level rather than at the CZ level.

To estimate the effects of SC on employment, we construct the employment shares of the time-fixed CZ population for likely undocumented and U.S.-born workers by dividing the number of employed individuals in each group at the CZ-by-year level by the total working-age CZ population in 2005. We multiply these shares by 100 to ease the presentation: $\frac{Emp_{jt}}{Pop_{j2005}} * 100$, where j indexes CZ and t indexes survey year. To calculate both the numerator and the denominator, we use the ACS-provided person-level weights. Our primary samples are working-age (20-64) low-educated foreign-born (high school degree or less) and working-age U.S.-born individuals.[14] In both samples we exclude workers in the military and public administration sectors because of the potential direct impact of SC on their employment, but our results are robust to including these sectors. Given the construction of this employment share variable, SC will only affect the numerator. We use this as our primary measure of employment because it captures employment changes of likely undocumented immigrants through both potential mechanisms: 1) changes in the population of workers through both voluntary and involuntary migration, and 2) changes in employment of those remaining in the U.S. We do not focus on the employment to population ratio (or employment rate) used in other studies on the labor market impacts of immigration (e.g. Altonji and Card (1991), Dustmann et al. (2005), and Boustan et al. (2010)) because it only captures the effects of SC on the employment of immigrants who remain in the U.S., and is thus likely to understate the total employment effects of SC on likely undocumented immigrants (Dustmann et al., 2017). However, to shed light on the role of these two mechanisms, we also estimate the effects of SC on 1) the population share of time-fixed CZ population, and 2) the employment to population ratio for both LEFB and U.S.-born individuals. We use the employment share as our main measure of employment for U.S.-born to easily compare the results across both

[14] Our primary definition of likely undocumented immigrants does not condition on citizenship status because of concerns about misreporting by undocumented immigrants (Van Hook and Bachmeier, 2013; Brown et al., 2018). One additional concern with our definition of "likely undocumented" is 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). While previous studies estimate an overall 7.5 percent undercount of undocumented immigrants (Warren, 2014), we are unable to assess how the undercount varies in response to SC. This undercount is likely to bias the estimates of the LEFB population towards zero, while it should not impact the estimated effects on the U.S.-born population.

groups (though, we show the effects on the employment to population ratio of U.S.-born workers is very similar.)

We complement our employment analysis by estimating the effect of SC on hourly wages. Hourly wages are not directly reported in the ACS, so we calculate them by dividing annual earnings by annual hours.[15] We then use the ACS-provided person-level weights to calculate average group-specific wages for each CZ and year. As is common in the literature, we use a sample of full-time LEFB and U.S.-born workers aged 20-64 who report to usually work 30 or more hours per week (Dustmann et al., 2017). All wages are in constant 2014 dollars.

Since our sample period spans the Great Recession, we test the robustness of the results to accounting for CZ time-varying economic conditions that may influence employment by including Bartik-style measures of labor demand (Bartik, 1992) for U.S.-born workers and foreign-born workers separately (described in section 2.2 and Appendix A), measures of the housing boom and bust, and region by year fixed effects. We also check the robustness of the results to controlling for the presence of 287(g) agreements, E-Verify, and Sanctuary City policies.[16]

We provide summary statistics for all the main variables in Appendix Table (A1). The average employment share of LEFB across CZs is 6.75 percent. The average employment share of U.S.-born workers is 58.61 percent. The *total* employment share for all individuals of working-age is 71.39 percent (not shown in the table), so LEFB make up roughly 9.5 percent of all workers in this time period. Turning to wages, the average wage of full-time LEFB is $15.60 per hour, and the average wage of U.S.-born full-time workers is $24.39.

[15]We calculate annual hours by multiplying usual hours of work per week by weeks worked last year. Because weeks of work are reported in intervals in 2006-2014, we use the midpoint of the reported interval as the value for weeks of work. Specifically, weeks values of 8, 20.8, 33.1, 42.4, 48.3, and 51.9 are used, respectively, for the reported intervals 1-13, 14-26, 27-39, 40-47, 48-49, and 50-52. To adjust for outliers, observations are winsorized at the 2.5 and 97.5 percentile wage value.

[16]Details on these programs and variable construction are in Appendix A.

4 Empirical Strategy

The empirical strategy uses the geographic and temporal variation in the implementation of the SC program to identify its effects on the employment and wages of likely undocumented and U.S.-born workers. Specifically, we estimate the following model:

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

where 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 model includes year fixed effects, λ_t, to account for national economic shocks, and CZ fixed effects, ν_j, to control for CZ time-invariant unobserved heterogeneity, such as proximity to the border. To account for differential trends in labor outcomes across areas, we include CZ-specific linear trends, $t\delta_j$.[17] Our fully specified model also includes a vector of Bartik-style measures of labor demand, X_{jt}, which controls for time-varying CZ-specific economic conditions. The model is weighted by the CZ population in 2000, which ensures that we are estimating the policy-relevant effect of SC on the average individual. We cluster the standard errors by CZs.

The underlying identification assumption is that there were no time-varying CZ-specific factors correlated with the timing of the adoption of SC. To provide support for this assumption, we follow Suarez Serrato and Zidar (2016), Fuest et al. (2018), and Schmidheiny and Siegloch (2020) and estimate the following "distributed lag" model:

$$Y_{jt} = \alpha + \sum_{k=-2}^{2} \gamma_k SC_{j,t-k} + X'_{jt}\gamma + \nu_j + \lambda_t + t\delta_j + \epsilon_{jt}, \tag{2}$$

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

where the estimated γ_ks measure the relationship between SC and the *change* in the labor market outcome of interest. The pre-treatment effect changes are denoted by the leads ($k < 0$), and the effects of SC after adoption on changes in outcomes are denoted by the lags ($k \geq 0$). The distributed lag model allows us to use a continuous treatment variable, so it is more comparable to our baseline model. However, to calculate treatment effects that represent the total effect of SC before and after implementation we need to choose a reference period and then sum the distributed lag coefficients moving away from this reference period. This is because the γ_ks only measure changes in the treatment effect. We denote the total effects that we plot as $\beta_k s$. We follow the convention used in event study models and choose the period $k = -1$ as our reference period so we set $\beta_{-1} = 0$. For periods $k < -1$ we cumulate negatively so that $\beta_{-2} = -\gamma_{-1}$, $\beta_{-3} = -\gamma_{-1} - \gamma_{-2}$, and so on. For periods $k > -1$ we cumulate positively so that $\beta_0 = \gamma_0$, $\beta_1 = \gamma_0 + \gamma_1$, and so on (Schmidheiny and Siegloch, 2020). If the parallel trends assumption holds, the estimates of β_k where $k < 0$ should be close to zero and statistically insignificant.

5 Results

5.1 Effects on Employment and Wages

We begin by estimating the labor market effects of SC on likely undocumented immigrants. Estimates of equation (1), where we control for CZ and year fixed effects, as well as CZ-specific linear trends, are presented in Panel A of Table (3). The results in Column 1 indicate that SC reduced the employment share of LEFB by 0.402 percentage points, significant at the 1 percent level. We add controls for Bartik-style measures of labor demand in Panel B, which capture CZ-level differences in the impact of the Great Recession. Reassuringly, these controls do not impact the estimated effects, suggesting that the timing of SC implementation was not correlated with predicted changes in local economic conditions. Including these Bartik-style controls is our preferred specification, and, with this, we find that SC reduced the employment share of LEFB by about 5.78 percent relative to their mean ($-0.390/6.75$). Although our identification strategy is not designed to estimate *nationwide* effects, we compare the decline in the employment share of LEFB to the *total* number of deportations as a share of the 2005 U.S. population. If we assume that deportations were evenly distributed across CZs, the share of deportees in each CZ would be equal to 0.262 ($\frac{454000}{1.73\times10^8}$). Comparing this our estimate of 0.390 suggests mechanisms other than deportations may be important.

The effects of SC on the log hourly wage of likely undocumented immigrants are presented in Column 4 of Table (3). As with the effects on employment, the estimates do not change when we control for local impacts of the Great Recession in Panel B. We find that SC reduced the average hourly wage of LEFB by 1.3 percent, and this estimate is marginally insignificant (p-value=0.15). The wage results are estimated with less precision than the employment results, which may be due to the potential measurement error in the wage variable. It is important to note that wages are only observed for currently employed workers. Although we cannot assess directly how the employment and wage outcomes of deportees compare to the average LEFB, we find little evidence that SC changed the composition of LEFB who were not deported (in terms of age, ethnicity, number of years in the U.S., and educational attainment). Nonetheless, if we assume that deported LEFB had lower wages and were less likely to be employed than the average LEFB, then we would conclude that the labor market effects of an enforcement policy that targets the "average" LEFB would be larger (in absolute value) compared to a police-based enforcement policy.

Since most deportees under SC were male, we expect the policy to have had a larger impact on the employment share of males compared to females. The results in Columns 2-3 of Table (3) show that while SC reduced the employment share of LEFB males by 0.329 percentage points, it decreased the employment share of LEFB females by only 0.062 percentage points (significant at the 1 and 10 percent levels, respectively). In relative terms, the results imply that the effect of SC on the employment share of LEFB males is three times as large as the effect for LEFB females (7.81 vs. 2.43 percent). Although statistically insignificant, the effect on hourly wages is also larger for male immigrant workers (Column 5) relative to female immigrant workers (Column 6). Overall, these results show that although the labor market effects of SC are stronger for males we cannot rule out that SC had a negative impact on the labor market outcomes of LEFB females.

We next estimate the effect on the employment share and hourly wages of U.S.-born workers, and these results are presented in Table (4). Similar to our findings for LEFB, the results are not sensitive to adding Bartik-style controls (Panels A and B). The results provide no evidence that the average employment share or hourly wages of U.S.-born workers increased after the implementation of SC, contrary to the predictions of the canonical labor supply and demand model under the assumption that LEFB and U.S.-born workers are substitutes. Instead, SC *reduced* the employment share of U.S.-born workers by 0.320 percentage points, significant at the 10 percent level, which is about a 0.55 percent decline relative to the mean employment share (Column 1). Based on the 95 percent confidence interval of this estimate, we can rule out employment effects which are smaller than -0.65 percentage points and bigger than 0.01 percentage points, providing little evidence that U.S.-born workers benefit from increased immigration enforcement. Additionally, SC decreased hourly wages of U.S.-born workers by 0.6 percent (Column 4, statistically significant at the 5 percent level). Based on the 95 percent confidence interval of this estimate, we can rule out wage effects which are smaller than -1.2 percent and bigger than -0.012 percent. The decline in both the employment share and wages of U.S.-born workers is consistent with SC reducing the demand for their labor.

In contrast to the LEFB, it is a priori unclear whether the effects of SC should differ between male and female U.S.-born workers. The results in Column 2-3 of Panel B in Table (4) indicate that SC reduced the employment share of male U.S.-born workers by 0.228

percentage points, about 0.75 percent relative to the mean, which is statistically significant at the 10 percent level. The effect of SC on the employment share of female U.S.-born workers (Column 2) is about half as large (0.32 percent) and is not significant at conventional levels. These results could be explained by the higher degree of overlap in the employment of male LEFB and male U.S.-born workers across sectors, shown in Appendix Figure (A1). This figure plots the employment share of male and female LEFB and U.S.-born workers across sectors in 2005. Sectors with the highest share of male LEFB workers, such as agriculture and construction, also employ a much larger share of male U.S.-born workers relative to female U.S.-born workers. For wages, we find a less clear difference by sex (Columns 5-6).

To corroborate the validity of the research design, Panels A and B of Figure (2) plot the dynamic effects of SC, before and after implementation, for LEFB individuals estimated with equation (2). Panels C and D plot these dynamic effects for U.S.-born individuals. In all panels, the β_k's in the pre-SC periods are small and statistically indistinguishable from zero, consistent with the parallel trends assumption. In each panel, in the post-period, we include the corresponding difference in difference estimates, reported in Columns 1 and 4 of Tables (3) and (4), with a gray line for ease of comparison. Although we lack precision, the distributed lag estimates provide evidence of a decline in the employment share and wages of LEFB after SC implementation that are similar in magnitude to the difference in difference estimates. We also see similar patterns for U.S.-born workers. The loss in precision may be because the distributed lag model is a more demanding specification. Nevertheless, this test confirms that our estimates are not being driven by differential pre-trends across CZs.[18]

For some outcomes, there is an increase in effect size over time after SC implementation, which could imply that the treatment effects are dynamic, indicating that the difference in difference estimates may be biased (Goodman-Bacon, 2021). It is important to note, however, that the estimates could grow over time because SC is phased in across counties within the

[18] We also report these β_k's and their standard errors in Appendix Table (A2).

CZs at different times. To test whether heterogeneous treatment effects across CZs bias our estimates, we implement the estimator proposed by Callaway and Sant'Anna (2021). The advantage of this approach is that it avoids using earlier treated units as controls for later treated units, which are comparisons that can lead to biased estimates. The results are presented in gray in Appendix Figure (A2), along with the estimates from the distributed lag model in equation (2) in black.[19] The results from the Callaway and Sant'Anna method follow the same general patterns as in the distributed lag model. Overall, the confidence intervals are smaller when using the Callaway and Sant'Anna method, possibly because these methods do not calculate standard errors in the same way. And, there are a few coefficients that are statistically significant with Callaway and Sant'Anna that are not in the distributed lag model. However, except for two coefficients in Panel C, the 95 percent confidence intervals overlap across the two methods.

5.2 Changes in Population vs. Employment

Given the way we construct the employment share variable, the decline for LEFB could be due to changes in migration or changes in employment among LEFB who remain in the U.S. We first investigate the migration channel by estimating whether SC reduced the population share of LEFB, which, analogous to the employment share, is calculated by dividing the population of LEFB in year t and CZ j by the working-age CZ population in 2005, and multiplying by 100 ($\frac{Pop_{jt}}{Pop_{j2005}} * 100$). The result, shown in the first Column of Panel A in Table (5), indicates that SC reduced the population share by 0.119 percentage points, or a 1.18 percent relative to the mean (p-value=0.16). We also split the sample by sex in Columns 2-3, and the results are consistent with the fact that most of those deported under

[19]The models in this figure do not control for Bartik-style local labor demand due to differences in the way time-varying controls are included across the two estimators. The Callaway and Sant'Anna method does not allow for a continuous treatment, so we dichotomize the SC variable to equal one once a CZ is first treated, and zero in prior years. To account for linear trends with the Callaway and Sant'Anna estimator, we use a two-step method (Goodman-Bacon, 2016). The first step estimates a linear trend for each CZ and outcome using pre-period data only. The second step de-trends the data using the extrapolated trend. We use these de-trended variables as our outcomes.

SC were male. The male likely undocumented population share declined by a significant 1.9 percent compared to an insignificant decline of 0.38 percent for females. If we assume that all deported individuals were employed, these results imply that changes in the population of overall (and male) LEFB can explain up to a third of the overall (and male) decline in their employment share (-0.119/-0.390 for all and -0.100/-0.329 for males).[20]

Next, we estimate the effect on the employment to population ratio of LEFB. To construct this variable, we take the same numerator as we used to calculate the employment share, but the denominator is instead the time-varying group-specific population in each CZ j and year t: $\frac{Emp_{jt}}{Pop_{jt}} * 100$ (*Labor Force Statistics from the Current Population Survey*, 2021). This allows us to examine the employment response *among LEFB who remain in the U.S.* The results indicate that SC reduced the employment to population ratio of LEFB by 0.907 percentage points, or 1.37 percent relative to the mean (Column 1 of Panel B in Table (5)). This decline in the overall employment to population ratio is driven by likely undocumented male workers (Column 3), and is due to an increase in their unemployment, rather than changes in their labor force participation (Columns 1 and 3 of Appendix Table (A4)). This is consistent with either a chilling effect causing immigrants to take longer to find a job or a decrease in demand for their labor, which we return to in more detail below.

In contrast to the effect of SC on the population share of LEFB, we find little evidence that the policy changed the population share of U.S.-born. Although the point estimate of -0.139 (Column 4 of Panel A in Table (5)) is close in magnitude to the estimate for LEFB, it is not statistically significant and corresponds to only a 0.16 percent change relative to

[20] To further verify that SC affected the undocumented population, we estimate its effect on detentions using restricted-access data from TRAC, which is described in Appendix A. Appendix Table (A3) reports the impact of SC on the number of detentions at the CZ-level scaled by the total working-age CZ population in 2005. Using the full set of controls as described in equation (1), the results in Column 2 indicate that SC increased the detention share of the time-fixed population by 0.118 percentage points, an increase of about 123 percent relative to the mean detention rate. We are unable to directly estimate the impact of SC on deportations because our data set on deportations does not include data prior to the implementation of SC. Moreover, we chose not to implement an IV strategy using detention data, since the increase in detentions is only one potential mechanism through which the effects operate.

the mean. This result supports our research design, as it indicates that SC did not impact the migration decisions of U.S.-born individuals. The effects of SC on U.S.-born workers, instead, operate through a reduction in their employment to population ratio. Specifically, we find that SC reduced the employment to population ratio of U.S.-born workers by 0.519 percentage points, or 0.77 percent relative to the mean, which is significant at the 1 percent level. The fact that the effects for the U.S.-born are similar whether we use a time-fixed (employment share) or time-varying (employment to population ratio) denominator (-0.55 percent compared to -0.77 percent) and have over-lapping 95 percent confidence intervals provides further evidence that population changes are not an important mechanism for U.S.-born workers. Again, these effects operate through an increase in unemployment, which is consistent with a decline in labor demand for U.S.-born workers (Columns 2 and 4 of Appendix Table (A4)).

5.3 Robustness Checks

We first explore the robustness of the results for LEFB to using alternative definitions of likely undocumented immigrants in Table (6), since documentation status for immigrants is not available in the ACS. The results on the employment share and hourly wages are reported in Panels A and B, respectively. In Column 1, we restrict the sample to include only Hispanic LEFB. In Column 2, we restrict the sample to Hispanic LEFB who entered the U.S. after 1980, and, in Column 3 we restrict the sample to LEFB who were born in Mexico or Central America ("CA") and entered the U.S. after 1980 (Passel and Cohn, 2014; Warren, 2014; Passel and Cohn, 2016a; Borjas and Cassidy, 2019).[21] The results in Panel A are similar across these different definitions, indicating that SC reduced the employment share of likely undocumented immigrants by about 8.74 to 9.20 percent. These results are slightly larger but similar to the effect on our main sample of all LEFB (5.78 percent).

[21] Previous literature has used 1980 as a cutoff year to classify likely undocumented immigrants because immigrants who arrived in the U.S. before 1982 could have been legalized under the Immigration Reform and Control Act (IRCA) (Warren, 2014).

Although statistically insignificant, the wage results using these alternative definitions of likely undocumented immigrants in Panel B are also similar in magnitude to the results on all LEFB, except in column 2 where the estimate is smaller and statistically insignificant, though it has overlapping confidence intervals with our baseline estimates. We also show in Appendix Table (A5) that the estimated effects are similar when the sample of LEFB is based on U.S. citizenship (low-educated non-citizens), instead of country of birth (low-educated foreign-born) (Orrenius and Zavodny, 2009; Amuedo-Dorantes and Bansak, 2012).

We next check the robustness of the main results for LEFB and U.S.-born workers to alternative samples and specifications, which we report in Table (7) (employment share), and Table (8) (hourly wages). For ease of comparison, we report the estimated effects in Columns 1 and 4 from Tables (3) and (4) in the first column of each of these tables. In Column 2, we drop CZs that adopted SC before 2010, since these have been shown to be more selective on observable characteristics, such as proximity to the U.S.-Mexico border (Cox and Miles, 2013). We next drop CZs that adopted a Sanctuary City policy before the implementation of SC in Column 3, and, in Column 4 we drop Arizona, which passed measure SB 1070 in 2010, one of the broadest and most restrictive immigration enforcement measures in the country. Reassuringly, the results in Columns 2-4 for both employment and wages are robust across all these different estimation samples for both LEFB and U.S.-born workers.[22]

Our sample period overlaps with the rollout of both 287(g) agreements and E-Verify policies. We include these controls one at a time (287(g) in Column 5 and E-Verify in Column 6) to better understand how each program in isolation might affect our estimates, but the results are similar if we control for both at the same time. Controlling for the presence of either policy does not impact the estimated effects of SC. Thus, the results do not indicate

[22]The results are also robust to dropping the largest 1 and 5 percent of CZs, which excludes the largest 80 and 370 CZs, respectively. This suggests that the effects of SC are not being identified off of a handful of large labor markets. These results are available upon request.

that these enforcement policies confound the estimated effects of SC.

To further control for changes in local economic conditions, we include region by year fixed effects to account for the fact that different regions were impacted differently by the Great Recession.[23] The results in Column 7 indicate that the addition of these fixed effects has little impact on the estimated effects of SC. Finally, we include quadratic trends in the 2000-2006 change in local housing prices, and quadratic trends in the 2000-2006 change in the number of housing building permits issued in the CZ. These controls provide an alternative measure for the differential effects of the Great Recession. The results in Column 8 do not meaningfully change from the baseline, providing further support that the implementation of SC was not correlated with the severity of the Great Recession.

5.4 Discussion of Results

We gauge the plausibility of the estimated effects of SC by first comparing them to historical policies that removed immigrants from the U.S. labor market. Specifically, the fact that SC had no beneficial effects on U.S.-born employment or wages is consistent with the findings of Clemens et al. (2018) who showed that the removal of migrant labor through the ending of the Bracero program did not improve employment or wages of U.S. native workers because firms adjusted their capital investments and output goods. Lee et al. (2022) studied repatriations (forced out-migration) in the 1930s, and estimated that a 1 percent decline in the population of Mexican migrants reduced the probability of being employed for incumbent native workers by 0.2-0.3 percent, and reduced overall native wages by 0.3 percent.[24] In comparison, our results imply that a 1 percent decrease in the employment share of LEFB is associated with

[23]Regions are defined based on nine Census divisions within the U.S. These include New England, Middle Atlantic, East North Central, South Atlantic, East South Central, West North Central, West South Central, Mountain, and Pacific. Note that a few CZs span multiple divisions, and we assign them to the division to which the majority of their population belong. We do not include state by year fixed effects because 10 states and the District of Columbia implemented SC on a state-wide basis. Moreover, many CZ's span multiple states making state by year fixed effects empirically difficult to implement.

[24]Unlike in our analysis, Lee et al. (2022) found that U.S.-born individuals moved within the U.S. in response to repatriations, so we focus on their estimates for individual workers, rather than local labor markets, the latter of which is influenced by internal migration.

a 0.1 percent decline in the employment share and wages of U.S.-born. We are unable to directly compare our results to the effects of 287(g) agreements because these papers do not estimate the effects separately for immigrants and natives (Bohn and Santillano, 2017; Pham and Van, 2010).[25]

Next, we compare our findings to previous studies on the impact of immigration *inflows*. We particularly focus on studies that use policy-driven variation in the inflow of immigrants across space and over time (Dustmann et al., 2016). Consistent with no overall benefit of removing immigrants on the outcomes of natives, Card (1990) and Peri and Yasenov (2019) found that a large influx of Cuban immigrants to the Miami labor market did not reduce the wages of low-educated native workers or increase their unemployment rates. More recently, Foged and Peri (2016) studied the impact of refugee inflows to Denmark on the labor market outcomes of Danish workers and found that a 1 percentage point increase in the employment share of low-skilled refugees led to a 0.8 percent increase in the fraction of the year worked by low-educated native Danish workers, and a 1.8 percent increase in their hourly wage. This positive impact was driven by Danish workers who were able to move to less manual-intensive occupations, consistent with a complementarity in production between refugee and low-educated Danish workers. Our results (while of the opposite sign, since we look at immigrant *outflows*) are of a similar magnitude: a 1 percentage point decrease in the employment share of LEFB led to a 1.4 percent decrease the employment share of U.S.-born workers (0.55/0.39), and to a 1.5 percent decrease in their wages (0.6/0.39). An exception are the results of Dustmann et al. (2017) who found that an increase in commuter Czech workers to bordering German cities decreased the employment and wages of German workers. This may be because Czech workers did not live in Germany, so had a limited impact on local consumption, which, as detailed below, we find to be potentially important

[25] A broad literature has also estimated the effect of an employment-based enforcement program on the labor outcomes of immigrants and natives. Research on E-Verify has found negative effects on labor outcomes of likely undocumented immigrants, with mixed evidence on the effects on documented immigrants and native workers (Amuedo-Dorantes and Bansak, 2014; Orrenius and Zavodny, 2015; Ayromloo et al., 2020).

in explaining the negative effects of SC on U.S.-born workers. Additionally, firms may have considered the commuting policy to be temporary, which prevented them from expanding their operations in response to the influx of workers.

6 Sources of Changes in Labor Demand for US-Born Workers

In this section, we test for evidence of two possible mechanisms through which SC may have impacted the labor market outcomes of U.S.-born workers: 1) changes in job competition and job creation, and, 2) changes in local consumption.

6.1 Job Competition and Job Creation

We first consider how SC may impact the demand for substitute and complement U.S.-born workers. In the canonical model, immigrant and native workers are assumed to be perfect substitutes and earn the same wage, so the decline in the labor supply of undocumented immigrants is expected to increase the employment and wages of U.S.-born workers. However, several studies have found that undocumented immigrants in the U.S. earn substantially lower wages compared to U.S-born workers. This could be due to differences in skill, or to the fact that employers exert monopsony power over undocumented immigrants because of their legal status, which reduces their outside options, limits their access to the social safety net, and lowers their bargaining power (Rivera-Batiz, 1999; Kossoudji and Cobb-Clark, 2002; Pan, 2012; Albert, 2021). As a result, a reduction in the labor supply of undocumented immigrants is expected to increase the average labor costs that firms face, which decreases their ability to create new jobs. This decline in job creation counteracts the positive effects that U.S.-born substitute workers experience from decreased job competition with undocumented immigrants. Therefore, as Chassamboulli and Peri (2015) and Albert (2021) show with a job search model, these two competing forces imply that the effect of a policy like SC on the labor market outcomes of substitute U.S.-born workers is theoretically ambiguous.[26] In contrast, the decrease in job creation is expected to unambiguously decrease the demand for U.S.-born workers who act as complements to low-educated workers (including LEFB).

The predictions described above suggest that the spillover effects of SC on U.S.-born workers should vary by their degree of substitution with undocumented immigrants. To proxy for this, as is common in much of the prior literature, we estimate the heterogeneous effects of SC by education, where U.S.-born workers with a high school degree or less are considered the closest substitutes for immigrant workers. The results in Table (9) indicate that SC led to a statistically insignificant 0.107 percentage point decline in the employment share of low-educated U.S.-born workers and a statistically significant 1.2 percent decline in their wages (Column 1). SC also reduced the employment share of U.S.-born workers with more than a high school degree (high-educated) by 0.213 percentage points, significant at the 10 percent level, and led to a statistically insignificant 0.4 percent decrease in their wages (Column 2). Thus, the evidence is consistent with a decline in the labor demand for both education groups of U.S.-born workers possibly due to a decline in job creation.

Prior studies, however, have shown that immigrants "downgrade" their skills upon arrival and that native workers shift to less manually-intensive occupations in response to migration (Dustmann et al., 2012; Foged and Peri, 2016). Thus, stratifying native workers only by education may be too coarse to allow us to identify who will be impacted by the job competition and job creation effects. We use an alternative method to estimate the heterogeneous effects of SC by stratifying the U.S.-born workers into occupational skill categories (Peri and Sparber, 2009; Ottaviano and Peri, 2012). Specifically, we categorize occupations based on the fraction of workers that have at least a college degree in each 3-digit SOC occupation in 2005. We plot the distribution of occupational skill based on this measure in

[26]The source of the decline in the competition faced by native workers differ between the two papers. While hiring is assumed to be random in Chassamboulli and Peri (2015), Albert (2021) assumes non-random hiring in which firms can distinguish between types of workers based on their nativity and immigration status.

Appendix Figure (A3).[27] The median occupation has only about 13 percent of workers with a college degree, and the 25$^{\text{th}}$ and 75$^{\text{th}}$ percentiles have 5 and 42 percent, respectively. Thus, workers with at most a high school degree may still work in occupations with above median occupational skill. On the other hand, workers with a college degree are unlikely to work in occupations at the bottom of the skill distribution.

We estimate heterogeneous effects on the employment share and wages of U.S.-born workers within these occupational bins, separately for those with a high school degree or less and with more than a high school degree. We calculate the employment share by dividing the total number of workers in each occupational skill bin, education group and CZ-year, by the total number of working-age individuals in the CZ in 2005, and multiply by 100. We calculate wages by restricting the sample to the workers in the relevant occupational skill bin, education group and CZ-year. For individuals who are not currently working, we use the occupation they report working in most recently.

To fix ideas about how job competition and job creation effects may differ by occupational skill, we plot the share of all workers that are LEFB in each occupational skill bin in 2005 (Panel A of Figure (3)). We use a "moving window" approach in which we group occupations in the lowest quartile (0-25) and then change this bin by 5 percentiles (e.g., 5-30, 10-35, etc.). As expected, the share of LEFB is highest at the bottom of the distribution. These occupations include chefs and cooks, construction laborers, agricultural workers, and janitors and building cleaners. Panel B of Figure (3) plots the estimated effects of SC on the employment share of low-educated U.S.-born workers along the same occupational skill distribution. Interestingly, SC had a positive and marginally significant effect on the employment share of low-educated U.S.-born workers at the bottom of the occupational skill distribution, where most undocumented immigrants work. These employment effects are consistent with the hypothesis that the benefits of reduced job competition for these workers

[27] The results are similar if we instead stratify occupations by average wages, or the percent of the occupation with less than a high school degree.

outweigh the costs of reduced job creation. We find little evidence of changes in wages of low-educated U.S.-born workers at the bottom of the occupational skill distribution (Panel D).

The effects of SC on the employment of low-educated U.S.-born become muted and then turn negative as we move up the occupational skill distribution. This is consistent with the idea that low-educated workers in higher skilled occupations are less likely to benefit from the job competition effect, likely because they operate as complements in production, so the job creation mechanism dominates. This is supported by the fact that the occupations where we find the most negative effects on low-educated U.S.-born workers can be described as supervisory of occupations with many undocumented immigrants, such as first-line supervisors of construction and production, construction managers, farmers and ranchers, and food service and lodging managers. Similarly, we find negative effects on low-educated U.S.-born wages at the upper end of the occupational skill distribution. These findings are similar to Dustmann et al. (2012) who showed that immigration inflows reduce the wages of native workers in the bottom 20th percentile of the wage distribution, where most migrant workers are located, while increasing the wages of natives in the upper part of the wage distribution.

Panel C of Figure (3) plots the estimated effect on employment along the occupational skill distribution for U.S.-born workers with more than a high-school degree. There is little evidence that SC impacted the employment share of high-educated U.S.-born workers at the bottom of the occupational skill distribution, which likely reflects their low share of employment in these occupations. However, consistent with a decrease in job creation, SC lowered the employment share of high-educated workers in the upper half of the occupational skill distribution, who likely act as complements in production. There is little evidence of significant changes in wages for high-educated workers (Panel E).[28]

[28] The results at the bottom of the occupational skill distribution for high-educated U.S.-born are noisy, likely because few high-educated individuals work in these occupations.

6.2 Local Consumption

The second mechanism through which SC can impact the labor market outcomes of U.S.-born workers is reduced consumption for local goods. If this channel is an important driver of our overall results, we expect the negative effects of SC on U.S.-born individuals to be larger in non-tradable industries. Since tradable goods are consumed largely outside of the local labor market, the overall level of demand in these sectors–and therefore the prices of these goods–will be largely unaffected by local demand shocks. The demand for non-tradable goods, on the other hand, is likely to go down in response to SC, which will impact prices and lead to a larger decline in these industries' demand for labor (Burstein et al., 2020).

To examine this mechanism, we analyze the effect of SC separately for tradable and non-tradable industries and report these results in Table (10).[29] Consistent with the hypothesis of a decline in local demand, we find a large, negative, and statically significant effect on employment of U.S.-born workers within non-tradable industries (Column (1) of Panel B). The estimate indicates a 1.02 percent decline in the employment share, which is larger than the 0.55 percent decline across all industries (Table (4)). In the tradable sector, we find no evidence of an overall negative effect on employment (Column (1) of Panel A). We find little difference in the effects of SC on the wages of U.S.-born workers across tradable and non-tradable industries (Column 2). Although these results suggest a potential role for the consumption channel, our reduced form estimates cannot distinguish its relative importance compared to the job competition and job creation mechanism.

7 Conclusion

Secure Communities, one of the largest federal immigration enforcement policies over the last decade, resulted in the deportation of almost half a million individuals during 2008-2014.

[29] We classify industries as tradable or non-tradable following Burstein et al. (2020). Tradable industries include agriculture and manufacturing, and non-tradable sectors include construction, retail, wholesale, and non-tradable services such as haircuts.

This paper makes an important contribution to the immigration literature by estimating the effects of this policy on the employment and wages of both likely undocumented immigrants and U.S. born individuals.

We find that SC led to a significant decrease in the employment share and wages of likely undocumented immigrants. Importantly, the results indicate that SC did not affect their labor market outcomes only through physical removals, but also by reducing the employment and wages of those who remained in the U.S.

The results further indicate that SC did not improve the overall employment or wages of U.S.-born workers. In fact, we find that SC reduced the overall employment share and wages of U.S.-born workers. We propose and test two potential mechanisms that may explain these results. First, any benefits from reduced job competition that U.S.-born workers face may be counteracted by a decline in labor demand due to an increase in labor costs when there are fewer undocumented immigrants in the labor force. Second, the reduction in the population and earnings of likely undocumented immigrants may reduce local consumption, which would also reduce labor demand for U.S.-born workers. Our results suggest that both mechanisms may be potentially important.

Based on our findings, police-based enforcement policies aimed at reducing the number of undocumented immigrants should consider the potential negative spillover effects on the labor market outcomes of immigrants who remain in the U.S. and on U.S.-born workers. The fiscal costs of operating SC, including the costs of detentions, were roughly $1 billion per year (*Possible solutions for immigrant inmate screenings*, 2008) and our results provide important evidence about additional costs due to these negative spillover effects on U.S.-born workers.

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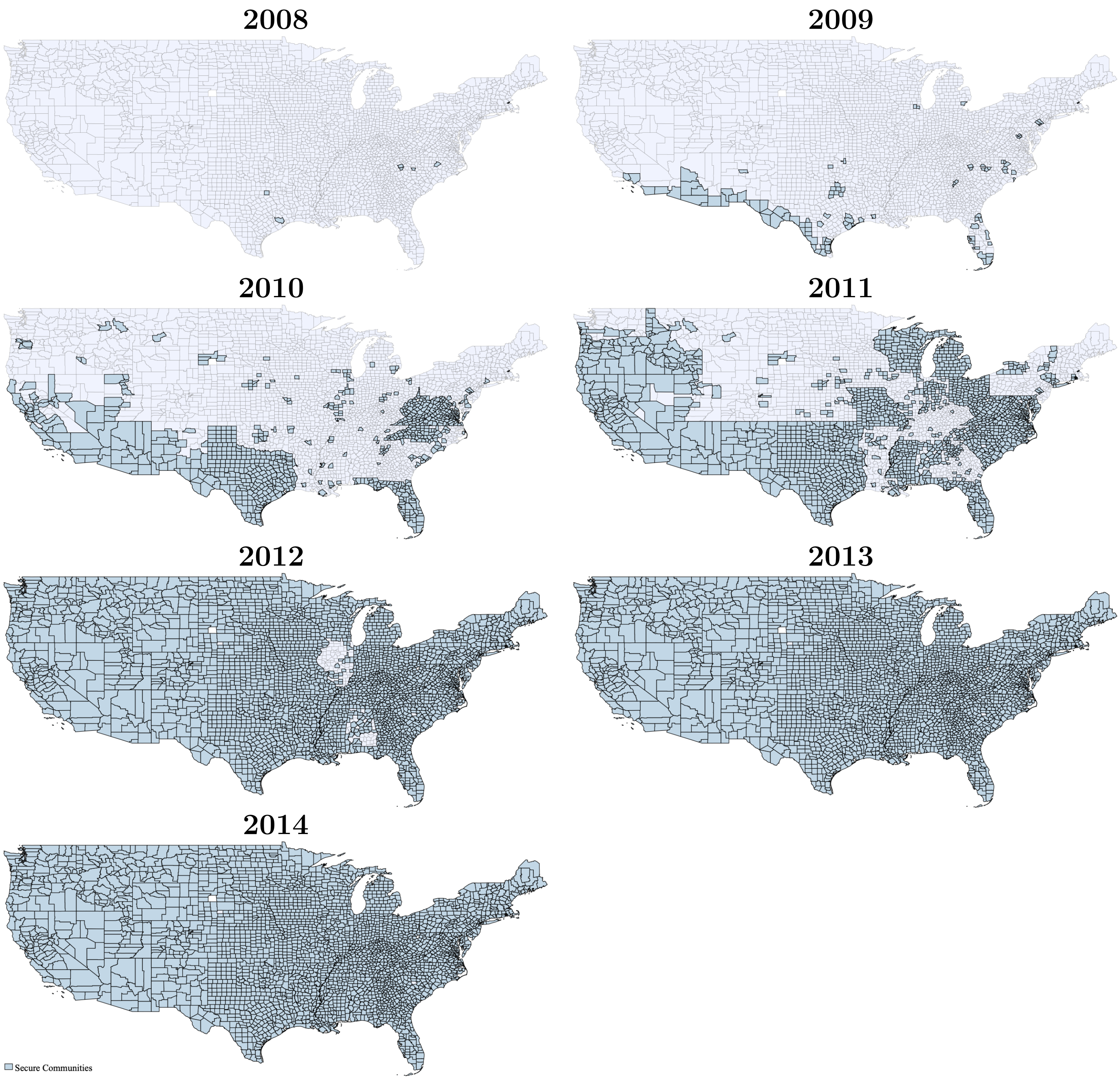
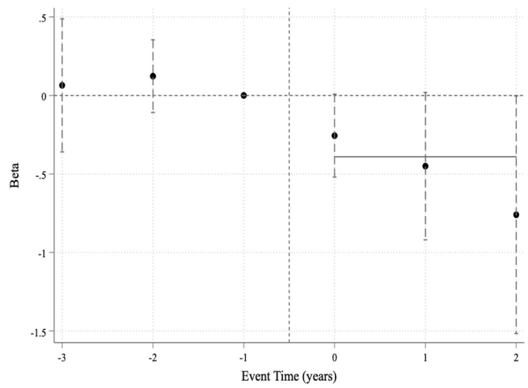
8 Figures

Figure 1: Rollout of Secure Communities Across Counties, by Year

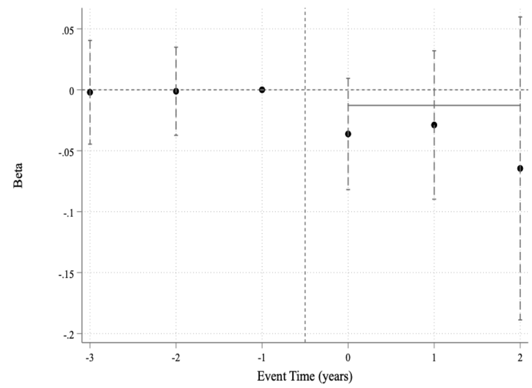


Notes: Counties that adopted Secure Communities during the calendar year are shaded. Rollout dates were collected from the Department of Homeland Security as described in the text.

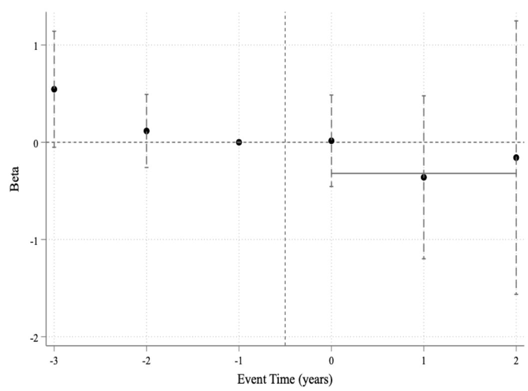
Figure 2: Distributed Lag Models; Effects of SC on the Employment Share and Wages of Low-Educated Foreign-Born and U.S.-Born



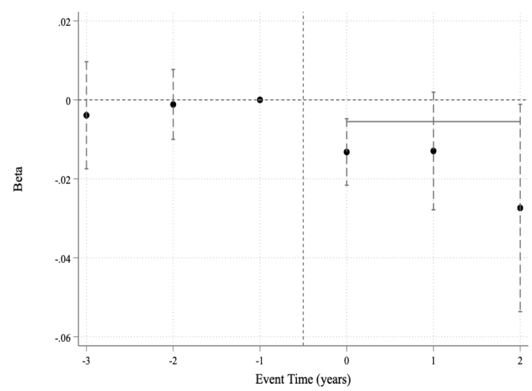
Panel A: Low-Educated Foreign-Born Employment Share



Panel B: Low-Educated Foreign-Born Wages



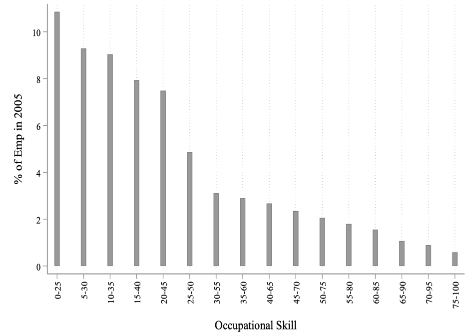
Panel C: U.S.-Born Employment Share



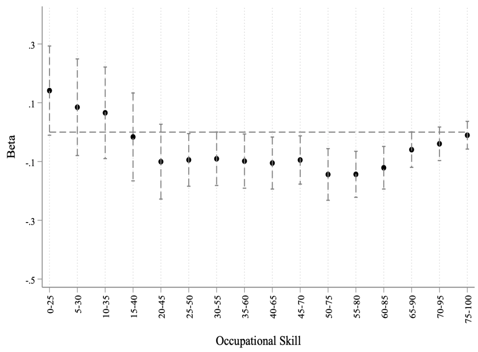
Panel D: U.S.-Born Wages

Notes: Data are drawn from the 2005-2014 American Community Survey. The samples are based on working-age (20-64) low-educated (high school or less) foreign-born individuals and U.S.-born individuals. In Panels A and C, the employment share is measured as total employment in the demographic group by CZ and year, divided by total working-age CZ Population in 2005, and then multiplied by 100. In Panels B and D, hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers. Distributed lag model γ_k coefficients are estimated using equation (2) and then transformed into total treatment effects ($\beta_k s$), which are plotted in the solid black dots. The 95% confidence intervals of the $\beta_k s$ are shown in the black dashed lines. The models include CZ fixed effects, year fixed effects, CZ-specific linear trends, and Bartik-style controls. The gray flat line in the post-SC period corresponds to the difference-in-difference estimates reported in Tables (3) and (4). All results are weighted using the CZ population in 2000 and all standard errors are clustered by CZ.

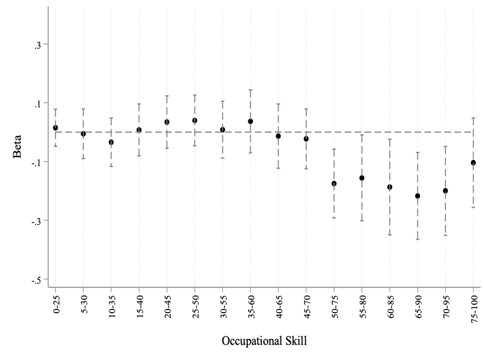
Figure 3: Effects of SC on the Employment Share and Wages of U.S.-Born by Occupational Skill



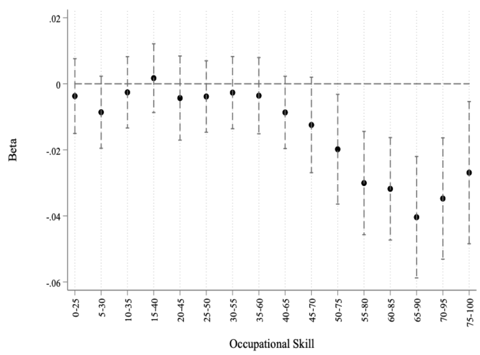
Panel A: Share of Workers that are Low-Educated Foreign-Born in 2005



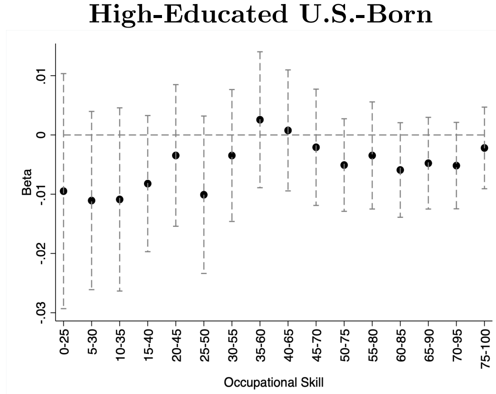
Panel B: Employment Share of Low-Educated U.S.-Born



Panel C: Employment Share of High-Educated U.S.-Born



Panel D: Hourly Wages of Low-Educated U.S.-Born



Panel E: Hourly Wages of High-Educated U.S.-Born

Notes: Data are drawn from the 2005-2014 American Community Survey. The horizontal axis denotes occupational skill bins, starting from the lowest skill group (0-25 percentiles) on the left, and increasing by 5 percentiles in each bin moving to the right (e.g. 5-30, 10-35, etc). Panel A plots the share of all workers in 2005 in the occupational skill group that are low-educated (high school degree or less) foreign-born. Panels B and D include a sample of low-educated (high school degree or less) U.S.-born individuals aged 20-64. Panels C and E include a sample of high-educated (more than a high school degree) U.S.-born individuals aged 20-64. The employment share by occupational skill, in Panels B and C, is calculated by dividing the number of workers in the demographic group, occupational skill group, CZ and year, by the total working-age CZ population in 2005, and then multiplying by 100. For individuals working, we use their current occupation, and for individuals not currently working, we use the most recent occupation they worked in. For wages, in Panels D and E, we calculate the log of the hourly wages of workers in each occupational skill group. All models in Panels B-E include CZ fixed effects, year fixed effects, CZ linear trends, and Bartik-style controls. These results in Panels B-E are weighted using the CZ population in 2000 and standard errors are clustered by CZ. The 95% confidence intervals are shown in the dashed lines in Panels B-E.

9 Tables

Table 1: Most Serious Criminal Conviction and Basic Demographic Characteristics of Deportees under SC, 2008-2014

	Share of All Deportees (percent)
Most Serious Criminal Conviction	
None	20.63
All Violent	18.54
All Non-Violent	60.83
DUI	10.94
Traffic	7.01
Immigration	5.46
Marijuana	2.38
Sex	
Male	95.61
Country of Citizenship	
Latin America	92.22

Notes: Information on deportees is drawn from the individual listings of all deportations under SC from Transactional Records Access Clearinghouse (TRAC) 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 2: Correlation between the CZ Adoption Year of SC and Changes in CZ Characteristics in 2005-2007

	Mean of Characteristic	St. Dev. of Characteristic	Regression Estimate
Change % Non-Citizen	0.06	0.21	0.413
			(0.269)
Change % Male Non-Citizen	0.09	0.32	0.359*
			(0.214)
Change % Low-Edu Male Non-Cit	0.13	0.57	0.073
			(0.116)
Change % Hisp Low-Edu Male Non-Cit	0.27	1.90	0.021
			(0.026)
Change Task 287(g)	0.01	0.09	0.339
			(0.656)
Change Jail 287(g)	0.04	0.14	-2.189***
			(0.513)
Change U.S.-Born Bartik	49.49	78.48	-0.027
			(0.487)
Change Foreign-Born Bartik	44.01	70.57	0.002
			(0.020)
Change Low-Edu Bartik	44.39	69.90	-0.006
			(0.031)
Change High-Edu Bartik	49.55	77.87	0.032
			(0.029)
Mean Y			2010.10
R-Squared			0.14
N			740

Notes: Data on CZ Characteristics are drawn from the 2005-2007 American Community Survey. See section 2 and Appendix A.1 for more detailed descriptions of these variables. We estimate the following regression: $year_j = \alpha + \theta \Delta W_j' + \epsilon_j$ where $year_j$ is the year SC was implemented in the CZ j. $\Delta W_j'$ includes changes in CZ-level demographics, economic conditions, and 287(g) agreements between 2005 and 2007. The regressions are weighted by the CZ population in 2000. * p<0.10, ** p<0.05, *** p<0.01

Table 3: Effects of Secure Communities on the Employment Share and Log Hourly Wages of Low-Educated Foreign-Born

	(Total Group Emp/ Total CZ Pop in 2005)*100			**Log Hourly Wages**		
	All	Females	Males	All	Females	Males
A: Baseline						
β: SC	-0.402***	-0.063	-0.339***	-0.012	-0.008	-0.015
	(0.114)	(0.041)	(0.085)	(0.009)	(0.015)	(0.012)
CZ-Year Trends	X	X	X	X	X	X
Bartiks						
Y mean	6.75	2.55	4.21	2.74	2.59	2.81
% Effect	-5.95	-2.46	-8.07	-	-	-
Observations	7400	7400	7400	7383	7331	7367
B: Add Bartiks						
β: SC	-0.390***	-0.062*	-0.329***	-0.013	-0.008	-0.016
	(0.093)	(0.033)	(0.076)	(0.009)	(0.015)	(0.012)
CZ-Year Trends	X	X	X	X	X	X
Bartiks	X	X	X	X	X	X
Y mean	6.75	2.55	4.21	2.74	2.59	2.81
% Effect	-5.78	-2.43	-7.81	-	-	-
Observations	7400	7400	7400	7383	7331	7367

Notes: Data are drawn from the 2005-2014 American Community Survey. The sample of low-educated foreign-born includes working-age (20-64) foreign-born individuals with a high school degree or less. Columns 2 and 3, and 5 and 6 split the sample by sex. In columns 1-3 the employment share is measured as total employment in the demographic group by CZ and year, divided by total working-age CZ Population in 2005, and then multiplied by 100. In columns 4-6, hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers. See section 3 for more details. All models include CZ and year fixed effects and CZ-specific linear trends. Panel B also includes Bartik-style controls for local labor demand. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 4: Effects of Secure Communities on the Employment Share and Log Hourly Wages of U.S.-Born

	(Total Group Emp/ Total CZ Pop in 2005)*100			**Log Hourly Wages**		
	All	Females	Males	All	Females	Males
A: Baseline						
β: SC	-0.348**	-0.121	-0.227**	-0.005*	-0.006	-0.006*
	(0.164)	(0.091)	(0.111)	(0.003)	(0.003)	(0.003)
CZ-Year Trends	X	X	X	X	X	X
Bartiks						
Y mean	58.61	28.39	30.22	3.18	3.05	3.28
% Effect	-0.59	-0.43	-0.75	-	-	-
Observations	7400	7400	7400	7400	7400	7400
B: Add Bartiks						
β: SC	-0.320*	-0.092	-0.228*	-0.006**	-0.006*	-0.006*
	(0.169)	(0.085)	(0.117)	(0.003)	(0.003)	(0.003)
CZ-Year Trends	X	X	X	X	X	X
Bartiks	X	X	X	X	X	X
Y mean	58.61	28.39	30.22	3.18	3.05	3.28
% Effect	-0.55	-0.32	-0.75	-	-	-
Observations	7400	7400	7400	7400	7400	7400

Notes: Data are drawn from the 2005-2014 American Community Survey. The U.S.-born sample includes working-age (20-64) individuals. Columns 2 and 3, and 5 and 6 split the sample by sex. In columns 1-3 the employment share is measured as total employment in the demographic group by CZ and year, divided by total working-age CZ Population in 2005, and then multiplied by 100. In columns 4-6, hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers. See section 3 for more details. All models include CZ and year fixed effects and CZ-specific linear trends. Panel B also includes Bartik-style controls for local labor demand. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 5: Effects of Secure Communities on the Population Share and Employment to Population Ratio of Low-Educated Foreign-Born and U.S.-Born

	Low-Edu Foreign-Born			US-Born
	All	Females	Males	All
A: Effect on Population				
β: SC	-0.119	-0.018	-0.100*	-0.139
	(0.085)	(0.044)	(0.056)	(0.183)
CZ-Year Trends	X	X	X	X
Bartiks	X	X	X	X
Y mean	10.13	4.84	5.29	87.52
% Effect	-1.17	-0.38	-1.90	-0.16
Observations	7400	7400	7400	7400
B: Effect on Employment to Population Ratio				
β: SC	-0.907**	0.056	-1.816***	-0.519***
	(0.444)	(0.513)	(0.629)	(0.175)
CZ-Year Trends	X	X	X	X
Bartiks	X	X	X	X
Y mean	66.12	52.96	77.73	67.02
% Effect	-1.37	0.11	-2.34	-0.77
Observations	7397	7389	7390	7400

Notes: Data are drawn from the 2005-2014 American Community Survey. The sample in columns 1-3 includes working-age (20-64) foreign-born individuals with a high school degree or less, and this sample is split by sex in columns 2-3. The sample in column 4 includes all working-age (20-64) U.S.-born individuals. The population share, which is the outcome in Panel A, is calculated as the group-specific population in CZ j and year t, divided by the total working-age CZ population in 2005, and multiplied by 100 ($\frac{Pop_{jt}}{Pop_{j2005}} * 100$). The employment to population ratio, which is the outcome in Panel B, is measured as the group-specific employment in CZ j and year t, divided by the group-specific CZ population in year t, and multiplied by 100 ($\frac{Emp_{jt}}{Pop_{jt}} * 100$). The models include CZ fixed effects, year fixed effects, CZ-specific linear trends, and Bartik-style controls. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table 6: Effects of Secure Communities on the Employment Share and Log Hourly Wages Using Alternative Measures of Likely Undocumented Immigrants

	Dep. Var: Total Group Emp / Total CZ Pop in 2005 * 100		
	Hisp	Hisp Arrive 80+	Mx/CA Arrive 80+
β: SC	-0.266***	-0.229***	-0.208***
	(0.079)	(0.071)	(0.075)
CZ-Year Trends	X	X	X
Bartiks	X	X	X
Y mean	3.05	2.52	2.26
% Effect	-8.74	-9.10	-9.20
Observations	7400	7400	7400
	Dep. Var: Log Hourly Wages		
	Hisp	Hisp Arrive 80+	Mx/CA Arrive 80+
β: SC	-0.013	-0.001	-0.010
	(0.009)	(0.016)	(0.017)
CZ-Year Trends	X	X	X
Bartiks	X	X	X
Y mean	2.74	2.57	2.56
Observations	7383	7153	7108

Notes: Data are drawn from the 2005-2014 American Community Survey. The employment share in the top panel is measured as total employment of the demographic group by CZ and year, divided by total working-age CZ population in 2005, and then multiplied by 100. Hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers and those results are shown in the bottom panel. See section 3 for more details. The first column includes Hispanic foreign-born individuals with a high school degree or less. The second column further restricts the sample in column 1 to include those who arrived in the U.S. after 1980. The third column includes working-age (20-64) foreign-born individuals with a high school degree or less who were born in Mexico or Central America and arrived in the U.S. after 1980. The models include CZ fixed effects, year fixed effects, CZ-specific linear trends, and Bartik-style controls. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 7: Effects of Secure Communities on the Employment Share of Low-Educated Foreign-Born and U.S.-Born: Robustness Tests

	Baseline	Robustness to Dropping CZs			Control for Other Immigration Policies		Control for Economic Conditions	
		Drop Early Adopt	Drop Sanc. Cities	Drop AZ	Control for 287gs	Control for E-Verify	Region*Year FE	Quadratic Trend House Prices & Starts
A: Low-Edu Foreign-Born								
β: SC	-0.390***	-0.261***	-0.390***	-0.318***	-0.374***	-0.371***	-0.303***	-0.393***
	(0.093)	(0.079)	(0.093)	(0.081)	(0.108)	(0.081)	(0.097)	(0.093)
CZ-Year Trends	X	X	X	X	X	X	X	X
Bartiks	X	X	X	X	X	X	X	X
Y mean	6.75	6.49	6.75	6.66	6.75	6.75	6.75	6.77
% Effect	-5.78	-4.02	-5.78	-4.77	-5.53	-5.50	-4.49	-5.81
Observations	7400	6980	7400	7330	7400	7400	7400	6510
B: US-Born								
β: SC	-0.320*	-0.400**	-0.320*	-0.364**	-0.344*	-0.308*	-0.358*	-0.319*
	(0.169)	(0.198)	(0.169)	(0.178)	(0.177)	(0.167)	(0.193)	(0.170)
CZ-Year Trends	X	X	X	X	X	X	X	X
Bartiks	X	X	X	X	X	X	X	X
Y mean	58.61	59.07	58.61	58.69	58.61	58.61	58.61	58.60
% Effect	-0.55	-0.68	-0.55	-0.62	-0.59	-0.52	-0.61	-0.54
Observations	7400	6980	7400	7330	7400	7400	7400	6510

Notes: Data are drawn from the 2005-2014 American Community Survey. Panel A reports the effects on working-age foreign-born individuals with a high school degree or less. Panel B reports the effects on working-age U.S.-born individuals. The employment share is measured as total employment in the demographic group by CZ and year, divided by total working-age CZ population in 2005, and then multiplied by 100. Column 2 drops CZs that adopted SC before 2010. Column 3 drops CZs that adopted sanctuary city policies before the adoption of SC. Column 4 drops CZs in Arizona. Columns 5-6 add controls for state and local 287(g) agreements and E-Verify policies, respectively. Column 7 adds controls for region by year fixed effects using Census divisions, see footnote 23 for details. Finally, column 8 adds quadratic trends multiplied by the pre-period change in housing prices and housing starts. The models include CZ fixed effects, year fixed effects, CZ-specific linear trends, and Bartik-style controls. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 8: Effects of Secure Communities on the Hourly Wage of Low-Educated Foreign-Born and U.S.-Born: Robustness Tests

	Baseline	Robustness to Dropping CZs			Control for Other Immigration Policies		Control for Economic Conditions	
		Drop Early Adopt	Drop Sanc. Cities	Drop AZ	Control for 287gs	Control for E-Verify	Region*Year FE	Quadratic Trend House Prices & Starts
A: All Low-Edu Foreign-Born								
β: SC	-0.013	-0.018*	-0.013	-0.014	-0.015	-0.014	-0.019	-0.014
	(0.009)	(0.010)	(0.009)	(0.009)	(0.010)	(0.009)	(.)	(0.009)
CZ-Year Trends	X	X	X	X	X	X	X	X
Bartiks	X	X	X	X	X	X	X	X
Y mean	2.74	2.74	2.74	2.74	2.74	2.74	2.74	2.74
Observations	7383	6963	7383	7313	7383	7383	7383	6495
B: All US-Born								
β: SC	-0.006**	-0.006**	-0.006**	-0.007**	-0.006***	-0.006**	-0.005**	-0.006**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
CZ-Year Trends	X	X	X	X	X	X	X	X
Bartiks	X	X	X	X	X	X	X	X
Y mean	3.18	3.18	3.18	3.18	3.18	3.18	3.18	3.18
Observations	7400	6980	7400	7330	7400	7400	7400	6510

Notes: Data are drawn from the 2005-2014 American Community Survey. Panel A reports the effects on working-age foreign-born individuals with a high school degree or less. Panel B reports the effects on working-age U.S.-born individuals. Hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers. Column 2 drops CZs that adopted SC before 2010. Column 3 drops CZs that adopted sanctuary city policies before the adoption of SC. Column 4 drops CZs in Arizona. Columns 5-6 add controls for state and local 287(g) agreements and E-Verify policies, respectively. Column 7 adds controls for region by year fixed effects using Census divisions, see footnote 23 for details. Finally, column 8 adds quadratic trends multiplied by the pre-period change in housing prices and housing starts. The models include CZ fixed effects, year fixed effects, CZ-specific linear trends, and Bartik-style controls. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table 9: Effects of Secure Communities on the Employment Share and Log Hourly Wages of U.S.-born, by Education

	Dep. Var: Total Group Emp / Total CZ Pop in 2005 * 100	
	High School or Less	More than High School
β: SC	-0.107	-0.213*
	(0.121)	(0.116)
CZ-Year Trends	X	X
Bartiks	X	X
Y mean	23.59	35.02
% Effect	-0.45	-0.61
Observations	7400	7400
	Dep. Var: US-Born Log Hourly Wages	
	High School or Less	More than High School
β: SC	-0.012***	-0.004
	(0.003)	(0.003)
CZ-Year Trends	X	X
Bartiks	X	X
Y mean	2.94	3.31
Observations	7400	7400

Notes: Data are drawn from the 2005-2014 American Community Survey. The U.S.-born sample includes working-age (20-64) individuals. In the top panel, the employment share is measured as total employment in the demographic group by CZ and year, divided by total working-age CZ population in 2005, and then multiplied by 100. In the bottom panel, hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers. See section 3 for more details. All models include CZ and year fixed effects, CZ-specific linear trends, and Bartik-style controls. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table 10: Effects of Secure Communities on the Employment Share and Log Hourly Wages of U.S.-born, by Tradability

	Employment Share	Log Hourly Wages
A: Tradable		
β: SC	0.126	-0.008**
	(0.098)	(0.004)
CZ-Year Trends	X	X
Bartiks	X	X
Y mean	15.04	4.53
% Effect	0.84	-
Observations	7400	7400
B: Non-Tradable		
β: SC	-0.445***	-0.006
	(0.126)	(0.004)
CZ-Year Trends	X	X
Bartiks	X	X
Y mean	43.57	4.53
% Effect	-1.02	-
Observations	7400	7400

Notes: Data are drawn from the 2005-2014 American Community Survey. The U.S.-born sample includes working-age (20-64) individuals. In the first column, the employment share is measured as total employment in the demographic group by CZ and year, divided by total working-age CZ population in 2005, and then multiplied by 100. In the second column, hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers. See section 3 for more details. We stratify the sample by whether the individual works in a tradable or non-tradable industry (Panels A and B, respectively). For individuals not currently worked we use the most recent industry they worked in. We classify industries as tradable or non-tradable following Burstein et al. (2020). Tradable industries include, for example, agriculture and manufacturing, and non-tradable sectors include, for example, construction, retail, wholesale, and non-tradable services such as haircuts. All models include CZ and year fixed effects, CZ-specific linear trends, and Bartik-style controls. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Appendix For Online Publication

A Data Description

A.1 CZ-Year Control Variables

A.1.1 Bartik-Style Measures of Local Labor Demand

The Bartik measures predict labor demand for each group by taking advantage of the fact that different demographic groups work in different industries, and that this varies across local areas. This is done with a shift-share approach as follows:

$$Bartik_{jt} = \sum_{i=1}^{20} \frac{emp_{ij2005}}{emp_{j2005}} * emp_{it} * \frac{1}{100,000} \tag{3}$$

We use the 2005 ACS to create the "share" measures ($\frac{emp_{ij2005}}{emp_{j2005}}$); this is the share of group-specific CZ j employment in 2005 that is in industry i. The reason we use the 2005 ACS instead of, say the 2000 Census, is that the geographic codes are different in the Census, so this allows us to create variables that match the geography in our main analysis. We use 20 large industry groups for these calculations. We calculate the national shift measures (emp_{it}) – total number of workers in industry i and year t – with the 2005-2014 ACS. We divide by 100,000 to make the presentation of the results easier.

A.1.2 Housing Price and Start Information

The housing price information used in the trend control in Tables (7) and (8) comes from the Federal Housing Finance Agency and is available at the county by year level. Similarly, the housing permit ("start") information is available from the U.S. Census Bureau at the county by year level. We aggregate both housing data sets up to the CZ level using the weighting process as described in the main text for the SC variable.

A.1.3 287(g) Agreements

We also include controls for the presence of local-level 287(g) agreements in Tables (7) and (8). 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. There were three types of 287(g) agreements, and we control for the types separately. 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 like SC. A third "Hybrid" model includes both the Task Force and Jail models.[30] In 2012, due to criticism about the efficacy of the program, including issues of racial profiling, Task Force agreements were no longer renewed. 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.

A.1.4 E-Verify

We control for the presence of state-level E-Verify policies in Tables (7) and (8). E-Verify is an employment-based immigration enforcement program which required some employers to verify eligibility for work in the U.S. of job applicants through a federal database. Some states only required government employers and government-contracted employers to do this, whereas other states required all private employers over a given size to do so. We control for the presence of each type of E-Verify program (public only vs. all) separately. Since many CZs span multiple states, we take the population-weighted average value of the E-Verify variables across states within a CZ-year.

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

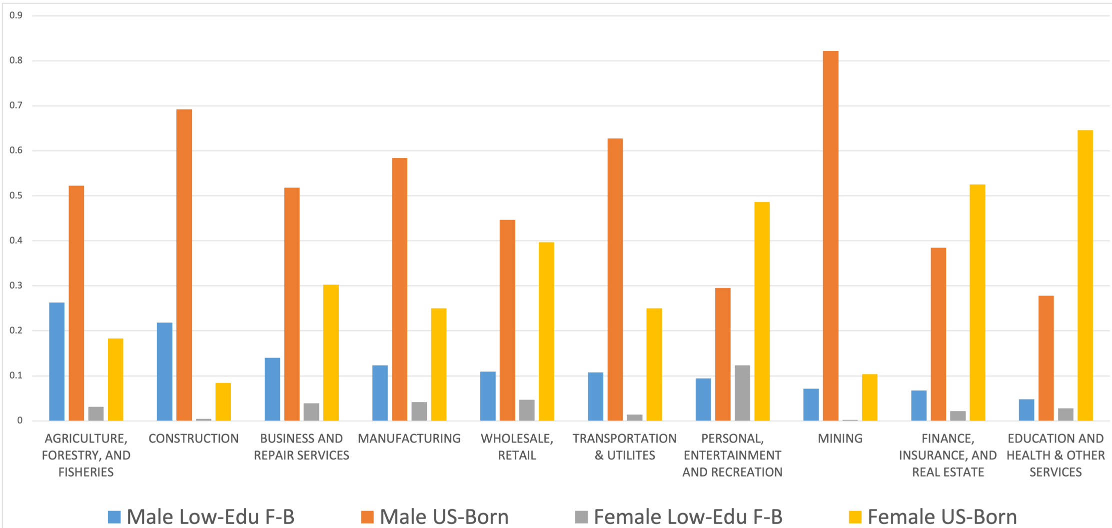
A.2 TRAC Data Description

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.[31] 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 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.

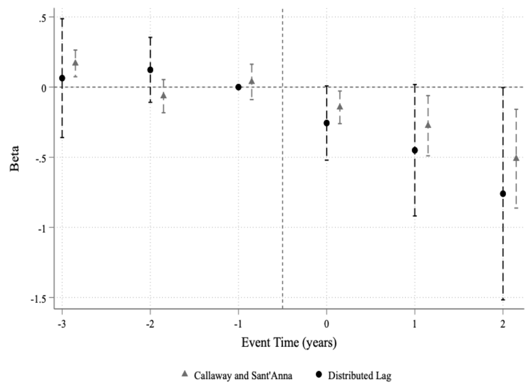
[31] The data also contain information about deportations under PEP, which replaced SC in 2014, as well as under the restoration of SC after January 2017, but we focus our attention on the first period SC was in place: 2008-2014.

Figure A1: Share of Sector Workers in 2005 that are Low-Educated Foreign-Born and U.S.-Born, Split by Sex

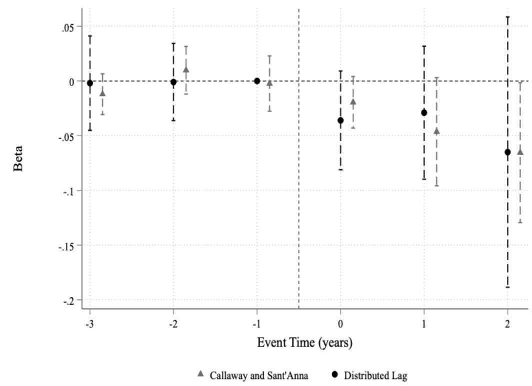


Notes: Data are drawn from the 2005 American Community Survey. The sample includes all working-age individuals who report an industry of current or recent employment. The bars show the share of total sector employment made up by each demographic group: male low-educated foreign-born, male U.S.-born, female low-educated foreign-born, and female U.S.-born, respectively. The results are weighted using the ACS-provided person weights.

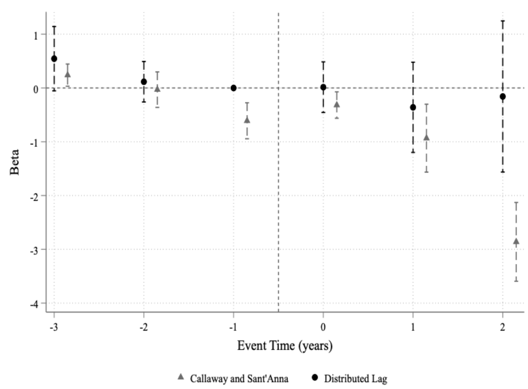
Figure A2: Effects of SC on the Employment Share and Wages of Low-Educated Foreign-Born and U.S.-Born, using Callaway and Sant'Anna (2020)



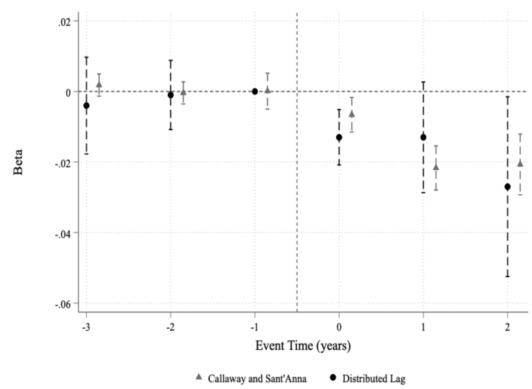
Panel A: Low-Educated Foreign-Born Employment Share



Panel B: Low-Educated Foreign-Born Wages



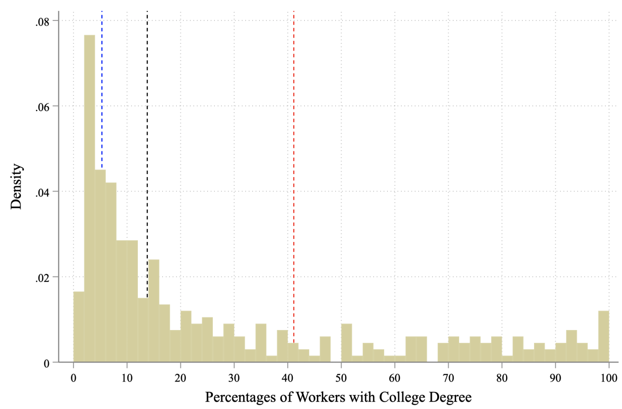
Panel C: U.S.-Born Employment Share



Panel D: U.S.-Born Wages

Notes: Data are drawn from the 2005-2014 American Community Survey. The samples are based on working-age (20-64) low-educated (high school or less) foreign-born individuals and U.S.-born individuals. In Panels A and C, the employment share is measured as total employment in the demographic group by CZ and year, divided by total working-age CZ Population in 2005, and then multiplied by 100. In Panels B and D, hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers. Distributed lag model γ_k coefficients are estimated using equation (2) and then transformed into total treatment effects ($\beta_k s$), which are plotted in the solid black dots. The 95% confidence intervals of the $\beta_k s$ are shown in the black dashed lines. The distributed lag models include CZ fixed effects, year fixed effects, and CZ-specific linear trends. The Callaway and Sant'Anna estimates are based on de-trended data, and are plotted with gray triangles. The corresponding 95% confidence intervals are shown by the gray dashed lines. All results are weighted using the CZ population in 2000 and all standard errors are clustered by CZ.

Figure A3: Distribution of Workers with a College Degree or More Across Occupations



Notes: The figure plots the density of our occupational skill measure–the fraction of workers in each occupation with at least a college degree. This is calculated using the 2005 American Community Survey. The black bar indicates the occupation with the median skill (13.78 percent of workers in this median skill have a college degree), and the blue and red bars depict the 25th and 75th percentile skill occupations, respectively (5.30 and 41.13 percent of workers in these skills have a college degree, respectively).

Table A1: Summary Statistics

Employment Share of Low-Educated Foreign-Born	
Low-Educated Foreign-Born (LEFB)	6.75
LEFB Female Individuals	2.55
LEFB Male Individuals	4.21
Employment Share of U.S.-Born	
All	58.61
Female Individuals	28.39
Male Individuals	30.22
Hourly Wages of Low-Educated Foreign-Born	
Low-Educated Foreign-Born (LEFB)	15.60
LEFB Female Individuals	13.60
LEFB Male Individuals	16.79
Hourly Wages of U.S.-Born	
All	24.39
Female Individuals	21.51
Male Individuals	26.91

Notes: Data are drawn from the 2005-2014 American Community Survey. The low-educated foreign-born sample includes working-age (20-64) individuals with a high school degree or less. The U.S.-born sample includes working-age (20-64) individuals. The employment share is measured as total employment in the demographic group by CZ and year, divided by total working-age CZ Population in 2005, and then multiplied by 100. Hourly wages are calculated by dividing annual earnings by annual hours worked among full-time workers. See section 3 for more details. Summary statistics are weighted by the CZ population in 2000.

Table A2: Distributed Lag Model Coefficients

	Dep. Var: Total Group Emp / Total CZ Pop in 2005 * 100		Dep. Var: Log Hourly Wages	
	Low-Edu Foreign-Born	US-Born	Low-Edu Foreign-Born	US-Born
-3	0.064	0.545	-0.002	-0.004
	(0.216)	(0.305)	(0.022)	(0.007)
-2	0.123	0.116	-0.001	-0.001
	(0.118)	(0.192)	(0.018)	(0.005)
-1	omitted	omitted	omitted	omitted
0	-0.256	0.015	-0.036	-0.013
	(0.135)	(0.240)	(0.023)	(0.004)
1	-0.450	-0.360	-0.029	-0.013
	(0.239)	(0.428)	(0.031)	(0.008)
2	-0.760	-0.158	-0.065	-0.027
	(0.386)	(0.717)	(0.063)	(0.013)
CZ-Year Trends	X	X	X	X
Bartiks	X	X	X	X
Y mean	6.71	58.40	2.74	3.19
Observations	5920	5920	5910	5920

Notes: Data are drawn from the 2005-2014 American Community Survey. The samples are based on working-age (20-64) low-educated (high school or less) foreign-born individuals and U.S.-born individuals. The models include CZ fixed effects, year fixed effects, CZ-specific linear trends and Bartik-style controls. Distributed lag model γ_k coefficients are estimated using equation (2) and then transformed into total treatment effects ($\beta_k s$), which are then reported, along with their standard errors, in the table. All results are weighted using the CZ population in 2000 and all standard errors are clustered by CZ. P-values not denoted.

Table A3: Effect of Secure Communities on Detentions

	Dep. Var: Total Detainers / Total CZ Pop in 2005 * 100	
	(1)	(2)
β: SC	0.121***	0.118***
	(0.021)	(0.021)
CZ-Year Trends	X	X
Bartiks		X
Y mean	0.10	0.10
% Effect	125.67	122.84
Observations	7400	7400

Notes: Data are from the 2005-2014 Transactional Records Access Clearinghouse (TRAC). Detentions are measured as total number of detentions in year t, divided by total working-age CZ Population in 2005, and then multiplied by 100. All models include CZ and year fixed effects and CZ-specific linear trends. The second column includes Bartik-style controls for local labor demand. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A4: Effects of Secure Communities on the Unemployment and Labor Force Participation of Low-Educated Foreign-Born and U.S.-Born

	Total Group Unemp / Total Group Pop * 100		Total Group Out of LF / Total Group Pop * 100	
	LEFB	USB	LEFB	USB
β: SC	0.906***	0.363**	-0.272	0.031
	(0.305)	(0.175)	(0.232)	(0.062)
CZ-Year Trends	X	X	X	X
Bartiks	X	X	X	X
Y mean	5.14	5.13	7.99	9.23
% Effect	17.64	7.07	-3.41	0.34
Observations	7397	7400	7397	7400

Notes: Data are from the 2005-2014 American Community Survey. The sample is based on all working-age (20-64) individuals with subgroups denoted in the column headings. The unemployment share is measured as total unemployment in the demographic group by CZ and year, divided by total working-age CZ Population in 2005, and then multiplied by 100. The labor force share is measured as total number of out the labor force in the demographic group by CZ and year, divided by total working-age CZ Population in 2005, and then multiplied by 100. The models include CZ and year fixed effects, CZ-specific linear trends, and Bartik-style controls for labor demand. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01

Table A5: Effects of Secure Communities on Employment Share and Hourly Wages, by Citizenship Status

	Dep. Var: Total Group Emp / Total CZ Pop in 2005 * 100	
	Low-Educated Non-Citizens	Citizens
β: SC	-0.285***	-0.435**
	(0.079)	(0.174)
CZ-Year Trends	X	X
Bartiks	X	X
Y mean	4.37	64.62
% Effect	-6.53	-0.67
Observations	7400	7400
	Dep. Var: US-Born Log Hourly Wages	
	Low-Educated Non-Citizens	Citizens
β: SC	-0.013	-0.006***
	(0.013)	(0.002)
CZ-Year Trends	X	X
Bartiks	X	X
Y mean	2.63	3.18
Observations	7332	7400

Notes: Data are from the 2005-2014 American Community Survey. Column 1 reports the effects on working-age non-citizen individuals with a high school degree or less. Column 2 reports the effects on working-age U.S.-citizen individuals. The models include CZ and year fixed effects, CZ-specific linear trends, and Bartik-style controls for local labor demand. The results are weighted using the CZ population in 2000. Standard errors are clustered by CZ and are reported in parentheses. * p<0.10, ** p<0.05, *** p<0.01