Immigration Enforcement, Entrepreneurship, and Firm Dynamics*

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Abstract

We analyze whether reducing the undocumented immigrant population affects the local business dynamics and the entrepreneurial climate by leveraging the temporal and spatial variation in the implementation of the Secure Communities (SC) program. SC relies on data-sharing between local law enforcement agencies to identify and arrest undocumented immigrants. We find that the SC implementation at the commuting zone level reduced the number of establishments and establishment entries, and increased establishment exits in the construction sector, along with a decrease in job creation. As expected, we find no effect on economic sectors with a traditionally low percentage of immigrant workers. Surprisingly, we also find no significant effects in the agricultural sector. We are currently working on testing four potential mechanisms to explain the effects in the construction sector, which we call the *entrepreneurial drain effect*, the *chilling effect*, the *labor cost effect*, and the *consumption effect* respectively.

Keywords: Immigration policy, firm dynamics, entrepreneurship, labor markets

JEL Codes: F22, K37, L26, M13, O30

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1 Introduction

The trend of undocumented immigrants[1] in the United States has been fluctuating over the last few decades. According to data from the Pew Research Center, the number of undocumented immigrants peaked at around 12.2 million in 2007, which was four percent of the U.S. population and they accounted for around 5.3 percent of the U.S. labor force. The numbers have since been on a decreasing trend (Krogstad et al., 2017). In 2016, the last year with available data, almost 4.8 percent of the total U.S. workforce was composed of undocumented immigrants. In addition to the Great Recession and changes in economic and demographic conditions in their origin countries, this decline can also be attributed to increased border control and local jurisdiction immigration enforcement programs (Bohn et al., 2014; Caballero et al., 2018).

There is some evidence of the adverse labor market impacts of such negative immigration shock (Kostandini et al., 2014; Bohn and Santillano, 2017; East et al., 2018). However, the impact of these policies on business entries and exits, job creation and destruction, and the general entrepreneurial climate of the country is largely unstudied. If immigration creates more jobs, as shown by the limited recent literature (Azoulay et al., 2022; Kerr and Kerr, 2020; Zelekha, 2013), a restriction on immigration should curb them. Nonetheless, whether this symmetric relationship holds is empirically not straightforward. Many undocumented immigrants fill low-wage jobs that are crucial to certain industries. In addition, immigrants are reported to start firms at higher rates than native-born individuals (Kerr and Kerr, 2020), hence also acting as job creators in the local economy.

In this paper, we causally estimate the effects of a negative immigration shock on entrepreneurship and firm entry/exit by leveraging the temporal and spatial variation in the implementation of the Secure Communities (SC) program. SC is a data-sharing program

[1]Other terms like 'illegal immigrants' or 'illegal aliens' are also widely used to refer to undocumented immigrants. Throughout this paper we use the term 'unauthorized immigrants' that is a more accurate and neutral term that avoids the negative connotations of the word 'illegal'. Moreover, many unauthorized immigrants are not necessarily criminals but rather individuals who have violated civil immigration laws.

that relies on the coordination between federal and local law enforcement agencies to identify and remove undocumented immigrants. The program was rolled out on a county-by-county basis between 2008 and 2013 because the DHS was unable to implement it simultaneously across the U.S. The policy was ultimately adopted by all U.S. counties, and more than 469,000 individuals were removed under SC during our sample period of 2008-2014 (Transactional Records Access Clearinghouse, 2017). More were likely displaced due to the fear of being racially targeted or being asked to present forms of identification (Valdivia, 2019; Kohli et al., 2011). SC was replaced by the Priority Enforcement Program at the end of 2014, before being reactivated in 2017.

Because SC is a federal mandate implemented universally throughout the U.S., there was no self-selection of counties into the program. Moreover, resource bottlenecks, technological constraints, and the sheer scope of the task of communicating between federal and local law enforcement agencies made it impossible not to have a staggered roll-out, and the timing of implementation at the county level was decided by the DHS. Previous studies have also found no evidence that the timing of SC enactment in a local area can be predicted by pre-SC changes in demographic and economic characteristics (East et al., 2018). Considering these, both the adoption and the timing of SC implementation can be thought of as plausibly exogenous. Therefore, using SC as a policy change to account for a negative immigrant labor shock has an empirical advantage over other immigration policies like the Immigration and Nationality Act 287(g), E-Verify, or the Omnibus Immigration Bill, which local jurisdictions could self-select into.

Preliminary results using the 2001-14 data from the Business Dynamics Statistics (BDS) and the County Business Patterns (CBP), both published by the U.S. Census Bureau, show that the SC implementation at the commuting zone (CZ) level reduced establishment entries and increased establishment exits in the construction sector, accompanied by a decrease in job creation. We also find a significant drop in employment and job creation in agriculture. Using the Public Use Microdata Sample (PUMS) from the American Community Survey (ACS) from 2005-14, we are currently working on testing for potential mechanisms.

The mandate affects the formation of new businesses in several distinct ways due to the changes in labor demand and supply. On the labor demand side, first, the policy diminishes the presence of entrepreneurial individuals in the community, especially those from immigrant backgrounds, through either deportation or migration to other counties. In their study in Italy, Anelli et al. (2023) finds that larger emigration rates of Italian citizens between 2008 and 2015 reduced firm creation and innovative start-ups, which was largely driven by the reduction in entrepreneurial individuals. We call this the *entrepreneurial drain effect*. It is important to note that the U.S. law does not prohibit undocumented immigrants from registering a business. Second, even if the entrepreneurial undocumented immigrants do not migrate out of the county, they may not invest in a new business due to the risk of losing the investment when and if they get identified and arrested. We call this the *chilling effect*.

Third, on the labor supply side, industries with a large share of immigrant workers, like agriculture and construction, experience an inward labor supply shift, increasing equilibrium wages. It makes starting new businesses costlier and may potentially shut down existing firms. We call this the *labor cost effect*. Finally, immigrants create the demand for goods and services, which augments the creation of businesses for immigrants and non-immigrants alike. Immigration enforcement, conversely, could discourage firm entries and increase firm exits due to the drop in such demand. We call this the *consumption effect*. We empirically test each of these possibilities.

The economic literature on immigration has often emphasized its role in expanding the labor supply, thus creating competition for native workers in the labor market (Azoulay et al., 2022). However, many studies have shown that positive immigration shocks do not adversely affect native wages and employment (Card, 1990; Peri, 2012). An often neglected factor in immigration discourse is the ability of immigrants to be founders of businesses that increase the labor demand instead of just augmenting the supply as workers (Sant'Anna and Shrestha, 2023).

There is limited literature on the effects of immigration on entrepreneurship, although immigrants are reported to start firms at higher rates than native-born individuals (Kerr and Kerr, 2020). The higher entrepreneurial proclivity of immigrants can broadly be attributed to two factors. First, immigrants, or migrants in general, have lower risk aversion than non-migrants (Jaeger et al., 2010), and since entrepreneurs engage in calculated risk-taking behavior (Hmieleski and Baron, 2009), immigrants are more likely to start businesses. Second, there is evidence of discrimination in hiring in salaried employment against immigrants, especially when they lack host-country-specific social or human capital, which motivates them to open businesses and build a competitive advantage by leveraging their unique and often niche products and services (Zelekha, 2013).

This paper contributes to the literature in several ways. First, it adds to the very scarce literature on the effects of a *negative* immigration shock on entrepreneurship. We focus on a policy that has driven the immigrant population out of the counties. Most of the prior studies focus on the outward shift of the immigrant population that can enhance business growth. However, there is little evidence on how business growth is affected by an inward immigration labor supply shift, which may not be symmetrical. Second, the policy primarily concerns a particular segment of the immigrant population: the unauthorized aliens. It may pose unique challenges to industries that rely heavily on such demographics, thus allowing us to investigate and affirm the extent of dependence of such businesses on their labor supply. Findings will have implications for informing the local government immigration policies.

The remainder of the paper is organized as follows. Section 2 briefly explains the background of this study. Section 3 puts forth the economic model to theoretically motivate the relationship between immigration enforcement and firm creation and retention. Section 4 explains the data. Section 5 describes the empirical strategy. Section 6 shows the results before section 9 concludes.

2 Background

2.1 Undocumented Immigrants in the U.S. Economy

The undocumented immigrant population in the United States rose rapidly in the 1990s from an estimated 3.5 million in 1990 to a peak of 12.2 million in 2007. It then dropped sharply during the Great Recession before stabilizing in 2009. The numbers peaked at around 12.2 million in 2007, which was four percent of the total U.S. population, and has since been on a decreasing trend. The latest estimate for 2017 was more than 11 million individuals. The largest share of unauthorized immigrants (approximately 52 percent) are from Mexico. Other countries with significant numbers include other Central American countries like El Salvador, Guatemala, and Honduras (Passel and Cohn, 2021).

Undocumented immigrants are involved in a wide range of sectors, including agriculture, construction, food service, and domestic work. According to a report by the Migration Policy Institute, approximately 24 percent of undocumented workers are employed in the construction industry, 17 percent work in leisure and hospitality, and 14 percent work in manufacturing. They also make up a significant portion of the workforce in certain industries. For example, a significant portion of farm workers in the U.S. are undocumented Mexican immigrants (Kostandini et al., 2014; Charlton and Kostandini, 2021).

The overall economic impact of undocumented immigrants in the U.S. has been a subject of much debate. Recent research has attempted to measure the effect of illegal immigration on wages and employment in the U.S. labor market. Some studies find that illegal immigration has a negative effect on the wages of U.S.-born workers in low-skilled occupations, while others find no significant effect. For example, a study by Borjas (2017) finds that the large influx of low-skilled immigrants in the U.S. labor market between 1980 and 2000 led to a decline in wages for U.S.-born workers in this category. However, a study by

Peri (2016) finds that there is no significant effect of illegal immigration on the wages of U.S.-born workers in any occupation.

Undocumented immigration also has an impact on the U.S. economy as a whole, including the federal budget and tax revenues. Boeri et al. (2002) finds that illegal immigrants contribute positively to the U.S. economy by increasing the overall size of the economy and providing a source of cheap labor. However, the same study finds that illegal immigrants also impose a cost on the U.S. government in terms of social services and infrastructure, particularly at the state and local level.

2.2 The Secure Communities Program

The passage of the Illegal Immigration Reform and Immigrant Responsibility Act of 1996 (IIRIRA) laid the groundwork for more local immigration enforcement measures in the United States in the ensuing years, including the SC program. SC is an immigration enforcement program administered by ICE from 2008 to 2014 after which it was replaced by the Priority Enforcement Program (PEP), before being reactivated in 2017. The program is aimed at helping ICE arrest and remove individuals who were in violation of federal immigration laws, including those who failed to comply with a final order of removal, or those who had engaged in fraud/willful misrepresentation in connection with government matters. Under SC, county jails submit arrestees' fingerprints not only to criminal databases, but also to immigration databases, allowing ICE access the information on individuals held in jails.

SC was rolled out on a county-by-county basis between October 2007 and January 2013. Resource bottlenecks, technological constraints, and the sheer scope of the task of communicating between federal and local law enforcement agencies made it impossible not to have a staggered roll-out. Because SC is a federal mandate implemented universally throughout the U.S., there was no self-selection of counties into the program. Moreover, the timing of implementation at the county level was decided by the DHS. Previous studies have also found no evidence that the timing of SC enactment in a local area can be predicted by pre-SC changes in demographic and economic characteristics (East et al., 2018).

Considering these factors, both the adoption and the timing of SC implementation can be thought of as plausibly exogenous. This makes the use of SC as a policy change to account for a negative immigrant labor shock empirically advantageous over other immigration policies, which local jurisdictions could self-select into. The staggered nature of the program implementation creates a natural experiment, allowing us to explore the impacts of a negative shock in the immigrant population on various outcomes (Miles and Cox, 2014; Bellows, 2019; Alsan and Yang, 2022; Kang and Song, 2022).

Unlike the Immigration and Nationality Act 287(g) program, no local law-enforcement agents are deputized to enforce immigration laws through Secure Communities. Once Secure Communities is activated in a county, local authorities have no way to share the fingerprints of arrestees with the FBI but not with DHS. Moreover, the program's structure made informal noncompliance with the screening system practically impossible.

Despite ICE stating that SC "prioritizes the removal of criminal aliens, those who pose a threat to public safety, repeat immigration violators," the program has not focused exclusively on convicted criminals, dangerous and violent offenders, or threats to public safety and national security. According to the DHS data, in Fiscal Year (FY) 2011, 26 percent of all SC deportations were immigrants with Level 1 convictions; 19 percent of those deported had Level 2 convictions, and 29 percent were individuals convicted of Level 3 crimes (minor crimes resulting in sentences of less than one year). Twenty-six percent of those deported had immigration violations and no criminal convictions. ICE statistics show that some jurisdictions' numbers for Level 3 and non-criminal deportations are well above the national average. Therefore, SC implementation can be seen as putting non-violent immigrants in a precarious legal position as well.

Moreover, in addition to a large number of removals, SC may have further reduced the

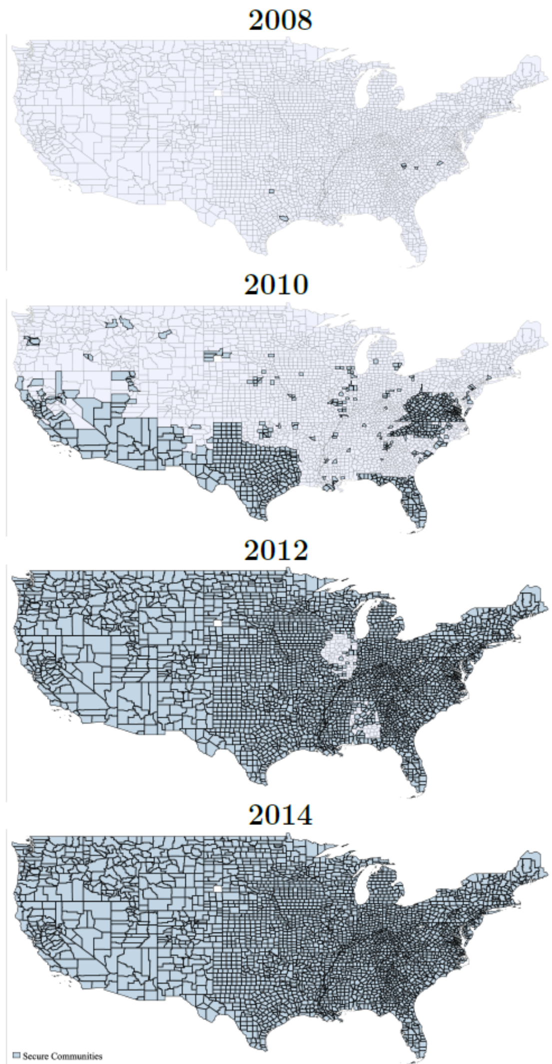

Figure 1: Rollout of Secure Communities across Counties and Year

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 (Valdivia, 2019; Kohli et al., 2011). Evidence shows that other similar police-based immigration enforcement policies, apart from the mechanism of arrests, reduce the immigration population indirectly by increasing the fear of being racially targeted or being deported in immigrant communities (Amuedo-Dorantes and Arenas-Arroyo, 2019).

3 Model (Skip for now; lots to be done here; this is just a snippet)

Immigrants can expand both the labor supply as workers and labor demand as employers. Immigrants also create more demand for final goods and services. We introduce the category of undocumented immigrants and the implementation of localized police-based immigration enforcement in Lucas Jr (1978), subsequently adapted by Azoulay et al. (2022), to theoretically motivate the discussion on the aggregate effects of SC on entrepreneurship.

Let there be N people in the economy, where individuals can choose to either work in a firm as a worker or start a firm as a founder. Each person is endowed with 1 unit of labor. Each person is also endowed with some level of entrepreneurial acumen. For individual i, entrepreneurial acumen, $a_i \geq 0$, which is distributed $f(a)$. The endogenous number of founders is E and workers is L, where $E + L = N$.

Firms maximize profits. They produce with decreasing returns-to-scale technology, and productivity depends on the entrepreneur's skill. These features allow for positive profits and size distribution of firms in equilibrium. Specifically, a firm's output is:

$$y_i = a_i l_i^{\beta} \tag{1}$$

Here, $\beta \in (0, 1)$ and l_i is the labor employed. The profit maximization problem is:

$$\pi_i^* = \underset{l_i}{argmax}[y_i - wl_i] \tag{2}$$

Here, the final good price is taken as numeraire (there is only one type of output).

Individuals choose their careers to maximize income. The individual's choice is to work for a wage, w, or start a firm and earn profit π_i^*. Individuals choose to become entrepreneurs if $\pi_i^* \geq w$ and choose to be workers otherwise. The utility is strictly increasing in the consumption of the final good. Individual consumption is thus equated to their income, and total consumption is equated to GDP.

Finally, we consider three sub-populations, indexed $j \in 0, 1, 2$, to represent the native-born, documented immigrants, and undocumented immigrants respectively. The total population is partitioned as $N = N_0 + N_1 + N_2$ and we similarly partition $L = L_0 + L_1 + L_2$ and $E = E_0 + E_1 + E_2$. The distribution of entrepreneurial acumen for each group is $f_j(a)$. The overall distribution of acumen in the economy is the summation of those two sub-population distributions, each weighted by its population share. The results below will (eventually) specialize to consider Pareto distributions,

$$f_j(a) = \frac{\gamma a_j^\gamma}{a^{\gamma+1}}, \gamma > 0, a \geq a_j \tag{3}$$

Here, the parameter a_j acts as a distributional shifter.

We now provide general statements about equilibrium outcomes in light of immigration enforcement.

To solve for the equilibrium allocation, we have firm-level profit maximization and the individual career decision. These are the choices in the economy. We close the model through the resource constraints, which are the total available population and, most importantly, the immigration enforcement policy and the distributions of entrepreneurial acumen for the native-born and for immigrants of both types.

From profit maximization, profits are strictly increasing in the individual acumen, a_i. The entrepreneurial choice decision then implies a unique threshold value a^*, where individuals choose entrepreneurship if $a_i \geq a^*$ and choose to be workers otherwise. In the absence of immigration enforcement, this threshold level of acumen is given by the following equation for any distribution of talent, $f(a)$.

$$a^* = \frac{w}{\beta}\Big(\frac{\beta}{1-\beta}\Big)^{1-\beta} \tag{4}$$

This equilibrium condition provides a monotonically increasing relationship $a^*(w)$. This produces an upward-sloping labor supply relationship. A higher wage means that more people choose to be workers.

4 Data

4.1 Data on Immigration Enforcement Policy Implementation

We assembled the data on the implementation of immigration enforcement policies from several sources. We obtained the data on the year of SC implementation at the county level from East et al. (2018). Similarly, for other localized immigration enforcement policies that we use as control variables in our estimation strategy, we obtained information on the timing of E-Verify and Omnibus Immigration Bill from Luo and Kostandini (2023) and the timing of the implementation of the Immigration and Nationality Act 287(g) from Kostandini et al. (2014).

4.2 Data on Outcome Variables for the Aggregate CZ Level Analysis

We use the Business Dynamics Statistics (BDS) from the U.S. Census Bureau as the primary data for the aggregate analysis of the impacts on the business dynamics. The BDS tracks businesses over time, providing annual measures of establishment openings and closings, firm startups and shutdowns, and job creation and destruction. The BDS data are compiled from the Longitudinal Business Database (LBD), which is a longitudinal database of business establishments and firms. LBD is constructed by linking annual snapshot files from the Census Bureau's Business Register (BR) to provide a longitudinal history for each establishment. The linkage process allows the tracking of net employment changes at the establishment level, which, in turn, allows the estimation of jobs gained at opening and expanding establishments, and jobs lost at closing and contracting establishments.

The smallest geographical unit that BDS dataset identifies is the county. We concord the county information with CZ using the crosswalk by Tolbert and Sizer (1996). The data is also disaggregated at the industry level using the North American Industry Classification System (NAICS) codes. We conduct economic sector analysis at the level of 2-digit NAICS industry code. However, to maintain privacy, the dataset suppresses information for industry-by-county level data for smaller counties. We explain the magnitude and implications of suppressed outcome variable data in Section A in the Appendix but for the main analysis, we replace the suppressed values by zero. In Section A, we also recheck the robustness of some of our results by only using CZs with no missing values for all counties for the outcome variable. For all our analysis using the BDS dataset, We use the statistics from 2001-14. We do not include the years beyond 2014 in the analysis because ICE suspended SC in early 2015 before reinstating it in 2017.

4.3 Data on Outcome Variables for the Firm Level Analysis

Restricted-access U.S. Census Data (Longitudinal Business Database). Proposal to be sent this summer.

4.4 Data on Outcome Variables for the Individual Level Analysis

To test the mechanisms, we use the American Community Survey (ACS)[2] Integrated Public Use Microdata Sample (IPUMS) dataset. The ACS is an annual cross-sectional survey on worker-level demographics and labor force engagement conducted also by the U.S. Census Bureau. The ACS sample includes about 3.5 million households each year, representing about 1 percent of the U.S. population. The ACS is considered to be a vital tool for

[2] ACS is better equipped for our analysis compared to two other popular national-level datasets, the Quarterly Census of Employment and Wages (QCEW) and the Current Population Survey (CPS). QCEW data only provides information on county-level averages of wages and employment whereas the CPS, although representative of the entire U.S. population, is not representative at the level of smaller geographies. The ACS dataset has neither of these shortcomings.

understanding the characteristics and needs of the American population, particularly in areas that are rapidly changing or experiencing demographic shifts.

Unlike the BDS dataset, we use the ACS dataset from 2005-14 because the Public Use Microdata Areas (PUMAs), the smallest geographical region mentioned in the public-use data, are only available from 2005 onward. Although ACS data does not specifically identify undocumented immigrants, we use socio-demographic variables to create a proxy for undocumented immigrants as in previous studies like Amuedo-Dorantes et al. (2022) and use the self-employment variable as a proxy for entrepreneurship.

5 Methods

We are interested in the relationship between SC implementation and entrepreneurship in the local economy. For the treatment variable, we create the population-weighted share of a CZ that had SC active in a given year. To create this share variable, we use the following formula:

$$SC_{z,t} = \frac{1}{N_{z,2000}} \sum_{c \in z}^{Z} \frac{1}{12} \sum_{j=1}^{12} \mathbb{1}(SC_{c,j}) P_{c,2000} \tag{5}$$

In equation (5), $\mathbb{1}(SC_{c,j})$ is an indicator function that is equal to 1 if SC is active in county $c \in$ CZ z during month j[3] of year t. $P_{c,2000}$ and $N_{z,2000}$ are the population of county c and CZ $z \ni c$ respectively for 2000, the year prior to the rolling of SC. $SC_{z,t} \in [0,1]$ thus denotes the 2000-population-weighted fraction of the CZ z where the SC was active in year t. We approximate local labor markets using the construct of CZs developed by Tolbert and Sizer (1996). CZs are suited to the analysis of local labor markets because they encompass both urban and rural areas, and are based primarily on economic geography rather than incidental factors such as minimum population (Autor et al., 2019). Our analysis includes the 722 CZs across the contiguous United States.

[3]We consider a month as treated if a county implemented the SC on or before the 15th of the month.

Equation (6) highlights our primary baseline reduced form econometric specification. In the equation, $sinh^{-1}(y_{z,t})$ is an inverse hyperbolic sine function[4] of six outcome variables: (a) total employment, (b) the number of establishments, (c) the number of establishment entries, (d) the number of establishment exits, (e) the number of job creations, and (f) the number of job destructions for each CZ z and year t.

$$sinh^{-1}(y_{z,t}) = \alpha + \beta SC_{z,t} + \gamma X_{z,t} + \theta_z + \phi_t + \epsilon_{z,t} \tag{6}$$

In the equation, $X_{z,t}$ is a matrix of seven variables that measure the fraction of the CZ with the seven other respective immigration enforcement policies[5] active at the given month j in year t. We do not control for variables related to population or economic growth proxies because the SC implementation can affect them, making them mediators blocking the mechanism. Likewise, θ_z and ϕ_t are CZ and year fixed effects respectively. The error term, $\epsilon_{z,t}$, denotes robust standard errors.

Since $SC_{z,t} \in [0,1]$ is the population-weighted fraction of the CZ z with SC active in year t, our coefficient of interest, β, can be interpreted as the average treatment effect on the treated (ATT) when a CZ goes from 0 percent to 100 percent of its counties having SC active for all 12 months in a year. Mathematically,

$$\begin{aligned}
\beta &= ATT(y_{z,t}, SC_{z,t}(SC_{c,j})) && (7)\\
&= E\,[\,sinh^{-1}(y_{z,t}) \mid SC_{z,t} = 1\;\forall\, j)\,] - E\,[\,sinh^{-1}(y_{z,t}) \mid SC_{z,t} = 0\;\forall\, j)\,] && (8)\\
&= E\,[\,sinh^{-1}(y_{z,t}) \mid SC_{c,j} = 1\;\forall\, j\;\forall\, c)\,] - E\,[\,sinh^{-1}(y_{z,t}) \mid SC_{c,j} = 0\;\forall\, j\;\forall\, c)\,] && (9)
\end{aligned}$$

The underlying identification assumption is that there were no time-varying CZ-specific

[4]The inverse hyperbolic sine function is implemented because of the presence of zero values in the outcome variables. Read Bellemare and Wichman (2020).

[5]These seven other immigration policies include (1) Immigration and Nationality Act 287(g) at the state level and (2) county level, (3) E-Verify, and variations of the Omnibus Immigration Bill including (4) the "show me your papers" policy, (5) limited public benefits access, (6) limited driving license access, and (7) limited college education access.

factors correlated with the timing of the adoption of SC. To provide support for this assumption, we follow Fuest et al. (2018), Schmidheiny and Siegloch (2019), Suárez Serrato and Zidar (2016), and East et al. (2018) and estimate the following distributed lag model:

$$log(y_{jt}) = \alpha + \sum_{\substack{k=-3 \\ k\neq -1}}^{3} \gamma_k SC_{j,t-k} + X'_{jt}\rho + v_j + \lambda_t + \epsilon_{jt} \tag{10}$$

In the equation, the estimated γ_ks measure the relationship between SC and the change in the outcomes of interest related to establishment count, entries, and exits. 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. We denote the total effects that we plot as β_ks. We set the period $k = -1$ as the reference period by setting $\beta_{-1} = 0$. 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 negatively so that $\beta_0 = \gamma_0$, $\beta_1 = \gamma_0 - \gamma_1$, and so on. If the parallel trends assumption holds, the estimates of β_k where $k < -1$ should be statistically not different from zero.

6 Results

Table 1 shows the association between SC implementation and the (logged) total employment at the CZ level. While there is no significant effects while using the overall sample, we see a significant effect on construction employment. Compared to a CZ with SC not active in any counties, a CZ with SC active in all counties throughout the year experienced the construction sector employment decline by 9.5 percent, which is highly significant.

Surprisingly, we do not see any effects on agricultural employment, although previous studies show that the farm labor was affected by other similar immigration policies like the 287(g) (Kostandini et al., 2014; Ifft and Jodlowski, 2022), E-Verify (Lim and Paik, 2023), and the Legal Arizona Workers' Act (Luo and Kostandini, 2022). One possible reason for SC not affecting agriculture employment could be the exceeding participation of farm employers in the H-2A guest worker Visa program that helps retain the desired number of farm workers. However, we will further disentangle this using the Census of Agriculture (CoA) data soon and also redo the analysis by only using agricultural or rural counties.

We also run our analysis on the 'placebo' group that consists of the subsample of economic sectors that traditionally do not have high percentage of undocumented immigrants. The placebo group in our analysis includes NAICS values 51, 52, 53, 54, and 55, which are, respectively, "information", "finance and insurance", "real estate and rental and leasing", "professional, scientific, and technical services", and "management of companies and enterprises". As expected, we do not see any significant effects of SC implementation on employment in these sectors.

Table 2 shows the association between SC implementation and the (logged) number of establishments at the CZ level. While there is no significant effects while using the overall sample, we see a significant effect on construction establishment count. Compared to a CZ with SC not active in any counties, a CZ with SC active in all counties throughout the year experienced the construction sector establishment count decline by 4 percent, which is highly significant. We do not find significant relationship in case of the agricultural sector but, as expected, see no effects in the placebo sample.

Table 3 shows the association between SC implementation and the (logged) number of establishment entries at the CZ level. While there is no significant effects while using the overall sample, we see a significant effect on construction establishment entries. Compared to a CZ with SC not active in any counties, a CZ with SC active in all counties throughout the year experienced the construction sector employment decline by almost 7 percent, which is significant at 10 percent level. One surprising result in this table is the significant negative effect in the placebo group. SC implementation dropped placebo group establishment entries by 13.7 percent.

Table 4 shows the association between SC implementation and the (logged) number of establishment exits at the CZ level. While using the overall sample, compared to a CZ with SC not active in any counties, a CZ with SC active in all counties throughout the year experienced establishment exits increase by 6 percent, which was primarily driven by the construction sector that experienced similar exit rates.

Table 5 shows the association between SC implementation and the (logged) number of job creations at the CZ level. Compared to a CZ with SC not active in any counties, a CZ with SC active in all counties throughout the year experienced the construction sector job creation decline by almost 13 percent, which is highly significant. Parallel to the above findings, we do not see significant effects in the agricultural sector, while see expected null effects on the placebo sectors.

Finally, Table 6 shows the association between SC implementation and the (logged) number of job destructions at the CZ level. The findings in this table are rather surprising. While the SC implementation did not have significant effects on construction, it negatively affected the overall job destruction, which seems to be driven by the effects in the agricultural sector. However, while redoing the analysis by dropping the agricultural sector (not shown in the table), we still see a negative overall effects on job destruction.

Table 1: Effects of SC on employment

	(1) All Sectors	(2) Construction	(3) Agriculture	(4) Placebo
SC	-0.001	-0.095***	0.067	0.039
	(0.007)	(0.023)	(0.072)	(0.040)
Control variables	Yes	Yes	Yes	Yes
CZ fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	10,108	10,108	10,108	10,108

Standard errors in parentheses.

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

Table 2: Effects of SC on the no. of establishments

	(1) All Sectors	(2) Construction	(3) Agriculture	(4) Placebo
SC	0.001	-0.040***	0.030	0.017
	(0.004)	(0.012)	(0.042)	(0.022)
Control variables	Yes	Yes	Yes	Yes
CZ fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	10,108	10,108	10,108	10,108

Standard errors in parentheses.

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

Table 3: Effects of SC on establishment entries

	(1) All Sectors	(2) Construction	(3) Agriculture	(4) Placebo
SC	-0.005	-0.069*	-0.048	-0.137***
	(0.031)	(0.040)	(0.045)	(0.042)
Control variables	Yes	Yes	Yes	Yes
CZ fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	10,108	10,108	10,108	10,108

Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Effects of SC on establishment exits

	(1) All Sectors	(2) Construction	(3) Agriculture	(4) Placebo
SC	0.064**	0.064*	-0.066	-0.020
	(0.031)	(0.034)	(0.044)	(0.041)
Control variables	Yes	Yes	Yes	Yes
CZ fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	10,108	10,108	10,108	10,108

Standard errors in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Effects of SC on job creation

	(1) All Sectors	(2) Construction	(3) Agriculture	(4) Placebo
SC	-0.009	-0.134***	-0.002	-0.072
	(0.017)	(0.035)	(0.064)	(0.052)
Control variables	Yes	Yes	Yes	Yes
CZ fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	10,108	10,108	10,108	10,108

Standard errors in parentheses.

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

Table 6: Effects of SC on job destruction

	(1) All Sectors	(2) Construction	(3) Agriculture	(4) Placebo
SC	-0.029**	-0.038	-0.181***	-0.020
	(0.014)	(0.028)	(0.057)	(0.051)
Control variables	Yes	Yes	Yes	Yes
CZ fixed effects	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
N	10,108	10,108	10,108	10,108

Standard errors in parentheses.

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

6.1 Effects on the Immigrant Population

6.2 Effects on Firm Creation and Retention

6.3 Effects Based on Firm Size

6.4 Effects on Self-Employment

7 Robustness Checks

7.1 Metropolitan Statistical Area (MSA) Level Analysis

7.2 Removing Counties that Adopted Other Immigration Enforcement Policies

8 Mechanisms (Skip this for now)

$$y_{i,z,t} = \alpha + \beta SC_{z,t} + \gamma X_{i,z,t} + \lambda_i + \phi_t + \epsilon_{i,z,t} \tag{11}$$

8.1 The *Entrepreneurial Drain Effect*: Reduction in Entrepreneurial Immigrant Population

8.2 The *Chilling Effect*: Discouragement in Opening a Business

8.3 The *Labor Cost Effect*: Increase in the Cost to Start and Retain Firms

8.4 The *Consumption Effect*: Decrease in the Demand of Products

9 Conclusion

We used the BDS dataset to explore whether SC implementation affects the local business dynamics and the entrepreneurial climate at the CZ level. Although we see no significant effects while using the aggregate sample, we do find significant negative effects in the construction sector. Surprisingly, we do not find any significant effects in the agricultural sector although previous literature informs that we should be seeing effects consistent with farm worker shortage. Next steps are to use the Census of Agriculture data that is better suited for the agriculture sector data and use only the rural agricultural counties in the sub-analysis. ACS data will also be used to test the mechanisms.

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Appendices

A Note on the Implications of Suppressed Outcome Variable Data (Skip for Now)

As noted in Section 4, to maintain privacy, the BDS dataset suppresses information for industry-by-county level data for smaller counties. The percentage of observations that suffer from this varies between outcome variables (employment 12.71 percent, establishment count 12.71 percent, establishment entry 36.41 percent, establishment exit 36.09 percent, job creation 12.71 percent, and job destruction 12.71 percent).

Case I: Positive entrepreneurship effects in smaller counties

In this case, the sign of the effect is unclear. However, this case is not supported by the economic theory. So, this possibility can be negated.

Case II: Null entrepreneurship effects in smaller counties

In this case, the overall effects might still be negative, but the magnitude of the coefficient will move toward zero. This makes our current results the upper bound estimates in terms of magnitude.

Case III: Negative entrepreneurship effects in smaller counties

In this case, the implications depend on the differential effects on smaller counties in terms of magnitude. If the real effects in smaller counties are higher than the larger counties, then the current estimates will be lower bound. If the real effects are of similar intensity, the current estimates stand as they are. In the real effects are of lesser intensity, then the current estimates will be upper bound.

Redoing the Analysis with CZ Robust Subsample

To check whether the results, specifically for the construction sector, are robust after removing CZs with missing values, we still find estimates with similar direction and magnitude as our previous results.

Table 7: Construction sector outcomes after deleting CZs with missing values

	(1) Employment	(2) Establishments	(3) Establishment Entries	(4) Establishment Exits	(5) Job Creation	(6) Job Destruction
SC	-0.091***	-0.054***	-0.105***	0.000	-0.153***	-0.057
	(0.008)	(0.013)	(0.030)	(0.035)	(0.039)	(0.035)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
CZ fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
N	8,502	9,586	4,898	5,072	9,483	9,482

Standard errors in parentheses.

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