Immigration Enforcement and Local Business Dynamics*

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Abstract

This paper examines the effects of interior immigration enforcement on local business dynamics. Exploiting the federally determined rollout of the Secure Communities (SC) program across U.S. counties, we use county-sector-year Census administrative data to show that enforcement reduces establishment entry and job creation, with no significant effect on exit or job destruction. We provide evidence consistent with a labor supply mechanism: SC activation reduces the immigrant population, raises wages, and generates larger effects in immigrant-intensive and high-turnover sectors. The decline in job creation is driven by continuing establishments adjusting along the intensive margin rather than exiting.

JEL Codes: K37, R23, D22, J21

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1 Introduction

Immigration enforcement has expanded dramatically in the United States since the 1996 Illegal Immigration Reform and Immigrant Responsibility Act, which substantially increased the federal government's capacity to identify, detain, and remove undocumented immigrants. Subsequent decades saw the expansion of interior enforcement programs that put these capacities into practice at scale. A growing empirical literature shows that enforcement reduces the undocumented population in affected areas and generates meaningful changes in local labor markets (Bohn et al., 2014; East et al., 2023; Orrenius and Zavodny, 2015; East and Velásquez, 2024). Yet this evidence is largely focused on worker outcomes such as employment and wages. We know much less about how enforcement-induced labor supply shocks affect the firms that employ these workers, despite the fact that many industries depend heavily on low-skill immigrant labor and that firm-level responses are the primary margin through which labor market disruptions translate into production, employment, and aggregate economic activity (Decker et al., 2014).

This distinction matters because suppressed firm entry and expansion are economically different from layoffs and closures, even when aggregate employment effects appear similar. Unlike layoffs or closures, a slowdown in firm entry and expansion suppresses productive capacity in ways that are gradual and harder to observe in standard employment and wage data (Clementi and Palazzo, 2016; Decker et al., 2016). If enforcement primarily constrains hiring rather than forcing incumbent firms to exit, its economic costs accumulate through foregone growth rather than immediate dislocation, a distinction with direct implications for how policymakers evaluate the full consequences of interior enforcement.

In this paper, we estimate the effects of U.S. interior immigration enforcement on establishment entry and exit using variation in the rollout of Secure Communities (SC) across counties from 2008 to 2012. SC is a federal program that automatically checks the immigration status of individuals booked into local jails against federal databases, enabling the identification of unauthorized immigrants and potential removal. The program's activation schedule was determined federally by ICE based on technical readiness and administrative capacity rather than local adoption decisions (East et al., 2023; Cox and Miles, 2013). This renders the timing of activation plausibly exogenous to local economic conditions, a claim we support empirically by showing that pre-treatment changes in county characteristics do not systematically predict the timing of SC adoption. We combine SC activation with U.S. Census Bureau administrative data in a county-sector-year panel and estimate its effects using the staggered difference-in-differences estimator of Sun and Abraham (2021).

We find that interior immigration enforcement significantly reduces local business dynamism, operating primarily through the entry and hiring margins rather than establishment exit. SC reduces establishment entry by about 12% and job creation by roughly 14%, with no statistically significant effect on exit or job destruction. This asymmetry is central: SC does not force establishments out of business, but instead prevents new establishments from forming and constrains the expansion of existing ones. These patterns are consistent with the broader literature finding that labor supply shocks operate primarily through the inflow margin rather than through incumbent exit (Dustmann et al., 2017; Ortega and Verdugo, 2022). Event study estimates for establishment entry, exit, and job creation show no evidence of pre-trends, with entry and job creation declining sharply following activation. Cumulatively, SC reduces the total stock of establishments by 3.8% and firms by 4.4%. The underlying establishment entry and job creation declines are concentrated among smaller and younger firms.

We interpret these results through a labor supply mechanism and provide multiple pieces of evidence that jointly isolate this channel from alternative explanations. SC operates as a negative local labor supply shock that raises effective hiring costs and constrains firms' ability to recruit workers. When access to immigrant labor declines, firms face a smaller and less flexible workforce, particularly in sectors that rely on continuous inflows of labor. In this environment, firms adjust primarily by reducing hiring and slowing expansion.

This framework yields three testable predictions: (i) adjustment should occur along the hiring and job creation margins rather than through exit, (ii) effects should be concentrated in sectors that rely more heavily on immigrant labor or exhibit high turnover, and (iii) responses should be stronger in locations with greater baseline exposure to immigrant labor.

We provide evidence supporting each of these predictions. First, we document a direct first-stage effect on local labor supply: SC activation reduces the local immigrant population, particularly the non-citizen and Hispanic non-citizen shares, confirming that enforcement contracts the pool of workers most exposed to the policy. Second, consistent with adjustment along the hiring margin, we show that the decline in job creation is driven by reduced expansion among continuing establishments, with no corresponding increase in job destruction. Third, the effects on establishment entry and job creation are sharply concentrated in immigrant-intensive sectors, high-turnover industries, and counties with higher baseline immigrant shares, with negligible effects in low-exposure settings. This spatial and sectoral concentration closely tracks variation in reliance on immigrant labor and is more consistent with immigrant-specific labor supply exposure than with broad macroeconomic demand shocks or general policy uncertainty alone. Finally, we find that SC raises average weekly wages, particularly in immigrant-intensive sectors, providing corroborating evidence of labor market tightening consistent with increased hiring frictions. Together, these results align closely with the predictions of the labor supply mechanism.

We then examine additional margins that amplify the primary labor supply effect. First, we find that self-employment declines not only among non-citizens (-6.4%) but also among U.S. citizens (-2.2%). For non-citizens, this pattern is consistent with both reduced access to co-ethnic labor networks and an increase in the perceived risk of operating a business under heightened enforcement, which may lower the expected returns to entrepreneurship. For citizens, the decline is consistent with the labor supply channel: with fewer immigrant workers available to hire, entrepreneurs face greater difficulty staffing new ventures. Second, the effects on establishment entry and job creation are also stronger and more precisely estimated in non-tradable sectors that depend on local consumption, consistent with a local demand multiplier: reduced immigrant labor supply lowers both the local consumer base and spending, further dampening firm formation and expansion. Rather than competing explanations, these margins reinforce and amplify the primary labor supply mechanism.

The results are robust to a wide range of alternative specifications. Estimates are stable when absorbing commuting-zone-by-year fixed effects and when aggregating to the commuting zone level, indicating that the findings reflect net reductions in local business activity rather than within-market reallocation. We find no evidence that SC predicts differential data suppression, and results are stable across trimmed samples. The findings are also robust to alternative difference-in-differences estimators (Borusyak et al., 2024), alternative clustering schemes, spatial standard errors (Conley, 1999), and unweighted specifications. Across all checks, the estimates remain similar in magnitude and statistical significance.

This paper makes several contributions to the literature. First, it provides broad causal evidence on how a mandatory federal interior enforcement policy affects local business formation and employment dynamics. Prior work on immigration enforcement has focused primarily on worker outcomes: SC reduced employment among likely undocumented immigrants and lowered employment and wages for U.S.-born workers (East et al., 2023), generated broader equilibrium effects including higher prices for household services (East and Velásquez, 2024) and disruptions in immigrant-intensive local markets such as child care (Ali et al., 2024), and reduced labor market participation among immigrant women through network disruption (Bansak et al., 2025). Evidence on 287(g) programs, which permitted local law enforcement agents to perform some of the duties of ICE, documents sectoral employment declines (Bohn and Santillano, 2017), while firms exposed to intensified enforcement increase their reliance on guest worker programs as a substitute labor channel (Amuedo-Dorantes et al., 2021). This body of work establishes that enforcement disrupts labor markets and alters firm behavior. We complement it by using national county-sector business dynamics data to show how a mandatory federal enforcement shock propagates across establishment entry, exit, job creation, and job destruction.

Second, the paper contributes to the literature on how local economies absorb labor supply shocks and how access to immigrant labor shapes firm behavior. A literature on business dynamism shows that labor force growth is a primary driver of firm entry rates, with a shrinking labor pool suppressing startup activity while leaving incumbent dynamics largely intact (Karahan et al., 2019; Hopenhayn et al., 2022), and that immigration inflows raise establishment counts, business survival, and employment (Olney, 2013; Orrenius et al., 2020; Mahajan et al., 2024). Immigrants also account for a disproportionate share of new firm formation and high-growth firms in the United States (Azoulay et al., 2022; Kerr and Kerr, 2020), and firm-level evidence shows that access to immigrant labor raises productivity and growth (Mitaritonna et al., 2017; Beerli et al., 2021), consistent with immigrant and native workers being complements (Peri and Sparber, 2009). This literature implies that restrictions on immigrant labor should suppress firm dynamics, a prediction consistent with evidence that E-Verify mandates reduce formal employment and firm activity (Ayromloo et al., 2020). Our findings extend this mechanism to mandatory federal interior enforcement, highlighting that reductions in immigrant labor supply primarily suppress firm formation and expansion, shifting the costs of enforcement toward foregone growth.

Third, this paper contributes to the literature on declining U.S. business dynamism. A large body of work documents secular declines in firm entry rates, job reallocation, and the share of activity accounted for by young firms since the 1980s, with the decline becoming especially pronounced after 2000 (Haltiwanger et al., 2013; Decker et al., 2014; Davis and Haltiwanger, 2014; Decker et al., 2016). Karahan et al. (2019) and Hopenhayn et al. (2022) show that slowdowns in labor supply growth are a primary driver of declining startup rates, with entry suppressed while incumbent dynamics remain largely intact, which is precisely the pattern we document in the context of immigration enforcement. Our findings show that a specific federal policy can suppress the entry margin of business dynamism, connecting immigration enforcement to a macro debate about the long-run sources of declining firm formation and economic dynamism (Decker et al., 2020).

The remainder of the paper is organized as follows. Section 2 provides institutional background, Section 3 describes the data, and Section 4 outlines the empirical strategy. Sections 5 and 6 present the main results and mechanisms. Sections 7 and 8 report additional results and robustness checks. Section 9 concludes.

2 Institutional Background

2.1 Secure Communities: Design and Operational Mechanism

The 1996 Illegal Immigration Reform and Immigrant Responsibility Act substantially expanded the scope of interior immigration enforcement in the United States, increasing federal oversight and strengthening cooperation between federal and local law enforcement agencies.[1] This expansion raised the expected probability of detection and removal, altering local labor supply conditions.

Secure Communities, introduced in 2008, became the central interior enforcement program of this period. Its design differs from earlier programs by relying on automated information sharing between federal agencies. Under SC, when an individual is booked into a participating county jail, their fingerprints are transmitted to the Federal Bureau of Investigation and automatically checked against the Department of Homeland Security's immigration databases. If the individual is identified as potentially removable, ICE is notified.[2]

[1]It expanded civil penalties for unlawful presence, widened the use of expedited removal procedures, and, most relevant for this study, introduced Section 287(g), which authorizes formal agreements allowing state and local agencies to perform specified immigration enforcement functions under federal supervision. State-level omnibus immigration laws further extended enforcement through coordinated legislative efforts, including mandates on employment verification, restrictions on access to public benefits, limits on access to driver's licenses and certain educational benefits, and expanded local law enforcement authority.

[2]Following a match, ICE may issue an immigration detainer requesting that the local jail hold the individual for up to 48 additional hours beyond their scheduled release to allow federal authorities to assume custody.

Because this data-sharing is triggered automatically once the required infrastructure is in place, activation does not require affirmative policy adoption by local jurisdictions.

By increasing the probability that an arrest leads to detention and removal, SC raises the expected cost of residing and working without legal status through several behavioral channels. First, actual deportations directly remove workers from the local labor force (Cox and Miles, 2013; East et al., 2023; Alsan and Yang, 2024). Second, the elevated risk of detection induces voluntary out-migration among undocumented residents who relocate to jurisdictions with lower enforcement intensity (Bohn et al., 2014; Amuedo-Dorantes et al., 2019; Smith, 2026). Third, SC may deter new inflows of undocumented immigrants who would otherwise have entered the local labor market (Amuedo-Dorantes et al., 2019; Hanson and Spilimbergo, 1999). In particular, the design of SC implies stronger effects along margins that depend on new hiring and business formation, and in sectors with greater reliance on immigrant labor.

2.2 Federal Rollout and the Source of Identifying Variation

Secure Communities was implemented through a phased national rollout between 2008 and 2013, achieving near-universal county coverage by the end of that period. Activation occurred at the county level as data-sharing capabilities between local law enforcement agencies and federal databases were established and as federal enforcement capacity expanded. In contrast to earlier programs such as 287(g), which relied on voluntary agreements initiated by local jurisdictions, requiring counties to actively apply, negotiate terms, and enter into formal memoranda of agreement with ICE, SC was deployed through a centralized federal process and did not require counties to apply for participation or take any affirmative steps toward adoption. This distinction is important for identification: because 287(g) adoption required local initiative, its timing plausibly reflected local political preferences, enforcement priorities, and immigrant population characteristics, making it a poor source of quasi-experimental variation. SC activation, by contrast, was determined by federal infrastructure deployment decisions, making its timing substantially more plausibly exogenous to local economic conditions (Cox and Miles, 2013; East et al., 2023).

Although the rollout was not randomized, its timing was driven primarily by federal operational considerations, including technical readiness and administrative capacity, rather than contemporaneous local economic conditions. Conditional on county and time fixed effects, identification is therefore derived from differences in the timing of federal activation across counties. A remaining concern is that earlier activation was concentrated in counties with larger immigrant populations, border or metropolitan enforcement priorities, and potentially different exposure to the Great Recession. These features do not mechanically invalidate the design; they are partly a consequence of where the federal system first had operational and enforcement capacity, but they mean that SC timing should not be interpreted as randomly assigned.

We address this concern in three ways. First, in our regression specification, we control for a rich set of time-varying county and state characteristics, including other concurrent immigration enforcement policies, local labor demand conditions, and housing market exposure, which absorbs observable confounders that might correlate with both activation timing and business outcomes. Second, we report event-study estimates and formal pre-period tests, distinguishing outcomes with clean pre-trend evidence from those where the diagnostics are more mixed. and most directly, we test empirically whether pretreatment trends in local business dynamics, demographic changes, unemployment, housing conditions, and political characteristics predict the timing of SC activation, and find no systematic relationship, as we document in Section 4. While targeting on unobservables cannot be fully ruled out, the combination of centralized rollout, high-dimensional fixed effects, and flat outcome-specific pre-trend provides substantial support for the identifying assumption.

We restrict our main analysis to the 2008-2012 period for two complementary reasons. First, we exclude 2013 from the sample to retain a set of not-yet-treated counties that serve as a comparison group in the empirical strategy, as some counties are first activated in that year.

Second, in 2014 SC was discontinued and replaced by the Priority Enforcement Program, which narrowed the scope of enforcement by focusing on individuals with serious criminal convictions, creating a structural break in the policy environment that would complicate identification beyond this window. Restricting the sample to 2008-2012 therefore ensures both a valid comparison group and a consistent policy regime throughout the analysis period.

Figure 1 illustrates the geographic expansion of SC across U.S. counties during the roll-out period, with activation initially concentrated along the southern border and in large metropolitan areas before reaching roughly 97% of counties by 2012.

3 Data

3.1 Data Sources

We use data from 2005 to 2012, which includes several pre-treatment years prior to the initial rollout of SC in 2008. We exclude 2013 to retain a set of not-yet-treated counties, as some counties are first treated in that year, as required by our empirical strategy. SC was discontinued in 2014 and replaced by the Priority Enforcement Program, which narrowed the scope of enforcement, so our sample period reflects a consistent policy regime. For all datasets, we restrict the sample to the contiguous United States. Below, we describe our main data sources.

3.1.1 Business Dynamics Statistics

Our primary dataset for outcome variables is the Business Dynamics Statistics (BDS), published annually by the U.S. Census Bureau and constructed from the Longitudinal Business Database (LBD), which links administrative records on employer establishments over time. The BDS reports non-farm establishment and employment counts, along with entry and exit flows and job creation and destruction, at the county-sector-year level. Sectors correspond to

2-digit NAICS industry classifications. The BDS sample contains 479,712 county-sector-year observations for our sample period.[3]

We study several outcomes capturing firm dynamics. Our main outcomes are the number of establishment entries and exits.[4] We also examine job creation and job destruction to capture changes in employment. To further decompose local job dynamics, we distinguish between flows arising from establishment births and deaths and those from continuing establishments. Specifically, job creation is decomposed into job creation from establishment births and job creation from expansions among continuing establishments, while job destruction is decomposed into job destruction from establishment deaths and job destruction from contractions among continuing establishments. The data also provide rich heterogeneity by firm size and firm age, which we exploit to examine differential responses across these dimensions.

3.1.2 American Community Survey

We use data from the American Community Survey (ACS), which provides large, nationally representative samples with rich socio-demographic information, allowing us to track population composition and self-employment over time. We draw on two ACS products for different purposes. For the self-employment panel over 2005-2012, we use ACS 1-year accessed via IPUMS (Ruggles et al., 2023). These data are available at the Public Use Microdata Area (PUMA) level, and we map PUMAs to counties using a probabilistic crosswalk with population weights. For baseline measures of immigrant exposure used in heterogeneity analysis, including county-level and sector-level shares of immigrants with a high school education or less, we use ACS 5-year summary tables accessed via the Census API (Walker and Herman,

[3]The BDS suppresses cells with establishment or employment counts below a disclosure threshold, resulting in missing values for some county-sector-year cells; consequently, the estimation sample is smaller than the full set of observations reported here. We address the potential for selection bias from this suppression in Section 8.2.

[4]We follow the BDS convention throughout. An *establishment* is a single physical location where business is conducted. A *firm* is a legal entity that may own one or more establishments.

2021), which are available directly at the county level and do not require a crosswalk. These summary tables are available for counties with a population of more than 65,000.

3.1.3 Quarterly Census of Employment and Wages

To examine wage responses to immigration enforcement, we use data from the Quarterly Census of Employment and Wages (QCEW) from 2005 to 2012. The QCEW is a dataset administered by the U.S. Census Bureau and constructed from administrative records of employers covered by state unemployment insurance programs, providing near-universe coverage of employment and wages at the county-industry level. We use the QCEW variable on average weekly earnings by county, sector, and year. Since the QCEW does not report hourly wages or hours worked, average weekly earnings provide the closest available measure of labor compensation, although they may also reflect variation in hours worked.[5]

3.1.4 Additional Datasets

To characterize differences in labor market frictions across sectors, we use the Quarterly Workforce Indicators (QWI), a Census Bureau dataset derived from the Longitudinal Employer-Household Dynamics (LEHD) program, which links non-farm employer and employee administrative records and provides measures of employment, earnings, hires, separations, and turnover. We use these data to construct baseline sectoral turnover rates at the county-sector level using the 2005-2007 data, which serve as a proxy for hiring frictions and allow us to examine whether enforcement effects are stronger in high-turnover sectors.

We also construct a measure of state-level housing boom exposure using data from the Federal Housing Finance Agency (FHFA). Specifically, following East et al. (2023), we compute the population-weighted average county-level FHFA house price index within each state

[5] To the extent that enforcement affects both hourly wages and hours worked, changes in average weekly earnings combine these margins. If enforcement raises wages but reduces hours, weekly earnings may attenuate the underlying wage response; conversely, if hours remain stable or increase, weekly earnings more closely reflect wage changes. We therefore interpret changes in average weekly earnings as suggestive evidence of labor market tightening rather than a pure wage effect.

in 2000 and 2006, and define the percentage change as a time-invariant measure of housing market exposure. This measure is interacted with linear and quadratic time trends to flexibly capture differential housing cycle trajectories across states.

To control for other immigration enforcement policies, we compile data from several sources. Specifically, we collect information on the timing of 287(g) program adoption at the county and state levels from Kostandini et al. (2014), E-Verify implementation from Orrenius and Zavodny (2015), state-level omnibus immigration laws from Allen and McNeely (2017) and Luo and Kostandini (2023), and sanctuary policy adoption from Martínez-Schuldt and Martínez (2019), where for each county we identify the earliest adoption date of any sanctuary policy at the city, county, or state level. We verify these data against official records from ICE and the DHS.

Finally, we obtain state minimum wage data from the Benzipperer Historical Minimum Wage Database (Vaghul and Zipperer, 2016), which we include as a control variable in our regressions.

3.2 Descriptive Statistics

Table C.1 reports pre-treatment descriptive statistics for both unweighted and population-weighted averages. Weighted averages are substantially larger throughout, as economic activity and immigrant populations are disproportionately concentrated in more populous counties. In what follows we discuss the weighted figures, which correspond to the estimator used in our regressions and are weighted by county population in 2000.

Weighted by baseline county population, the average county contains 20,837 firms and 24,507 establishments, with 3,125 establishment entries and 2,653 exits per year over 2005-2007, alongside 71,708 jobs created and 66,087 jobs destroyed. These magnitudes are consistent with the well-documented high pace of firm and job reallocation in the U.S. economy (Davis et al., 1998; Haltiwanger et al., 2013), where entry and hiring flows constitute a central margin of adjustment.

At the county-sector level, the unit of analysis in our regressions, the population-weighted average cell contains 1,122 firms and 1,320 establishments. This reflects a non-trivial density of economic activity even within narrowly defined local markets, comparable to the level of disaggregation used in studies of local labor demand shocks and business dynamics (Autor et al., 2013; Greenstone et al., 2010). The richness of this variation allows us to exploit differential exposure across sectors within the same local labor market.

The population-weighted average non-citizen share is 8.1%, with a foreign-born low-education share of 8.6% and a Hispanic non-citizen share of 5.2%. This concentration is a key feature of the empirical setting, as it generates meaningful differences in exposure to immigration enforcement across counties, which we exploit in heterogeneity analyses. These features point to an environment with active firm and labor market adjustment and substantial heterogeneity in exposure to immigrant labor, which we leverage in the empirical strategy to identify the effects of immigration enforcement on local business dynamics.

4 Empirical Strategy

4.1 Empirical Specification

We estimate the effects of SC implementation on local business dynamics using a difference-in-differences framework with staggered treatment timing. We begin by outlining a standard difference-in-differences specification as a benchmark. A conventional three-way fixed effects model (TWFE) would take the following form:

$$Y_{c,j,t} = \beta\, SC_{c,t} + X'_{c,t}\phi + \mu_c + \lambda_t + \delta_j + \varepsilon_{c,j,t} \tag{1}$$

where $Y_{c,j,t}$ denotes the outcome variable for county c, sector j, and year t. The variable $SC_{c,t}$ is a binary indicator equal to one when SC is active for a majority of months in year t in county c.

The specification includes county fixed effects (μ_c), year fixed effects (λ_t), and sector fixed effects (δ_j). County fixed effects absorb all time-invariant county characteristics. Year fixed effects absorb aggregate time shocks common to all counties. Sector fixed effects absorb time-invariant differences across industries, such as baseline differences in entry, exit, and employment dynamics.[6]

The vector of county- and state-level controls, $X_{c,t}$, includes indicators for county-level 287(g) agreements, state-level 287(g) agreements, E-Verify mandates, Omnibus Immigration Bills, sanctuary policies,[7] [8] the state minimum wage, a Bartik labor demand index that captures sector-specific local demand shocks (Bartik, 1991; Autor et al., 2013),[9] and a state-level housing boom exposure measure interacted with linear and quadratic time trends.[10] Because our sample period spans the Great Recession, these last two variables flexibly capture differential exposure to both housing market shocks and broader macroeconomic demand fluctuations across states.

The error term $\varepsilon_{c,j,t}$ is clustered at the county level to account for serial correlation and arbitrary within-county dependence over time. All regressions are weighted by county population in 2000 to account for differences in county size and to ensure that estimates reflect effects on the average resident rather than the average county.[11] The estimation sample spans 2005-2012, which ensures sufficient pre-treatment observations for all SC activation cohorts while covering the main rollout period.

[6] Following East et al. (2023), we do not include state-by-year fixed effects, because SC was activated across all counties in some states simultaneously, so that state-by-year fixed effects would absorb much of the identifying variation in the SC rollout.

[7] Sanctuary policies can be implemented at the city, county, or state level. For each county, we identify the earliest adoption date of any sanctuary policy at any of these levels and define annual treatment as an indicator equal to one if the policy is in effect for at least six months of a given year.

[8] See Appendix A for descriptive of these immigration policies.

[9] See Appendix Section B for details on its construction.

[10] We construct this variable as follows: we construct the population-weighted average county-level FHFA house price index within each state, computed for 2000 and 2006, where the percentage change defines a time-invariant state-level housing boom exposure. This shock is interacted with linear and quadratic time trends to flexibly capture differential housing cycle trajectories across states.

[11] We also report unweighted estimates in the robustness section. These assign equal weight to each county and allow us to assess whether the results are driven disproportionately by more populous counties.

Equation (1) provides a familiar starting point for the empirical design. However, in settings with staggered treatment timing and heterogeneous treatment effects, the two-way fixed effects estimator can produce biased estimates (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak et al., 2024). In staggered adoption settings, TWFE implicitly uses already-treated counties as controls for newly treated ones, and when treatment effects vary across cohorts or over time, this generates negative weights on some comparisons, potentially biasing the aggregate estimate (Goodman-Bacon, 2021). We therefore rely on the interaction-weighted estimator of Sun and Abraham (2021), which restricts all comparisons to not-yet-treated counties and aggregates cohort-specific effects using non-negative weights, yielding an interpretable average treatment effect that is robust to arbitrary treatment effect heterogeneity. In the robustness section, we show that results are stable using the imputation estimator of Borusyak et al. (2024).

4.2 Average Treatment Effect and Event Study Estimates

To estimate dynamic treatment effects and assess pre-trends, we implement an event study using the interaction-weighted estimator of Sun and Abraham (2021), which allows treatment effects to vary across cohorts and over time. The specification is:

$$Y_{c,j,t} = \sum_{g\in\mathcal{G}} \sum_{k\neq -1} \beta_{g,k}\, \mathbb{1}(E_c = g)\, \mathbb{1}(t-g=k) + X'_{c,t}\phi + \mu_c + \lambda_t + \delta_j + \varepsilon_{c,j,t}, \tag{2}$$

where E_c denotes the first year in which SC is active for a majority of months in county c. The term $\mathbb{1}(E_c = g)$ identifies counties first treated in year g, and $\mathbb{1}(t-g=k)$ indicates event time k relative to treatment. The coefficients $\beta_{g,k}$ capture cohort-specific treatment effects at event time k, with $k=-1$ omitted as the reference period.[12]

[12]Counties first treated in 2013 or later are classified as never-treated, following the standard practice in staggered difference-in-differences designs of using not-yet-treated as the comparison group (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). Table C.2 reports the distribution of counties across activation cohorts.

For presentation, we report the interaction-weighted averages of these cohort-specific effects at each event time,

$$\beta_k = \mathbb{E}_g[\beta_{g,k}],$$

which summarize the average treatment effect at relative time k.

The event window spans $[-4, +3]$ relative to first activation.[13] Flat pre-treatment coefficients for $k \in \{-4, -3, -2\}$ provide evidence consistent with parallel trends, while post-treatment coefficients trace the dynamic response of business outcomes following SC activation.

To summarize the overall effect of SC, we report an average treatment effect on the treated obtained by aggregating cohort- and event-time-specific treatment effects estimated using Sun and Abraham (2021). Formally,

$$ATT = \mathbb{E}_g \left[\mathbb{E}_{k \geq 0} \left[\beta_{g,k}\right]\right], \tag{3}$$

where $\beta_{g,k}$ denotes the treatment effect for cohort g at event time k. In practice, this corresponds to a weighted average of post-treatment event-time coefficients, where weights reflect the relative frequency of treated observations across cohorts and event times. Specifically, cohorts with more counties and more post-treatment periods observed within the sample window receive greater weight in the aggregation.

4.3 Identification

Our empirical strategy compares within-county changes in business outcomes around the timing of SC activation to those in counties that have not yet been treated, while controlling for county, year, and sector fixed effects and a rich set of policy and economic controls. We interpret the estimates as effects for treated county-sector cells exposed during the federal

[13] The asymmetric window reflects the structure of the data, with four pre-treatment periods available from the 2005 sample start and post-treatment periods ranging from one to three depending on the activation cohort.

rollout, rather than as a universal elasticity of business dynamics with respect to all forms of immigration enforcement. Identification exploits staggered variation in the timing of SC activation across counties.

Formally, the identifying assumption is a generalized parallel trends condition. Let $Y_{c,j,t}(0)$ denote the untreated potential outcome. Then, for all event times $k < 0$,

$$E[Y_{c,j,t}(0) - Y_{c,j,t-1}(0) \mid E_c = g, X_{c,t}] = E[Y_{c,j,t}(0) - Y_{c,j,t-1}(0) \mid E_c = g', X_{c,t}]\,, \qquad (4)$$

for any cohorts g and g'. This assumption implies that, absent SC, early- and late-adopting counties would have followed similar trends in business dynamics. This is plausible in our setting because activation timing was determined by federal infrastructure deployment rather than local economic conditions, implying that counties activated earlier were not systematically on different trajectories in firm entry, job creation, or other business outcomes prior to treatment. We provide empirical support for this assumption through event study estimates showing flat pre-treatment coefficients across all main outcomes, and by demonstrating in Table C.3 that pre-treatment changes in county characteristics do not systematically predict activation timing.[14]

5 Main Results

Table 1 reports aggregate ATTs estimated using the staggered difference-in-differences estimator of Sun and Abraham (2021). Each column corresponds to a separate outcome: establishment entry, establishment exit, job creation, and job destruction. Our primary outcomes are establishment entry and exit, while job creation and destruction provide complementary

[14]Pre-period changes in county characteristics are largely uncorrelated with SC adoption timing (see Appendix Table C.3), with the only statistically significant predictor being establishment growth, which enters with a trivially small coefficient in a counterintuitive direction, as stronger business growth predicts earlier SC adoption contrary to the notion that economically struggling counties would be more likely to embrace an immigration enforcement program. These results suggest that SC adoption is plausibly as good as random conditional on observables.

evidence on the underlying employment dynamics. Treatment cohorts are defined by the first year in which SC is active for a majority of months in a county, with not-yet-treated serving as the comparison group. All specifications include county, year, and sector fixed effects, and control for other county- and state-level immigration enforcement indicators, the state minimum wage, a Bartik labor demand index, sanctuary policy status, and a state-level housing boom exposure measure interacted with linear and quadratic time trends. Observations are weighted by baseline county population, and standard errors are clustered at the county level.

We estimate regressions in levels and express coefficients as a percentage of the pre-treatment, population-weighted mean of the outcome in treated counties.[15] In addition, the BDS suppresses cells with establishment or employment counts below a disclosure threshold, so we drop observations with missing values. In Section 8.2, we show that the results are robust to potential bias from suppression-induced missing data.

SC activation reduces establishment entry by 12.3% on average per county-sector-year cell, while the coefficient for establishment exit is statistically indistinguishable from zero. Job creation falls by 13.8% per county-sector-year cell, while we find no evidence that SC affects job destruction. The decline in establishment entry and job creation are both statistically and economically significant, whereas the absence of detectable effects on establishment exit and job destruction is consistent with enforcement operating primarily through the entry and hiring margins rather than through firm shutdowns or separations.

The event study estimates confirm and extend these findings by tracing the temporal evolution of the effects and providing evidence on the parallel trends assumption. The dynamic analysis traces the temporal evolution of these effects and evaluates the parallel trends assumption. We again employ the heterogeneity-robust estimator of Sun and Abraham (2021). Figure 2 displays the event study estimates, with vertical bars representing

[15] Log-like transformations such as $\log(1 + y)$ or $\text{arcsinh}(y)$ yield scale-dependent estimates in settings where treatment affects the extensive margin, as their interpretation depends on the units of the outcome (Chen and Roth, 2024). We therefore report effects in levels throughout.

95% confidence intervals. This approach avoids the ‘forbidden comparisons’ that arise under standard two-way (or three-way) fixed effects with staggered adoption, where already-treated cohorts serve as implicit controls and enforcement timing can be confounded with broader macroeconomic conditions (De Chaisemartin and d’Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). The specifications include the same set of controls as in the baseline.

The pre-treatment coefficients are small and statistically indistinguishable from zero for establishment entry, establishment exit, and job creation, supporting the parallel trends assumption for these outcomes. In contrast, job destruction exhibits a persistent and statistically significant pre-treatment pattern. Accordingly, we place the greatest weight on the first three variables, which provide the clearest evidence of dynamic responses.

Following activation, establishment entry declines sharply on impact ($k = 0$) and remains negative throughout the post-treatment period, with little evidence of attenuation across event-time horizons. Job creation also declines after activation, with the largest effects concentrated in the earlier post-treatment years. Estimates at longer horizons become increasingly imprecise as the number of counties contributing to later event-time cells falls, reflecting limited support rather than meaningful mean reversion. By contrast, establishment exit and job destruction show no systematic post-activation response, consistent with the null aggregate ATTs. This asymmetry mirrors the static results: enforcement operates primarily through reductions in establishment entry and hiring, without offsetting increases in establishment exit and job separations, indicating a slowdown in firm formation and job growth.

6 Mechanisms

The main results show that SC activation reduces establishment entry and job creation, while we find no statistically significant effects on exit or job destruction. We now ask whether this pattern is consistent with immigration enforcement operating as a negative local labor supply shock, and whether alternative explanations can account for the evidence. We proceed in five steps. First, we exploit the spatial and sectoral concentration of effects to assess whether the evidence is more consistent with immigrant-specific exposure than with broad shocks alone. Second, we provide direct first-stage evidence that SC activation reduced the local immigrant workforce. Third, we trace how the labor supply contraction propagates through firm dynamics, focusing on the entry-incumbent asymmetry. Fourth, we examine corroborating evidence from wages and hiring frictions. Fifth, we consider two complementary channels, local demand and immigrant entrepreneurship, that may amplify the primary effects in specific settings.

6.1 Isolating the Labor Supply Channel

We begin by asking whether the pattern of effects is consistent with the labor supply channel specifically, or whether alternative explanations could account for the main results. The spatial and sectoral concentration of effects provides the key evidence.

The core identification logic is as follows. If the effects were driven by broad macroeconomic demand shocks or general policy uncertainty, we would not expect them to concentrate precisely along the dimension of immigrant labor intensity, conditional on controlling for local demand conditions. By contrast, a labor supply shock operating through the availability of immigrant workers generates a sharp prediction: effects should be large where immigrant labor is important and essentially zero where it is not. The concentration of effects in immigrant-intensive sectors and counties is therefore difficult to reconcile with any explanation that does not operate through immigrant labor specifically.

To test this prediction, we split the sample along two dimensions: sector-level immigrant employment share and county-level immigrant population share. In both cases, we divide at the median and estimate Sun and Abraham (2021) ATTs separately on each subsample.

Low-immigrant sectors and counties serve as implicit control groups, as they tell us what enforcement does to firms that do not depend on immigrant labor.

Table 2, Panels A and B report results by sector immigrant share. In high-immigrant-share sectors,[16] SC activation reduces establishment entry by 17.9% and job creation by 19.9%, both statistically significant. We find no evidence of effects on exit or destruction. In low-immigrant-share sectors, all four outcomes are economically small and statistically insignificant. Similarly, Panels C and D report results by baseline county immigrant share. In high-immigrant-share counties, SC activation reduces establishment entry by 11.7% and job creation by 11.7%. In low-share counties, all outcomes are economically small and statistically insignificant.

Together, these results weigh against explanations based solely on broad aggregate demand, general policy uncertainty, or economy-wide enforcement spillovers. They do not by themselves separately identify every immigrant-specific channel: local consumption, entrepreneurship, network disruption, and labor supply can all move more strongly in immigrant-intensive places. The concentration of effects is nevertheless consistent with the core mechanism that enforcement reduced the pool of immigrant workers available to local firms, constraining entry and expansion in the settings most dependent on that labor.

6.2 Enforcement and Local Labor Supply

Having established that the effects concentrate where immigrant labor matters, we now ask whether SC activation actually reduced the local immigrant workforce. The labor supply channel requires a first-stage effect on population.

Existing work shows that intensified interior enforcement reduces the local presence of undocumented immigrants through deportations, out-migration to less restrictive jurisdictions, and reduced inflows (Bohn et al., 2014; Amuedo-Dorantes et al., 2019; Smith, 2026). We provide direct evidence of this first-stage effect before examining how the contraction

[16] Table C.4 reports the immigrant share by sector underlying this split.

propagates through firm dynamics. We estimate Sun and Abraham (2021) event studies using the same specification as equation (2), replacing business outcomes with county-level immigrant population shares from the ACS 1-year estimates. Since population shares are county-level outcomes, the specification excludes sector fixed effects.[17]

Figure C.1 reports results for three measures: the non-citizen share of the population (Panel A), the foreign-born share (Panel B), and the Hispanic non-citizen share (Panel C). The non-citizen share captures the population directly subject to removal under SC. The foreign-born share is broader, including naturalized citizens and lawful permanent residents who are not directly targeted by enforcement. The Hispanic non-citizen share isolates the group most exposed to enforcement, as the vast majority of deportations under SC involved Hispanic non-citizens (Cox and Miles, 2013).

For the non-citizen and Hispanic non-citizen shares, pre-treatment coefficients are small and centered around zero, consistent with parallel trends. Following activation, estimates turn negative and increase in magnitude through $k = +3$. The pattern is most pronounced for the Hispanic non-citizen share, which exhibits a sharp post-treatment decline. The foreign-born share shows a similar decline, although pre-treatment coefficients display some downward drift, consistent with the inclusion of naturalized citizens whose trends may reflect earlier enforcement dynamics.

Table C.5 reports Sun and Abraham (2021) aggregate ATTs. The non-citizen share declines by 0.218 percentage points, a 2.9% reduction relative to the pre-treatment mean of 7.6 percentage points. The foreign-born share declines by 0.453 percentage points, significant at the 1% level, corresponding to a 3.4% reduction. The Hispanic non-citizen share declines by 0.211 percentage points, significant at the 5% level, a 4.2% reduction relative to its pre-treatment mean of 5.1 percentage points. The larger proportional decline for the Hispanic non-citizen share is consistent with SC disproportionately affecting the most exposed population.

[17] The sample is restricted to counties with populations above 65,000, as ACS 1-year summary tables are only available for counties meeting this population threshold.

These results confirm that SC activation reduced the local immigrant workforce, supporting the interpretation of enforcement as a negative local labor supply shock. The modest magnitudes of the first-stage population require care when interpreting the larger establishment entry and job creation declines. The 2.9-4.2% reduction in immigrant population shares is measured at the county level and does not capture how exposure varies across sectors and firms. Relative to the 12.3% decline in establishment entry and 13.8% decline in job creation, these first-stage estimates imply elasticities of roughly 2.9-4.2 for entry and 3.3-4.8 for job creation.

Importantly, these outcomes capture flow margins, namely establishment entry, exit, job creation, and job destruction, rather than changes in the stock of firms or establishments. As such, the estimated responses reflect adjustments at the margins of firm dynamics, where proportional changes can be larger than those observed in aggregate stocks.

This reflects that the relevant labor supply for many firms is smaller and more concentrated than the county-level immigrant share. Firms in immigrant-intensive and high-turnover sectors rely disproportionately on this subset of workers, so a modest decline in the county-average immigrant population can translate into a larger contraction in the effective labor pool they face. Accordingly, the estimates are best interpreted as reflecting responses in high-exposure settings rather than a universal enforcement elasticity.

6.3 Firm Adjustment Along the Intensive Margin

Having confirmed that enforcement contracts local labor supply, we now trace how this shock propagates through firms. The model in Appendix I predicts a sharp asymmetry: entrants face the full recruiting friction while incumbents adjust on the intensive margin. In plain terms, new establishments must recruit their entire workforce from scratch and are therefore especially sensitive to reductions in available workers. Incumbent firms have already matched with workers and face recruiting costs only on replacement hires and marginal expansion, allowing them to remain active even as labor constraints intensify. This asymmetry predicts exactly what we find: entry falls, job creation at continuing establishments falls, but exit and destruction are unchanged.

We test this prediction by decomposing job flows into those occurring at establishment births, establishment deaths, and continuing establishments. This decomposition distinguishes adjustments driven by firm turnover from those arising from changes in the behavior of surviving firms.

Table 3 shows that the decline in job creation is driven almost entirely by continuing establishments. Job creation at continuing establishments falls by 19.1%, while job creation at births declines modestly and is not statistically distinguishable from zero. This indicates that the reduction in aggregate job creation reflects slower expansion among incumbent firms rather than a collapse in the contribution of new establishments. On the destruction side, there is no evidence of increased exit: job destruction due to establishment deaths changes by only 1.5% and is statistically insignificant, and changes among continuing establishments are small and imprecise.

Figure 3 reinforces this interpretation. Job creation at continuing establishments declines sharply at $k = 0$ and remains persistently lower in the immediate post-treatment period, while job creation at births remains flat. We find no systematic evidence of changes in job destruction. These dynamics confirm that enforcement constrains firm expansion by increasing hiring frictions, shifting adjustment to the intensive margin rather than exit.

6.4 Labor Market Tightening and Hiring Frictions

The heterogeneity results and the firm-level decomposition are consistent with a tightening local labor market. We now examine two direct signatures of this tightening, rising wages and increased hiring frictions, using independent data sources from the QCEW and QWI.

Table C.6 reports estimates of the effect of SC activation on wages. Average wages increase modestly following activation, with statistically significant effects in log wages. Table C.7 shows that the effects are more pronounced in immigrant-intensive and non-tradable sectors, consistent with these sectors being more exposed to reductions in immigrant labor supply. The aggregate wage effect is modest at approximately 1% in log wages, which is expected as average wages are diluted by non-exposed sectors and workers. The sector-specific effects in Table C.7 are therefore the more informative numbers, and they point to meaningful labor cost increases precisely where enforcement exposure is highest.

In addition to raising labor costs, the labor supply contraction disrupts firms' ability to hire continuously. Sectors with high worker turnover depend on a steady flow of new hires to replace separations and sustain production, making them particularly sensitive to reductions in available workers. Table C.8 reports baseline turnover rates by sector. Figure C.2 shows that sectors with higher immigrant employment shares also have higher baseline turnover rates (Pearson $r = 0.77$). This strong correlation implies that the sectors most exposed to enforcement are also those most reliant on continuous hiring. Because immigrant share and turnover are highly correlated, we interpret turnover not as a separate mechanism but as a channel that amplifies the labor supply shock within immigrant-intensive sectors. Presenting turnover heterogeneity as independent evidence would overstate the distinctness of the two channels given this collinearity.

Table 4 shows that in high-turnover sectors, establishment entry declines by 14.7% and job creation by 19.0%, both statistically significant. In low-turnover sectors, entry declines by 8.8% and job creation is not statistically distinguishable from zero. These patterns confirm that sectors reliant on continuous labor inflows bear disproportionate adjustment costs when the available workforce contracts.

Together, rising wages and increased hiring frictions in exposed sectors provide corroborating evidence consistent with a labor market that has tightened following the enforcement-induced contraction in immigrant labor supply.

6.5 Secondary Channels

The labor supply and recruiting friction channels account for the core results. Two complementary channels may amplify these effects in specific settings: a reduction in local consumption demand from immigrant household departure, and a direct decline in immigrant entrepreneurship through removal and deterrence. We present suggestive evidence for each, while noting that the main results do not depend on either channel.

Local Consumption Demand. Non-tradable sectors depend on local consumption, while tradable sectors sell to broader markets and are less exposed to local demand shocks. Under a demand channel, effects should be stronger in non-tradable sectors. This channel is consistent with broader evidence that SC enforcement reduced local consumption demand (East et al., 2023) and disrupted non-tradable service markets: Ali et al. (2024) find that SC reduced children's participation in center-based child care, a non-tradable sector heavily staffed by immigrant workers.

Table 5 reports estimates separately by sector type.[18] Non-tradable sectors exhibit significant declines in both establishment entry and job creation. Tradable sectors show negative but imprecise estimates of similar magnitude, with the difference between sectors likely reflecting statistical power rather than a true difference in effects. This pattern is consistent with the demand channel operating as a secondary amplifier in non-tradable sectors while the labor supply channel operates across both sector types.

Immigrant Entrepreneurship. Enforcement may also reduce business formation directly by removing or deterring immigrant entrepreneurs. This channel is plausible because immigrants start businesses at higher rates than natives (Kerr and Kerr, 2020; Fairlie and Lofstrom, 2015), and emigration shocks reduce firm creation in origin communities (Anelli et al., 2023). Enforcement can operate through two margins: detention, deportation, and

[18] The tradable/non-tradable classification follows the standard division in the local labor markets literature (Moretti, 2010; Autor et al., 2013; Mian and Sufi, 2014).

induced out-migration directly reduce the stock of immigrant business owners, while enforcement risk may deter those who remain from starting businesses, registering firms, signing leases, or hiring workers.

We examine this channel using county-year self-employment measures constructed from the 2005-12 ACS data. Table 6 reports Sun and Abraham (2021) aggregate ATTs for non-citizen self-employment (columns 1-2), U.S. citizen self-employment (columns 3-4), and total self-employment (columns 5-6). The non-citizen self-employment rate declines by 0.6 percentage points (-6.4% relative to the pre-treatment mean), marginally significant. This is consistent with a 'chilling effect' in which enforcement risk deters non-citizen entrepreneurs beyond those directly removed. More striking, U.S. citizen self-employment also declines: the citizen rate falls by 0.2 percentage points (-2.2%, significant at the 5% level) and the citizen count falls by 1,185 (-2.6%, marginally significant).

Total self-employment declines by 1,425 (-2.5%). The decline among citizens, who face no direct enforcement risk, is consistent with the labor supply channel: with fewer immigrant workers available to hire, citizen entrepreneurs face greater difficulty staffing new ventures, consistent with East et al. (2023), who find that SC reduced native employment through complementarities between immigrant and native workers. This pattern distinguishes the labor supply mechanism from a pure removal story, in which only non-citizen self-employment would decline. The citizen decline also complements evidence from Mexico showing that SC deportations increased firm entry in receiving communities (Osuna Gomez and Medina Cortina, 2023), the counterpart to the entry decline we document on the U.S. side.

Synthesis. The concentration of demand effects in non-tradable sectors and of entrepreneurship effects among immigrants are both consistent with enforcement operating through immigrant-specific channels. Neither channel overturns nor competes with the primary labor supply interpretation, as they deepen it by identifying additional margins through which enforcement reduces local business dynamism.

7 Additional Results

7.1 Establishment and Firm Stock

The main analysis focuses on establishment entry and exit, capturing the margin of adjustment most directly affected by enforcement. Table C.9 complements these results by examining the stock of establishments and firms. SC activation reduces the total number of establishments by approximately 3.8% and the total number of firms by approximately 4.4%. Both estimates are significant at the 1% level.

The decline in firms exceeds that in establishments because the two measures capture different margins of adjustment. Firms are created only through new entry, while establishments can be created both through new firm entry and through expansion by existing firms. As enforcement reduces entry but leaves incumbent firms largely intact, establishment counts are partially sustained by ongoing expansion, attenuating their decline relative to firms.

Figure 4 presents the corresponding event studies. Pre-treatment coefficients are close to zero, consistent with parallel trends. The post-treatment decline emerges in the first year after activation and persists through the end of the event window.

7.2 Heterogeneity by Firm Size and Age

The results above indicate that enforcement effects are strongest in environments where firms rely on continuous hiring. We next examine which firms are most affected within these environments. Small and young firms are likely to be particularly vulnerable, as they recruit locally, have limited scope to substitute capital for labor, and lack the scale to reallocate workers across locations. We define heterogeneity at the firm level using BDS classifications and examine establishment entry, exit, job creation, and job destruction within firms of different sizes and ages.

We first examine heterogeneity by firm size. Firms are grouped as small (1-19 employees), medium (20-99), and large (100+). Table C.10 shows that the effects are concentrated among small firms. Establishment entry declines by 14.6% and job creation by 11.4%, both statistically significant. In contrast, medium and large firms exhibit weaker and imprecisely estimated responses. Large firms show no statistically significant change in entry and only a marginally significant decline in job creation (12.6%). These patterns indicate that the entry margin operates primarily through small firms, consistent with their greater reliance on local labor markets and limited ability to adjust through internal reallocation or capital substitution.

We next examine heterogeneity by firm age. Establishments are grouped as young (0-5 years), mature (6-10 years), and old (11+ years). Table C.11 shows that young firms drive the aggregate effects. Entry declines by 15.8% and job creation by 18.4%, both statistically significant. In contrast, mature firms show no statistically significant responses, while older firms exhibit only a marginally significant decline in job creation (13.1%) and no effects on entry or exit. This pattern highlights that enforcement disproportionately affects early-stage firms that depend on continued hiring to grow.

Taken together, these results point to a disruption of the startup margin. The decline in firm formation is concentrated among small and young firms, while larger and more established firms adjust primarily along the intensive margin without exiting. This asymmetry provides direct evidence that enforcement operates as a local labor supply shock that constrains entry and early-stage expansion, leaving the stock of incumbent firms largely intact.

8 Robustness

8.1 Robustness to Labor Market-Level Aggregation

A natural concern for county-level estimates is geographic displacement. If enforcement causes immigrants and the businesses associated with them to relocate to neighboring counties rather than exit the labor market, own-county coefficients overstate the aggregate effect. Counties are not self-contained labor markets; workers and firms operate across county boundaries within commuting zones (CZs) (Tolbert and Sizer, 1996; Autor et al., 2013), and low-skilled immigrants in particular are highly geographically mobile in response to local labor market conditions (Cadena and Kovak, 2016). We address this concern through three complementary tests.

Table C.12 reports results from two specifications. Panel A reproduces the baseline county-level estimates. Panel B replaces year fixed effects with CZ-by-year fixed effects, absorbing all time-varying shocks at the labor market level. If the county-level results reflect within-commuting-zone reallocation rather than net destruction, this specification would eliminate the effect. The establishment entry ATT moves from -12.3% to -12.2%, and the job creation ATT from -13.8% to -11.0%, both remain statistically significant and economically similar. The entry estimate is virtually unchanged; the job creation estimate shrinks modestly but gains precision. These results confirm that the main findings are driven by within-commuting-zone variation in enforcement timing across counties, not by differential trends across labor markets.

Panel C aggregates the county-sector-year panel to the commuting zone level and re-estimates.[19] At the commuting zone level, establishment entry declines by 16.6% and job creation declines by 13.9%, consistent in sign and proportional magnitude with the county-

[19] Treatment cohort at the commuting zone level is defined as the year when the cumulative population-weighted share of constituent counties under SC exceeds 50%. This avoids classifying a commuting zone as treated when only a small-population county has activated SC. Commuting zones where fewer than half of the population is treated by 2012 serve as the comparison group (18 CZs). We use the 1990 commuting zone definitions from Tolbert and Sizer (1996).

level results. The larger absolute magnitudes reflect the aggregation of multiple counties within each commuting zone. The commuting-zone specification also produces a positive and marginally significant estimate for job destruction. We therefore interpret the labor-market-level evidence as confirming net declines in entry and job creation, while treating the null job-destruction result as less robust than the entry margin.

8.2 Missing Data and Selection Bias

The public BDS data suppresses cells with establishment or employment counts below a disclosure threshold. Table C.13 reports the share of missing observations for each outcome. If enforcement-induced declines in business activity push county-sector cells below this threshold, differential missingness correlated with treatment could bias the estimates. We assess this concern using three tests.

First, we restrict the sample to a balanced panel of county-sector cells observed in every year. Table C.14 reports the results. The balanced-panel estimates are nearly identical to the full-sample estimates, confirming that differential attrition does not drive the main findings.

Second, we use missingness itself as the outcome. Table C.15 reports Sun and Abraham (2021) aggregate ATTs where the dependent variable is an indicator for whether the cell is suppressed. SC activation does not predict missingness for establishment entry, job creation, or job destruction. The one significant coefficient is for establishment exit, but this works against our findings: if enforcement causes marginal cells to be suppressed in the exit data, the true exit effect would be larger, reinforcing the entry-exit asymmetry rather than undermining it.

To gauge the potential magnitude of this bias, note that the baseline suppression rate for establishment exit cells is 36.1% and the estimated missingness coefficient is 0.021. Suppression in the BDS is concentrated among cells with very few establishments, which are precisely the cells least likely to drive aggregate exit dynamics, limiting the economic relevance of the missing observations. In the worst case, if all suppressed exit cells had above-average exit counts, the true exit effect could be larger in magnitude than our estimate. However, since our exit ATT is already statistically indistinguishable from zero, a larger true exit effect would only strengthen the entry-exit asymmetry that is central to our findings. The direction of any bias therefore works in favor of our interpretation rather than against it.

Third, we implement trimming bounds by dropping the smallest county-sector cells and re-estimating. Table C.16 reports results after trimming the bottom 1%, 5%, and 10% of cells by baseline establishment count. Point estimates and significance levels are stable across all thresholds. These results indicate that differential missingness induced by BDS suppression does not bias the estimates and that the main findings are not driven by sample selection.

8.3 Imputation Estimator

As a further check on robustness to treatment-effect heterogeneity, we re-estimate using the imputation estimator of Borusyak et al. (2024) with county-sector and year fixed effects, which constructs counterfactual outcomes for treated units using only never-treated and not-yet-treated observations and therefore rests on different assumptions than the Sun and Abraham (2021) estimator.

Table C.17 reports the aggregate ATTs estimated at the county-sector-year level using this approach, and Figure C.3 plots the corresponding dynamic event-study coefficients. Establishment entry declines by 18.8%, and job creation by 17.7%, both statistically significant. Exit and destruction are insignificant. The point estimates are somewhat larger than the Sun and Abraham (2021) estimates (Table 1), but both estimators agree on sign and significance for all four outcomes.

The divergence in magnitudes is expected given the staggered rollout structure. The two estimators weight cohorts differently when treatment effects are heterogeneous across activation cohorts: Sun-Abraham weights cohort-specific effects by their relative frequency in the data, while the imputation estimator of Borusyak et al. (2024) weights by the precision of the counterfactual estimates. When early and late cohorts have different treatment effect magnitudes, as is plausible given variation in local immigrant exposure across the roll-out period, the two estimators will produce different ATTs even when both are consistent. The agreement on sign and significance across both estimators therefore provides stronger confirmation of the main findings than magnitude alone would suggest.

8.4 Alternative Specification

As a robustness check, we replace year fixed effects and the explicit state-level controls with state-by-year fixed effects, which absorb all state-level time-varying confounds non-parametrically. As discussed in Section 4, we do not use state-by-year fixed effects as our baseline because they absorb a substantial share of the identifying variation. Table C.18 confirms that this specification delivers qualitatively similar results, with the SC coefficient identified from within-state, within-year variation. The trade-off is that state-by-year fixed effects absorb state-level enforcement policies that vary across states and years, so the SC coefficient is identified only from within-state, within-year variation.

8.5 Additional Robustness Checks

We conduct several additional robustness checks with full results in the Online Appendix. To assess sensitivity to omitted variable bias, we implement Oster (2019) bounds (Appendix D). The implied δ values are large in magnitude and negative, indicating that unobserved confounders would need to work in the opposite direction from observables and with implausible magnitude to eliminate the estimated effects. To account for spatial correlation across neighboring counties, we compute Conley (1999) spatial standard errors across a range of distance cutoffs (Appendix E). Standard errors are very similar to county-clustered standard errors and the statistical significance of the main results is unchanged.

We also confirm robustness to alternative clustering schemes, including state-level and two-way clustering by county and year (Appendix F), and dropping population weights (Appendix G). Finally, to assess whether the estimates are driven by any single activation cohort, we implement leave-one-cohort-out analyses (Appendix H). Dropping any single cohort leaves the establishment entry and job creation estimates stable in sign and significance, with entry ATTs ranging from −17.7 to −21.7 and job creation ATTs ranging from −176.2 to −572.2 across subsamples, confirming that no individual cohort drives the main findings. In all cases, the main estimates are stable in sign, magnitude, and significance.

9 Conclusion

Immigration enforcement reduces local business dynamism. Exploiting the staggered rollout of the Secure Communities program and county-sector-year data from several U.S. Census datasets, we show that enforcement reduces establishment entry by 12.3% and job creation by 13.8%, with no statistically significant effect on establishment exit or job destruction in the baseline specification. The effects are concentrated in immigrant-intensive and high-turnover sectors, high-immigrant counties, and among small and young firms. Dynamic estimates provide especially strong support for the establishment entry result; pre-trend diagnostics for job creation are more mixed, so we interpret that outcome as complementary evidence on the hiring margin. The results are robust to alternative estimators, labor market aggregation, and a wide range of specification checks.

The central pattern is a sharp asymmetry between entry and exit. We find no statistically significant evidence in the baseline estimates that enforcement induces firm closures or large-scale separations; instead, it suppresses firm formation and constrains expansion among existing establishments. The spatial and sectoral concentration of effects in immigrant-intensive settings is difficult to reconcile with explanations based solely on aggregate demand or general equilibrium reallocation, and points toward the availability of immigrant labor as a central channel. We document that SC reduces the local immigrant population, raises average weekly earnings in exposed sectors, and disproportionately affects sectors that rely on continuous hiring. Additional evidence shows stronger effects in non-tradable sectors and a decline in immigrant self-employment, indicating that reductions in local demand and entrepreneurship may amplify the primary labor supply channel. These results indicate that immigration enforcement operates as a negative local labor supply shock that propagates through firm entry and expansion.

These findings connect immigration enforcement to firm dynamics, a margin less developed in the existing enforcement literature. Prior work has focused primarily on effects on employment, wages, enforcement behavior, and specific markets. We show that these labor market disruptions transmit to the extensive margin of business formation and to the intensive margin of firm growth. The estimates are most directly informative about Secure Communities during its 2008-2012 rollout and about local labor markets with meaningful exposure to immigrant labor. They should not be mechanically extrapolated to all enforcement tools, periods, or low-immigrant places. The policy lesson is therefore not that every enforcement policy has the same business-dynamism cost, but that interior enforcement can reduce firm formation and expansion in exposed local economies, a cost that standard employment and wage outcomes may miss.

The policy implication follows directly from where adjustment occurs. Because enforcement operates through firm entry and expansion rather than exit, its economic costs are not realized as immediate dislocation but as a slowdown in the creation of new firms and jobs. This shifts the impact of enforcement toward the margin that governs long-run economic growth, implying that its effects accumulate over time through reduced dynamism (Davis and Haltiwanger, 2014) rather than through short-run disruptions to ongoing production, in ways that standard labor market measures of employment and wage would not fully capture.

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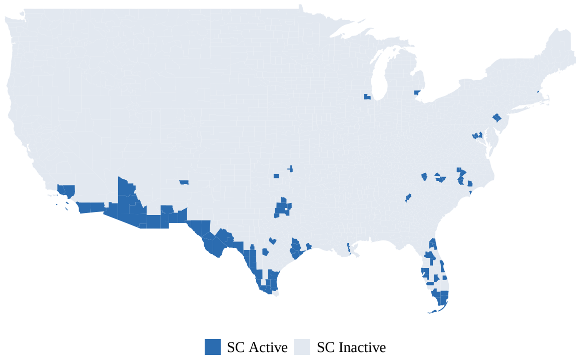
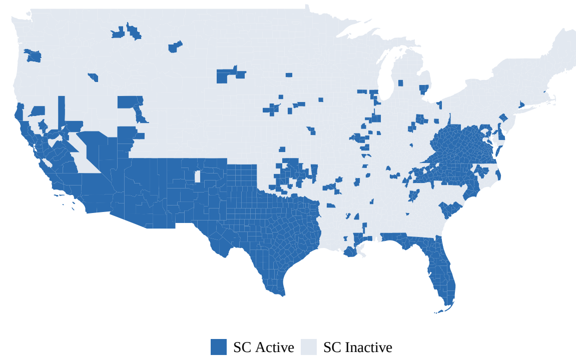
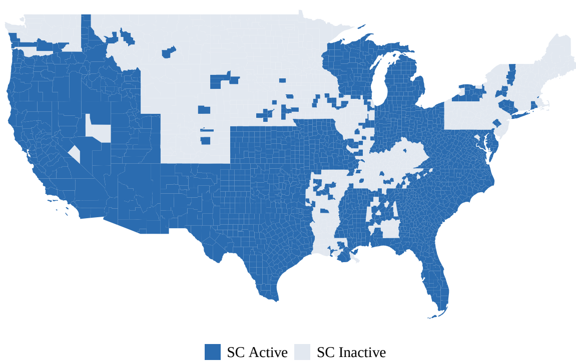
Figures



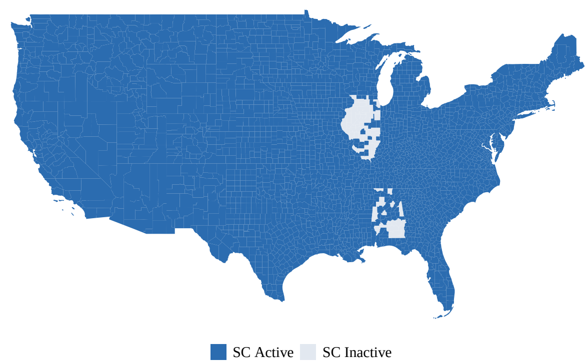
(a) By the end of 2009



(b) By the end of 2010



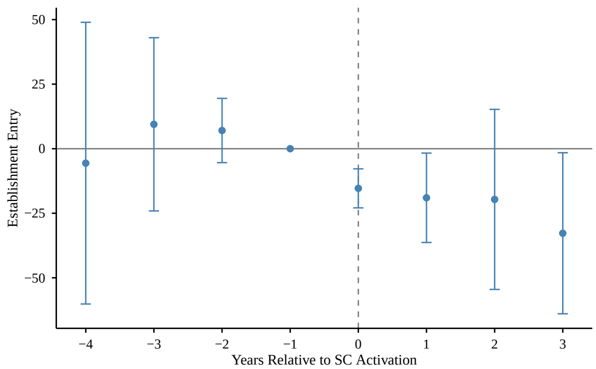
(c) By the end of 2011



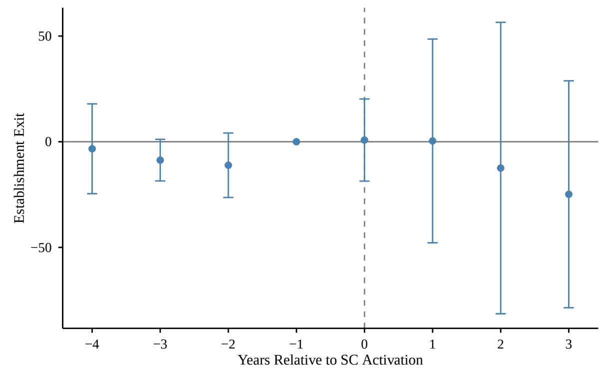
(d) By the end of 2012

Figure 1: Secure Communities Rollout by County, 2009-2012

Notes. Each panel shows counties where Secure Communities had been activated by the end of the indicated calendar year, based on ICE activation dates. Coverage expanded rapidly over the rollout period, reaching approximately 64% of jurisdictions by 2011 and near-universal coverage (about 97%) by 2012.



(a) Establishment Entry



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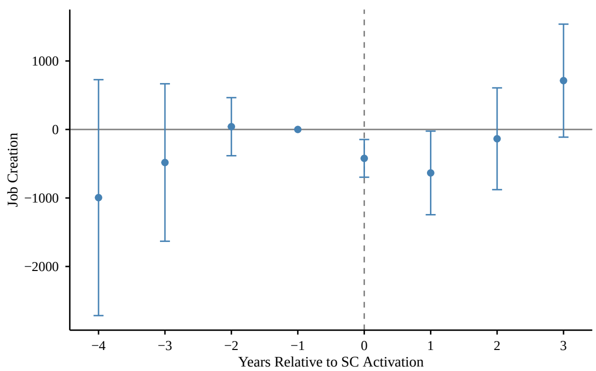
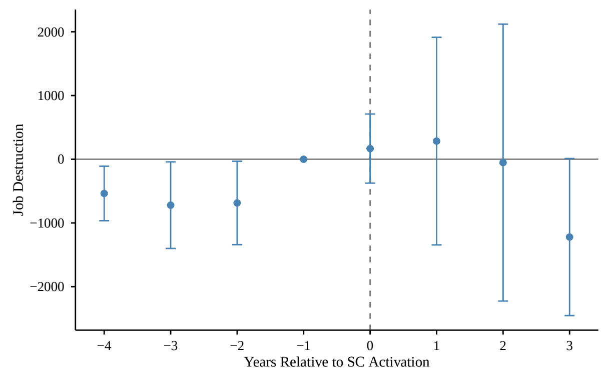


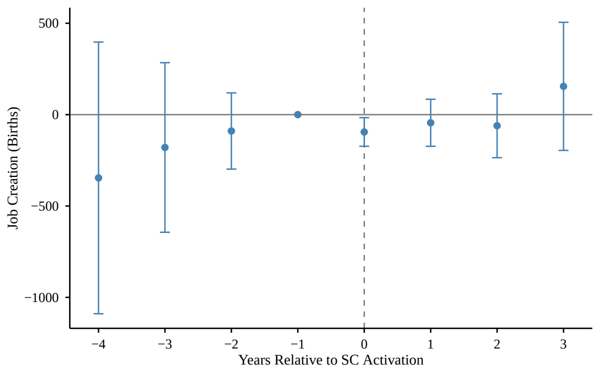
(c) Job Creation



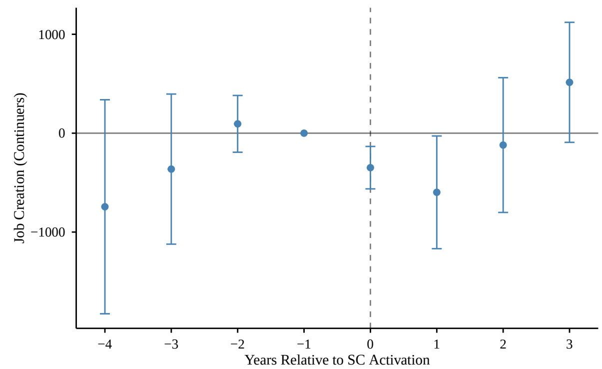
(d) Job Destruction

Figure 2: Event Study: Establishment and Employment Dynamics (Sun-Abraham)

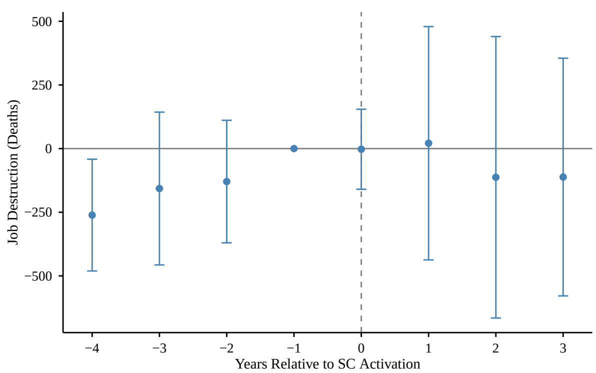
Notes. Sun-Abraham cohort-specific event study estimates (Sun and Abraham, 2021). Estimation sample: 2005-2012; event window $[-4, +3]$. Panels (a)-(b) show establishment entries and exits per county-sector-year cell; panels (c)-(d) show jobs created and destroyed. Event time $k = 0$ marks the first year of Secure Communities activation in county c; $k = -1$ is the omitted reference period. Controls include county-level and state-level enforcement indicators, the state minimum wage, a Bartik labor demand index, sanctuary policy status, and state-level housing boom exposure interacted with quadratic time trends. All specifications include county, year, and sector fixed effects, and are weighted by baseline county population. Standard errors clustered at the county level. Bars show 95% confidence intervals.



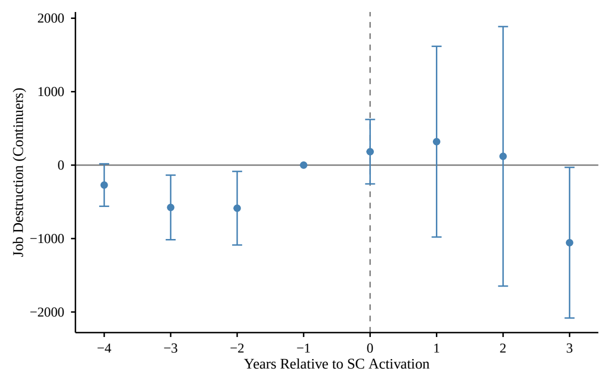
(a) Job Creation: Establishment Births



(b) Job Creation: Continuers



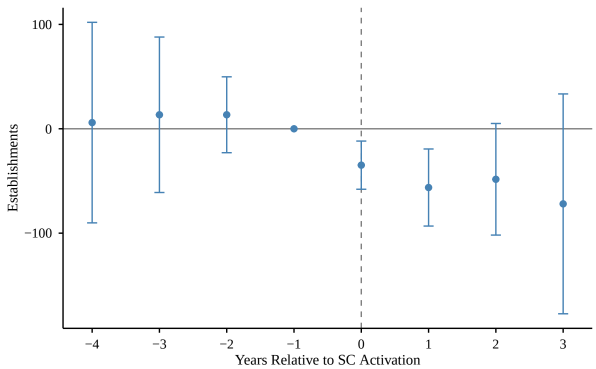
(c) Job Destruction: Establishment Deaths



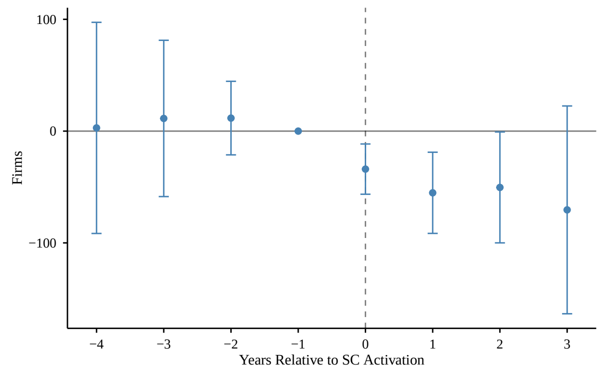
(d) Job Destruction: Continuers

Figure 3: Event Study: Job Flow Decomposition (Sun-Abraham)

Notes. Sun-Abraham event study estimates for the BDS job flow decomposition. Top row: job creation at births (a) and continuers (b). Bottom row: job destruction at deaths (c) and continuers (d). Estimation sample: 2005-2012. Event time $k = 0$ marks the first year of Secure Communities activation in county c; $k = -1$ is the omitted reference period. Controls include county-level and state-level enforcement indicators, the state minimum wage, a Bartik labor demand index, sanctuary policy status, and state-level housing boom exposure interacted with quadratic time trends. All specifications include county, year, and sector fixed effects, and are weighted by baseline county population. Standard errors clustered at the county level. Bars show 95% confidence intervals.



(a) Establishment Stock



(b) Firm Stock

Figure 4: Event Study: Establishment and Firm Stock

Notes. Sun-Abraham (Sun and Abraham, 2021) interaction-weighted event study estimates. Panel (a) shows results for the total number of establishments and panel (b) for the total number of firms. Event time $k = 0$ marks the first year of Secure Communities activation in county c; $k = -1$ is the omitted reference period. Controls include county-level and state-level enforcement indicators, the state minimum wage, a Bartik labor demand index, sanctuary policy status, and state-level housing boom exposure interacted with quadratic time trends. All specifications include county, year, and sector fixed effects, and are weighted by baseline county population. Standard errors clustered at the county level. Bars show 95% confidence intervals.

Tables

Table 1: Effects of Secure Communities on Establishment and Job Dynamics

	(1) Entry	(2) Exit	(3) Creation	(4) Destruction
SC Activation	-18.1**	-2.8	-394.4**	109.7
	(7.7)	(19.6)	(186.5)	(594.0)
Pre-treatment Mean	146.7	168.4	2,864.4	3,641.8
% Change	-12.3%	-1.7%	-13.8%	+3.0%
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	298,867	301,151	410,707	408,373
R^2	0.674	0.678	0.671	0.663

Notes: Sun-Abraham (Sun and Abraham, 2021) aggregate ATT estimates. Dependent variables are establishment entries, exits, job creation, and job destruction per county-sector-year cell from the 2005-2012 BDS. Treatment cohort defined by first year SC is active for a majority of months in county c; never-treated counties are the comparison group. All specifications include county, year, and sector fixed effects with the full control set, and are weighted by the 2000 Census baseline county population. Standard errors clustered at the county level. Observation counts vary across outcomes due to BDS cell-suppression thresholds. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Heterogeneity by Immigrant Concentration

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
I. By sector immigrant share				
Panel A: High immigrant-share sectors (above median, 10 sectors)				
SC Activation	−28.5**	−0.6	−649.8**	−125.4
	(11.4)	(25.1)	(271.6)	(899.7)
	[−17.9%]	[−0.3%]	[−19.9%]	[−2.7%]
Observations	167,648	170,002	230,055	229,067
R^2	0.793	0.797	0.745	0.741
Panel B: Low immigrant-share sectors (below median, 9 sectors)				
SC Activation	−5.9	−5.8	−98.4	383.0
	(7.3)	(13.8)	(143.8)	(292.3)
	[−4.5%]	[−4.2%]	[−4.1%]	[+14.8%]
Observations	131,219	131,149	180,652	179,306
R^2	0.560	0.558	0.601	0.595
II. By county immigrant share				
Panel C: High immigrant-share counties (above median, 1,549 counties)				
SC Activation	−19.4**	−2.5	−390.1**	216.4
	(7.8)	(21.9)	(175.1)	(689.8)
	[−11.7%]	[−1.3%]	[−11.7%]	[+5.1%]
Observations	164,889	165,686	212,141	211,202
R^2	0.680	0.684	0.674	0.666
Panel D: Low immigrant-share counties (below median, 1,549 counties)				
SC Activation	0.3	−0.3	−1.9	−5.7
	(0.3)	(0.3)	(6.7)	(7.2)
	[+3.4%]	[−4.0%]	[−1.4%]	[−4.0%]
Observations	133,978	135,465	198,566	197,171
R^2	0.644	0.638	0.500	0.511

Notes: Sun-Abraham (Sun and Abraham, 2021) aggregate ATT using 2005-2012 BDS data. Panels A-B split sectors at the median foreign-born low-education employment share (6.3%, ACS 1-year PUMS, 2005-2007). Panels C-D split counties at the median baseline foreign-born share. Percentage changes relative to pre-treatment treatment county means in square brackets. All specifications include county, year, and sector fixed effects with the full control set, and are weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Job Flow Decomposition: Births vs. Continuers

	Job Creation		Job Destruction	
	Estab. Births (1)	Continuers (2)	Estab. Deaths (3)	Continuers (4)
SC Activation	-61.3	-354.5^{**}	-19.3	164.6
	(45.3)	(171.0)	(169.4)	(476.0)
Pre-treatment Mean	1,158	1,860	1,266	2,534
% Change	-5.3%	-19.1%	-1.5%	$+6.5\%$
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	298,867	404,201	301,151	404,201
R^2	0.012	0.008	0.007	0.009

Notes: Sun–Abraham (Sun and Abraham, 2021) aggregate ATTs for the 2005–2012 BDS job flow decomposition. Job creation is decomposed into jobs created at establishments born in year t (births) and jobs created at establishments that existed in both $t-1$ and t (continuers). Job destruction is decomposed analogously into deaths and continuers. All specifications include county, year, and sector fixed effects with the full control set. Weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Heterogeneity by Sector Baseline Turnover Rate

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
Panel A: High baseline turnover sectors (above median, 10 sectors)				
SC Activation	-26.6^{**}	3.6	-589^{**}	-108
	(11.8)	(28.5)	(238)	(728)
	[−14.7%]	[+1.7%]	[−19.0%]	[−2.7%]
Observations	163,757	165,502	220,277	218,903
R^2	0.720	0.723	0.657	0.657
Panel B: Low baseline turnover sectors (below median, 9 sectors)				
SC Activation	-9.5^{*}	-5.7	-191	345
	(5.4)	(13.1)	(160)	(463)
	[−8.8%]	[−4.7%]	[−7.3%]	[+10.9%]
Observations	135,110	135,649	190,430	189,470
R^2	0.650	0.654	0.704	0.683
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Sun-Abraham (Sun and Abraham, 2021) aggregate ATT using 2005–2012 BDS data, estimated separately on high- and low-turnover sector subsamples. Sectors split at the median baseline turnover rate (9.7%, QWI 2005-2007 hire-weighted average). Turnover defined as the average quarterly ratio of hires plus separations to employment. Percentage changes relative to pre-treatment treatment county means in brackets. All specifications include county, year, and sector fixed effects with the full control set. Weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Heterogeneity by Tradable vs. Non-Tradable Sectors

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
Panel A: Tradable sectors				
SC Activation	-19.2	15.0	-367.5^{*}	185.2
	(16.0)	(21.5)	(218.0)	(494.5)
	[−15.4%]	[+10.2%]	[−15.0%]	[+5.6%]
Observations	100,639	101,078	143,110	141,773
R^2	0.389	0.422	0.535	0.532
Panel B: Non-tradable sectors				
SC Activation	-21.7^{***}	-7.5	-384.5^{**}	88.2
	(8.3)	(19.0)	(183.0)	(641.8)
	[−13.7%]	[−4.1%]	[−12.4%]	[+2.3%]
Observations	198,228	200,073	267,597	266,600
R^2	0.688	0.676	0.621	0.618
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Sun-Abraham (Sun and Abraham, 2021) aggregate ATT estimated separately on tradable and non-tradable sector subsamples. Panels A–B: 2005–2012 BDS establishment and job flow outcomes. Panel C: 2005–2012 QWI average weekly earnings (Wage in levels, Log Wage in logs). Tradable: agriculture (11), mining (21), manufacturing (31-33), wholesale (42), information (51), professional/technical (54), management (55). Non-tradable: all remaining sectors. Percentage changes relative to pre-treatment treatment county means in brackets. All specifications include county, year, and sector fixed effects with the full control set. Weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 6: Effect of SC Activation on Self-Employment

	Non-Citizen		U.S. Citizen		Total	
	Rate	Count	Rate	Count	Rate	Count
	(1)	(2)	(3)	(4)	(5)	(6)
SC Activation	-0.006^{*}	-240	-0.002^{**}	$-1,185^{*}$	-0.002^{**}	$-1,425^{*}$
	(0.003)	(417)	(0.001)	(714)	(0.001)	(842)
Pre-treatment Mean	0.093	11,662	0.090	45,390	0.090	57,052
% Change	−6.4%	−2.1%	−2.2%	−2.6%	−1.9%	−2.5%
County Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	24,569	24,748	24,748	24,748	24,569	24,748
R^2	0.248	0.998	0.856	0.999	0.852	0.999

Notes: Sun-Abraham (Sun and Abraham, 2021) aggregate ATT estimates. County-year panel created using 2005-12 ACS 1-year and PUMA-to-county crosswalks. Rate is the share of employed workers who are self-employed. Count is the number of self-employed workers. Columns (1)-(2): non-citizen workers. Columns (3)-(4): U.S. citizens. Columns (5)-(6): all workers. Self-employment defined as own-account work. All specifications include county and year fixed effects with the full set of control variables, and are weighted by the 2000 Census baseline county population. Standard errors clustered at the county level. Weighted by baseline county population. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

ONLINE SUPPLEMENTARY APPENDICES

Online Supplementary Appendices: Table of Contents

A Immigration Enforcement Control Variables

287(g) County. Section 287(g) of the Immigration and Nationality Act, added by IIRIRA in 1996, authorizes formal agreements between ICE and state and local law enforcement agencies allowing designated officers to perform specified immigration enforcement functions under federal supervision. County-level 287(g) agreements authorize designated local officers, often in jail settings and in some periods or jurisdictions in patrol or task-force roles, to identify and process removable immigrants encountered during routine law enforcement operations. We include an indicator for whether a county had an active 287(g) agreement in a given year to absorb enforcement variation operating through this channel, which predates and partially overlaps with the SC rollout.

287(g) State. In addition to county-level agreements, some states entered into 287(g) agreements at the state agency level, typically through state police or corrections departments. These agreements extend immigration enforcement authority across broader jurisdictions than county-level agreements and may affect counties that did not independently adopt 287(g) programs. We include a separate indicator for state-level 287(g) agreements to capture this additional layer of enforcement exposure that is not absorbed by the county-level indicator.

E-Verify. E-Verify is an internet-based employment eligibility verification system administered by the Department of Homeland Security that allows employers to confirm the work authorization status of newly hired employees by checking their information against federal databases. Unlike SC, which operates through the criminal justice system, E-Verify targets the employment relationship directly, raising the cost of hiring undocumented workers for participating employers. Several states mandated E-Verify participation for some or all employers during our sample period. We include an indicator for whether a state E-Verify mandate was active in a given year to ensure that our SC estimates are not confounded by this distinct employer-facing enforcement mechanism.

Omnibus Immigration Laws. Several states enacted comprehensive omnibus immigration legislation during our sample period that bundled multiple enforcement and restriction measures into single legislative acts. These laws varied in scope but often included provisions related to employment verification, eligibility verification for certain state and local public benefits, access to driver's licenses and educational benefits, and expanded local law enforcement authority to enforce immigration laws. Because omnibus laws alter the broader environment facing undocumented immigrants and may affect both labor supply and local demand conditions, we include an indicator for whether a state omnibus immigration law was active in a given year.

Sanctuary Policy. Sanctuary policies generally refer to formal policies adopted by city, county, or state governments that limit the cooperation of local law enforcement agencies with federal immigration enforcement authorities. These policies typically take the form of directives prohibiting local officers from inquiring about immigration status, declining to honor ICE detainer requests, or restricting information sharing with federal immigration authorities. Because sanctuary policies may partially offset the enforcement effects of SC by reducing the probability that local criminal justice contact leads to federal immigration consequences, we control for sanctuary policy status to isolate the effect of SC activation. For each county, we identify the earliest adoption date of any sanctuary policy at the city, county, or state level and define annual treatment as an indicator equal to one if a policy is in effect for at least six months of a given year.

B Construction of the Bartik Labor Demand Index

We construct a Bartik shift-share labor demand index following Bartik (1991) to control for differential local demand shocks that might confound the effect of immigration enforcement on business dynamics. The index interacts baseline county industry composition with national sectoral employment growth to generate predicted county-level labor demand, isolating demand variation that is plausibly exogenous to local enforcement decisions (Autor et al., 2013; Goldsmith-Pinkham et al., 2020).

Let $\text{emp}_{c,s,0}$ denote employment in county c, sector s in the base year (2001), and let $\text{emp}_{c,0} = \sum_s \text{emp}_{c,s,0}$ denote total county employment. The baseline industry share is

$$\omega_{c,s} = \frac{\text{emp}_{c,s,0}}{\text{emp}_{c,0}}. \tag{5}$$

We compute leave-one-out national sector employment growth to avoid mechanical correlation between the county's own employment and the national growth rate. Define $N_{-c,s,t} = \sum_{c' \neq c} \text{emp}_{c',s,t}$ as total national employment in sector s in year t, excluding county c. The cumulative growth ratio is

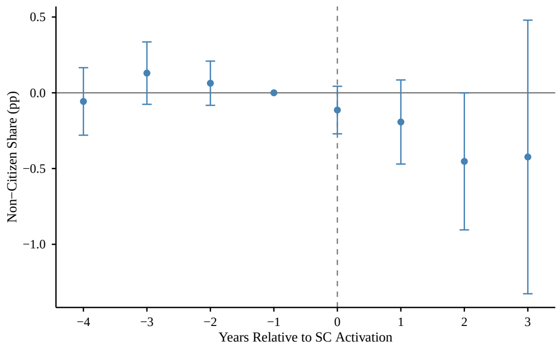
$$g_{-c,s,t} = \frac{N_{-c,s,t}}{N_{-c,s,0}}. \tag{6}$$

The Bartik index for county c in year t is then

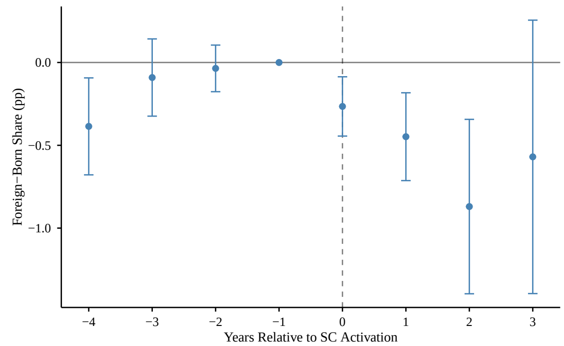
$$B_{c,t} = \sum_s \omega_{c,s} \cdot g_{-c,s,t}. \tag{7}$$

Values near one indicate that predicted labor demand in county c is close to its 2001 level; values above one indicate positive predicted demand growth. Employment data for both the baseline shares and the national growth rates come from the BDS. We use this index as a time-varying control in all regressions to absorb local labor demand shocks unrelated to enforcement.

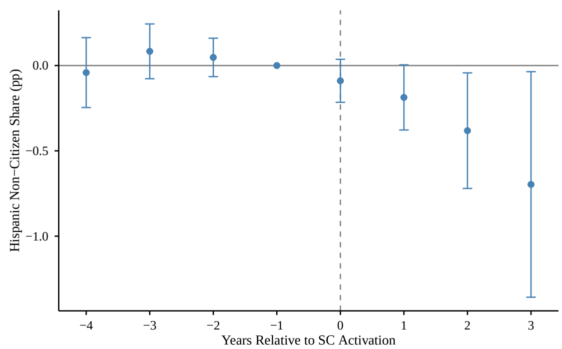
C Additional Figures and Tables



(a) Non-Citizen Share



(b) Foreign-Born Share



(c) Hispanic Non-Citizen Share

Figure C.1: First Stage: Effect of SC on Immigrant Population Shares

Notes. Sun-Abraham event study estimates of SC activation on county-level population shares (ACS 1-year, counties with pop. $\geq$ 65,000). Panel (a): non-citizen share; Panel (b): foreign-born share; Panel (c): Hispanic non-citizen share. Event time $k = 0$ marks SC activation; $k = -1$ is the omitted reference period. County and year fixed effects with the full control set. Weighted by baseline county population. Standard errors clustered at the county level. Bars show 95% confidence intervals.

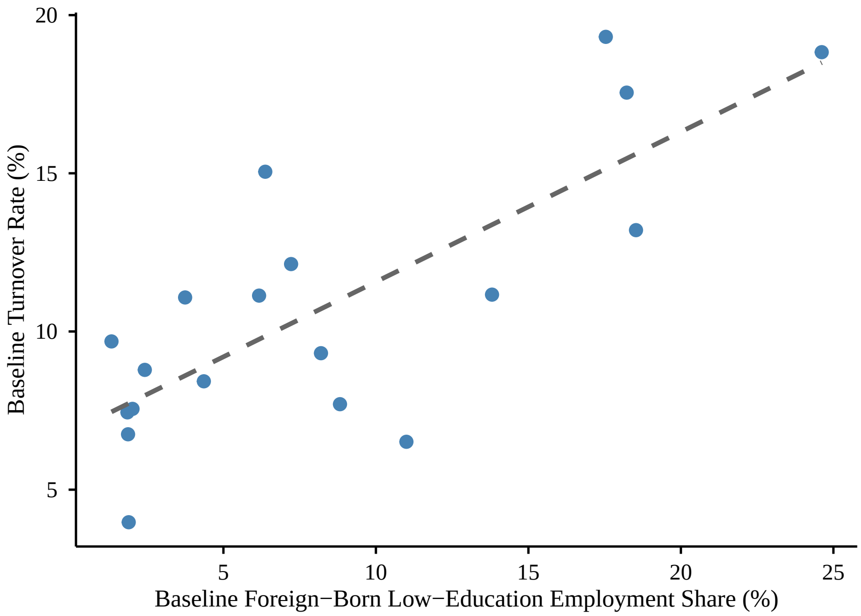
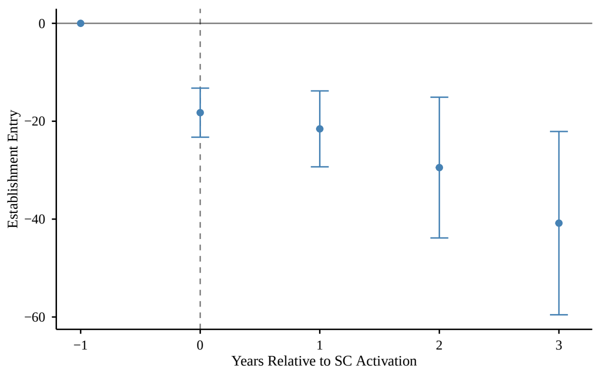


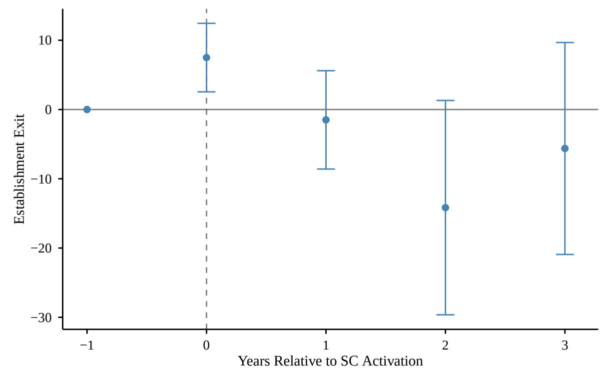
Figure C.2: Sector Immigrant Share and Baseline Turnover Rate

Notes. Each point is a 2-digit NAICS sector. The x-axis is the national share of employed workers who are foreign-born with at most a high school education (ACS 1-year PUMS, 2005-2007). The y-axis is the baseline turnover rate (average quarterly ratio of hires plus separations to employment, QWI 2005-2007, hire-weighted). Dashed line is OLS fit. Pearson correlation $r = 0.77$.

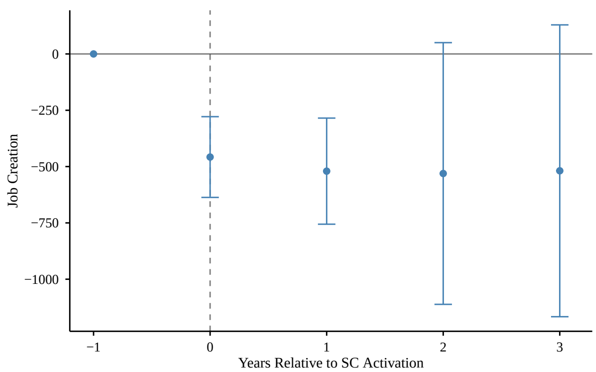
Figure C.3: Event Study: Borusyak–Jaravel–Spiess Imputation Estimator



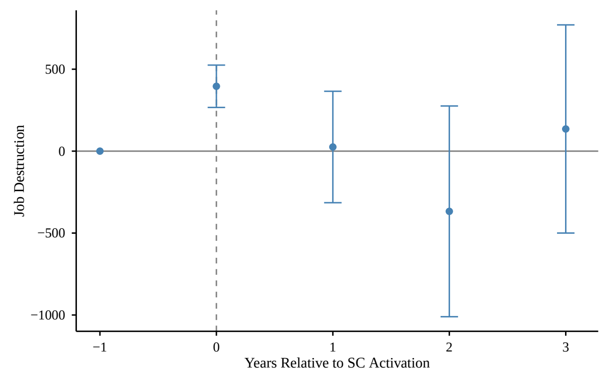
(a) Establishment Entry



(b) Establishment Exit



(c) Job Creation



(d) Job Destruction

Notes: Event-study coefficients from the Borusyak et al. (2024) imputation estimator. Each panel plots the dynamic treatment effect of SC activation on the indicated outcome, with 95% confidence intervals. Counterfactual outcomes for treated units are imputed from never-treated and not-yet-treated observations. County-sector-year panel, 2005-2012. Weighted by baseline county population. Standard errors clustered at the county level.

Table C.1: Descriptive Statistics

	Unweighted		Weighted	
	Mean	SD	Mean	SD
Panel A: Business Dynamics (County Level)				
No. of Firms	1,879	5,986	20,837	36,218
No. of Establishments	2,139	7,035	24,507	42,564
Establishment Entry	248	916	3,125	5,643
Establishment Exit	210	775	2,653	4,746
Job Creation	5,650	21,371	71,708	126,014
Job Destruction	5,094	19,417	66,087	117,802
N	3,147			
Panel B: Business Dynamics (County-Sector Level)				
No. of Firms	109	440	1,122	2,618
No. of Establishments	124	512	1,320	3,053
Establishment Entry	16	69	174	403
Establishment Exit	14	58	148	337
Job Creation	328	1,630	3,862	8,649
Job Destruction	298	1,401	3,564	8,134
N	59,793			
Panel C: Demographic Characteristics				
% Foreign-Born Non-Citizen	4.46	4.34	8.09	6.07
% Foreign-Born, ≤HS Education	3.32	4.99	8.57	8.26
% Hispanic Non-Citizen	3.45	3.66	5.18	4.55
N	3,147			

Notes. Weighted columns show the mean and standard deviation of the variables weighted by baseline county population. Panels A and B report pre-treatment averages (2005-2007) from the Business Dynamics Statistics (BDS), rounded to integers. Panel A aggregates across sectors within each county-year, then averages over the three years. Panel B reports county-sector-level averages. Panel C reports county-level demographic shares from the American Community Survey (ACS): % Foreign-Born Non-Citizen from ACS 1-year estimates (2005-2007); % Foreign-Born with ≤HS Education from ACS 5-year estimates (2006-2010); % Hispanic Non-Citizen from ACS 1-year estimates (2005-2007).

Table C.2: County Counts by Treatment Group, 2005–2012

	Counties
SC activated in 2009	51
SC activated in 2010	276
SC activated in 2011	1,026
SC activated in 2012	1,627
Never-treated (not activated by 2012)	176
Total counties	*3,156*

Notes: Panel A tabulates counties by first-activation year of Secure Communities; "Never-treated" denotes counties whose first activation falls after 2012. Panel B reports the count of counties ever exposed to each enforcement policy during 2005–2012. Policy indicators are coded 1 if active in a majority of months in a given year. Categories in Panel B are not mutually exclusive. Data: enforcement panel used in all main specifications.

Table C.3: Pre-Period Covariate Balance: Predictors of SC Adoption Timing

	First Treatment Year
Δ Establishments	-0.000^{**}
	(0.000)
Δ Share Non-Citizen	-0.019
	(0.026)
Δ Share Non-Hispanic White	0.013
	(0.021)
Δ Share Unemployed (LF)	-0.008
	(0.011)
Δ Log Housing Price Index	-0.159
	(0.869)
Republican Vote Share	-0.455
	(0.573)
Observations	2,690
R^2	0.167

Notes: OLS regression of first SC adoption year on 2005–2007 changes in county-level pre-period characteristics. All Δ variables reflect changes between 2005 and 2007. Share variables are expressed in percentage points. Regression weighted by 2000 Census baseline county population. Robust standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.4: Sectors by Baseline Immigrant Share

NAICS	Sector	Immigrant Share
Panel A: Above-Median Immigrant Share		
11	Agriculture, Forestry, Fishing & Hunting	0.246
23	Construction	0.185
72	Accommodation & Food Services	0.182
56	Admin., Support & Waste Mgmt.	0.175
81	Other Services	0.138
31-33	Manufacturing	0.110
42	Wholesale Trade	0.088
48-49	Transportation & Warehousing	0.082
44-45	Retail Trade	0.072
71	Arts, Entertainment & Recreation	0.064
Panel B: Below-Median Immigrant Share		
53	Real Estate & Rental/Leasing	0.062
62	Health Care & Social Assistance	0.044
21	Mining, Quarrying & Oil/Gas	0.037
51	Information	0.024
52	Finance & Insurance	0.020
22	Utilities	0.019
61	Educational Services	0.019
55	Management of Companies	0.019
54	Professional & Technical Services	0.013

Notes. Immigrant share is the fraction of sector employment that is foreign-born with at most a high school education, from the American Community Survey (ACS) 1-year PUMS, 2005–2007. Sectors are split at the median immigrant share across sectors.

Table C.5: First Stage: Effect of SC on Immigrant Population Shares

	Non-Citizen Share (1)	Foreign-Born Share (2)	Hispanic Non-Citizen (3)
SC Activation	-0.218	-0.453^{***}	-0.211^{**}
	(0.133)	(0.144)	(0.099)
Pre-treatment mean	7.591	13.387	5.084
% change	-2.9%	-3.4%	-4.2%
County Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Controls	Yes	Yes	Yes
Observations	6,118	6,223	3,836
R^2	0.991	0.997	0.989

Notes: Sun-Abraham (Sun and Abraham, 2021) aggregate ATT. County-year panel, 2005-2012, ACS 1-year estimates for counties with population $\geq$ 65,000. Non-citizen share: non-citizen population / total population. Foreign-born share: foreign-born population / total population. Hispanic non-citizen share: Hispanic non-citizen population / total population. All specifications include county and year fixed effects with the full control set. Weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.6: Effect of SC Activation on Average Weekly Wages: All Sectors (QCEW, Sun–Abraham)

	Wage (1)	Log Wage (2)
SC Activation	9.462	0.0081**
	(6.667)	(0.0037)
Pre-treatment mean	892	6.655
% change	[+1.1%]	
Observations	330,300	330,300
R^2	0.705	0.838
County Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Sector Fixed Effects	Yes	Yes
Controls	Yes	Yes

Notes: Sun–Abraham (Sun and Abraham, 2021) aggregate ATT of Secure Communities activation on average weekly wages from QCEW annual county-sector files, 2005–2012. Percentage changes in brackets computed as ATT divided by the pre-treatment mean. All specifications include county, year, and sector fixed effects, weighted by baseline county population (2000 Census). Standard errors clustered at the county level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.7: Effect of SC Activation on Average Weekly Wages: Heterogeneity (QCEW, Sun–Abraham)

Panel B: Tradable vs Non-Tradable Sectors	Non-Tradable		Tradable	
	Wage	Log Wage	Wage	Log Wage
	(1)	(2)	(3)	(4)
SC Activation	9.217**	0.0121***	17.830	0.0088
	(4.453)	(0.0037)	(12.211)	(0.0069)
Pre-treatment mean	749	6.490	1,150	6.955
% change	[+1.2%]		[+1.5%]	
Observations	219,456		110,834	
R^2	0.764	0.845	0.619	0.750

Panel B: Immigrant-Intensive vs. Non-Immigrant-Intensive	Imm-Intensive		Non-Imm-Intensive	
	Wage	Log Wage	Wage	Log Wage
	(5)	(6)	(7)	(8)
SC Activation	8.844*	0.0093**	9.631	0.0065
	(4.516)	(0.0043)	(13.699)	(0.0095)
Pre-treatment mean	708	6.465	1,106	6.877
% change	[+1.2%]		[+0.9%]	
Observations	197,228		133,056	
R^2	0.800	0.870	0.676	0.803
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Sun–Abraham (Sun and Abraham, 2021) aggregate ATT of Secure Communities activation on average weekly wages from QCEW annual county-sector files, 2005–2012. Tradable sectors include agriculture (11), mining (21), manufacturing (31–33), wholesale (42), information (51), professional/technical (54), and management (55). Immigrant-intensive sectors are defined as those above the cross-sector median share of foreign-born workers with at most a high school education (ACS 2009–2013). Percentage changes in brackets computed as ATT divided by the pre-treatment mean. All specifications include county, year, and sector fixed effects, weighted by baseline county population (2000 Census). Standard errors clustered at the county level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.8: Sectors by Baseline Turnover Rate

NAICS	Sector	Turnover Rate
Panel A: Above-Median Turnover		
56	Admin., Support & Waste Mgmt.	0.193
11	Agriculture, Forestry, Fishing & Hunting	0.188
72	Accommodation & Food Services	0.176
71	Arts, Entertainment & Recreation	0.150
23	Construction	0.132
44-45	Retail Trade	0.121
81	Other Services	0.112
53	Real Estate & Rental/Leasing	0.111
21	Mining, Quarrying & Oil/Gas	0.111
54	Professional & Technical Services	0.097
Panel B: Below-Median Turnover		
48-49	Transportation & Warehousing	0.093
51	Information	0.088
62	Health Care & Social Assistance	0.084
42	Wholesale Trade	0.077
52	Finance & Insurance	0.076
55	Management of Companies	0.074
61	Educational Services	0.068
31-33	Manufacturing	0.065
22	Utilities	0.040

Notes. Turnover rate is the hire-weighted national average of the quarterly ratio of hires plus separations to employment, from the Quarterly Workforce Indicators (QWI), 2005–2007. Sectors are split at the median turnover rate across sectors.

Table C.9: Effect of SC on Establishment and Firm Stock

	Establishments (1)	Firms (2)
SC Activation	-45.7***	-45.2***
	(17.1)	(16.4)
Pre-treatment mean	1217.0	1023.0
% change	-3.8%	-4.4%
County Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Sector Fixed Effects	Yes	Yes
Controls	Yes	Yes
Observations	417,803	417,803
R^2	0.661	0.652

Notes: Each column reports the Sun–Abraham (Sun and Abraham, 2021) aggregate ATT of Secure Communities activation on the indicated stock measure. The sample spans 2005–2012 at the county-sector-year level. All specifications include county, year, and sector fixed effects, weighted by baseline county population (2000 Census). Standard errors clustered at the county level in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.10: Heterogeneity by Establishment Size

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
Panel A: Small firms (1–19 employees)				
SC Activation	−280.1**	−65.7	−1,640.3***	−71.1
	(112.3)	(268.5)	(454.0)	(2,711.8)
	[−14.6%]	[−2.9%]	[−11.4%]	[−0.4%]
Observations	24,297	24,378	24,754	24,753
R^2	0.259	0.264	0.256	0.252
Panel B: Medium firms (20–99 employees)				
SC Activation	6.2	−29.8**	−3,241.9	−238.0
	(9.7)	(12.6)	(2,018.6)	(4,784.3)
	[+4.1%]	[−22.6%]	[−21.1%]	[−1.2%]
Observations	17,472	17,013	24,534	24,532
R^2	0.275	0.166	0.279	0.237
Panel C: Large firms (100+ employees)				
SC Activation	17.4	8.8	−2,878.0*	4,481.6
	(18.4)	(45.3)	(1,515.6)	(6,292.5)
	[+5.5%]	[+2.5%]	[−12.6%]	[+15.6%]
Observations	18,091	17,920	24,080	24,058
R^2	0.250	0.187	0.250	0.210
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Sun–Abraham (Sun and Abraham, 2021) aggregate ATT estimated separately for each firm size group using 2005–2012 BDS county-level data by firm size (coarse). Small: 1-19 employees; Medium: 20-99; Large: 100+. County-year panel (no sector dimension). Percentage changes relative to pre-treatment means in square brackets. All specifications include county and year fixed effects with the full control set. Weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.11: Heterogeneity by Establishment Age

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
Panel A: Young firms (0-5 years)				
SC Activation	-274.0^{***}	-109.6	$-3{,}046.3^{***}$	-942.3
	(69.9)	(214.1)	(841.7)	(2,902.1)
	[−15.8%]	[−8.4%]	[−18.4%]	[−7.5%]
Observations	24,454	23,262	24,689	24,604
R^2	0.283	0.276	0.276	0.235
Panel B: Mature firms (6-10 years)				
SC Activation	-0.9	7.7	-576.2	182.6
	(22.3)	(37.0)	(493.2)	(1,471.9)
	[−0.7%]	[+1.8%]	[−12.7%]	[+2.4%]
Observations	17,234	20,713	24,519	24,511
R^2	0.234	0.231	0.259	0.216
Panel C: Old firms (11+ years)				
SC Activation	14.6	15.1	$-4{,}131.8^{*}$	4,911.0
	(21.1)	(60.7)	(2,486.7)	(9,215.5)
	[+2.8%]	[+1.5%]	[−13.1%]	[+10.5%]
Observations	22,332	23,668	24,740	24,739
R^2	0.208	0.200	0.253	0.237
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Sun-Abraham (Sun and Abraham, 2021) aggregate ATT estimated separately for each firm age group using 2005–2012 BDS county-level data by firm age (coarse). Young: 0–5 years; Mature: 6–10 years; Old: 11+ years (includes left-censored firms). County-year panel (no sector dimension). Percentage changes relative to pre-treatment means in square brackets. All specifications include county and year fixed effects with the full control set. Weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.12: Spatial Robustness: Commuting Zone Tests

	Establishments		Jobs	
	Entry	Exit	Creation	Destruction
	(1)	(2)	(3)	(4)
Panel A: County + Year + Sector FE (baseline)				
SC Activation	-18.1**	-2.8	-394.4**	109.7
	(7.7)	(19.6)	(186.5)	(594.0)
Pre-treatment mean	146.7	168.4	2,864.4	3,641.8
% change	-12.3%	-1.7%	-13.8%	+3.0%
Observations	298,867	301,151	410,707	408,373
R^2	0.674	0.678	0.671	0.663
Panel B: County + CZ × Year + Sector FE				
SC Activation	-17.8***	10.2	-316.4***	679.9**
	(5.1)	(10.0)	(117.7)	(301.3)
Pre-treatment mean	146.7	168.4	2,864.4	3,641.8
% change	-12.2%	+6.0%	-11.0%	+18.7%
Observations	298,867	301,151	410,707	408,373
R^2	0.677	0.680	0.676	0.669
Panel C: CZ + Year + Sector FE (commuting zone level)				
SC Activation	-57.0***	18.6	-1,032.3***	2,339.7*
	(20.6)	(31.3)	(333.6)	(1,219.3)
Pre-treatment mean	344.3	402.0	7,431.5	9,624.0
% change	-16.6%	+4.6%	-13.9%	+24.3%
Observations	109,592	109,592	109,592	109,592
R^2	0.674	0.679	0.696	0.690
Controls	Yes	Yes	Yes	Yes

Notes: Panel A reproduces the baseline Sun–Abraham aggregate ATTs with county, year, and sector fixed effects. Panel B replaces year fixed effects with commuting zone × year fixed effects, absorbing all time-varying confounds at the labor market level. Panel C aggregates the panel to the commuting zone × sector × year level and re-estimates with CZ, year, and sector fixed effects; the treatment cohort is the year when the cumulative population-weighted share of constituent counties under SC exceeds 50 percent; CZ clustering. Commuting zones follow Tolbert and Sizer (1996). All specifications include the full control set, weighted by baseline population. Standard errors clustered at the county level in Panels A–B, at the CZ level in Panel C. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.13: Missing Values in Outcome Variables

	Total Panel	Non-missing	Missing	% Missing
Establishment entry	479,712	304,709	175,003	36.48
Establishment exit	479,712	306,697	173,015	36.07
Job creation	479,712	417,803	61,909	12.91
Job destruction	479,712	415,348	64,364	13.42
Job creation (births)	479,712	304,709	175,003	36.48
Job destruction (deaths)	479,712	306,697	173,015	36.07

Notes: The table reports the total number of observations, non-missing observations, missing observations, and the percentage of missing observations for each outcome variable in the county-sector-year panel.

Table C.14: Balanced Panel Estimates

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
SC Activation	-18.1^{**}	-2.8	-394.4^{**}	109.7
	(7.7)	(19.6)	(186.5)	(594.0)
Pre-treatment mean	146.7	168.4	2864.4	3641.8
% change	−12.3%	−1.7%	−13.8%	+3.0%
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	298,867	301,151	410,707	408,373
R^2	0.674	0.678	0.671	0.663

Notes: Sun–Abraham (Sun and Abraham, 2021) aggregate ATTs estimated on the same specification as Table 1, restricted to the balanced panel of county-sector cells observed in every sample year. All specifications weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.15: Missing Data Selection Test

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
SC Activation	-0.010	0.021**	0.0084	0.008
	(0.009)	(0.010)	(0.006)	(0.006)
Baseline Missing Rate	36.5%	36.1%	12.9%	13.4%
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	470,440	470,440	470,440	470,440
R^2	0.312	0.308	0.181	0.188

Notes: Dependent variable is an indicator for whether the county-sector-year cell is suppressed in the BDS. A significant positive coefficient would indicate that SC activation increases the probability of cell suppression. All specifications weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.16: Robustness: Trimming Bounds

	Estab. Entry	Estab. Exit	Job Creation	Job Destruction
Panel A: Trim bottom 1%				
SC Activation	-18.2**	-2.7	-390.4**	136.4
	(8.0)	(19.7)	(185.2)	(597.9)
	[-12.2%]	[-1.6%]	[-13.5%]	[+3.7%]
Observations	265,466	269,199	375,822	375,819
R^2	0.676	0.680	0.672	0.665
Panel B: Trim bottom 5%				
SC Activation	-17.8**	-2.5	-431.0**	117.6
	(8.0)	(19.8)	(197.2)	(593.5)
	[-11.9%]	[-1.5%]	[-14.7%]	[+3.2%]
Observations	256,804	259,266	361,272	361,272
R^2	0.677	0.681	0.673	0.666
Panel C: Trim bottom 10%				
SC Activation	-15.9**	-4.2	-439.9**	132.7
	(7.4)	(19.3)	(196.1)	(594.3)
	[-10.5%]	[-2.4%]	[-14.8%]	[+3.5%]
Observations	244,985	247,102	341,473	341,473
R^2	0.679	0.683	0.675	0.668
County Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Sector Fixed Effects	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Sun–Abraham (Sun and Abraham, 2021) aggregate ATTs after trimming county-sector cells with the smallest baseline establishment counts. Panel A: bottom 1% trimmed. Panel B: bottom 5 percent. Panel C: bottom 10 percent. Percentage changes in brackets. All specifications include county, year, and sector fixed effects, weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.17: Borusyak-Jaravel-Spiess Imputation Estimates

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
BJS ATT	-27.5^{***}	-3.4	-506.9^{**}	47.2
	(6.5)	(6.0)	(234.5)	(248.5)
Pre-treatment Mean	147	168	2,864	3,642
% Change	−18.8%	−2.0%	−17.7%	+1.3%
Observations	304,709	306,697	417,803	415,348
R^2	0.986	0.986	0.966	0.968

Notes: Borusyak et al. (2024) imputation estimator. Counterfactual outcomes for treated units are imputed using never-treated and not-yet-treated observations. ATT is the average of post-treatment event-time coefficients. County-sector-year panel, 2005–2012. Weighted by baseline county population. Standard errors in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.18: Robustness: Year FE with Controls vs. State × Year FE

	Establishments		Jobs	
	Entry	Exit	Creation	Destruction
	(1)	(2)	(3)	(4)
Panel A: Using Year Fixed Effects (Primary)				
SC Activation	-18.1**	-2.8	-394.4**	109.7
	(7.7)	(19.6)	(186.5)	(594.0)
Panel B: Using State-by-Year Fixed Effects (Robustness)				
SC Activation	-17.9***	6.5	-360.7**	510.4
	(5.2)	(12.8)	(144.2)	(437.3)
Observations	298,867	301,151	410,707	408,373

Notes: Panels A and B report Sun-Abraham aggregate ATTs using county, sector, and year fixed effects, and county, sector, and state-by-year fixed effects, respectively, along with a full set of control variables. Weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

D Selection on Unobservables

A persistent concern in panel identification is that unobserved county-level trends correlated with SC intensity could drive the results. We assess the robustness of our estimates to selection on unobservables using Oster (2019)'s δ statistic, which measures how much stronger selection on unobservables (relative to observables) would need to fully explain the estimated effect, under the assumption that the maximum R^2 attainable from a fully-specified model is R^2_{max}.

Table D.1 reports δ under two assumptions for R^2_{max}: $1.3 \times R^2_{\text{controlled}}$ (conservative) and $2.0 \times R^2_{\text{controlled}}$ (generous). The analysis focuses on establishment entry and job creation, the two headline outcomes that are statistically significant in the main specification.

For establishment entry, $\delta = -732.2$ at $R^2_{\text{max}} = 1.3 \times R^2_{\text{controlled}}$, rising in magnitude to $\delta = -2{,}440.7$ under the more generous assumption. Adding controls reduces the coefficient magnitude (from -23.0 to -17.0) while R^2 barely moves (0.672 to 0.673). The negative sign of δ means that unobservables would need to work in the *opposite direction* from observables to explain away the effect. The extreme magnitude reflects the near-stability of R^2 when controls are added: the observables barely move the model fit, so unobservable selection would need to be over seven hundred times as influential as observable selection to nullify the result.

For job creation, $\delta = -408.5$ at the conservative R^2_{max}, rising in magnitude to $-1{,}361.7$ under the generous assumption. Adding controls reduces the coefficient magnitude (from -740.3 to -555.5) while R^2 increases slightly from 0.668 to 0.670. Again, the negative δ implies that unobservables would need to work in the opposite direction from observables, and with a magnitude over four hundred times as large.

In both cases, $|\delta| \gg 1$, well above the standard robustness threshold proposed by Oster (2019). The bounds are inconsistent with a standard omitted-variable story and consistent with the identification design we argue for without claiming to rule out all possible confounds.

Table D.1: Oster (2019) Bounds

	Estab. Entry	Job Creation
$\hat{\beta}^{\text{naive}}$	-23.0	-740.3
R^2_{naive}	0.672	0.668
$\hat{\beta}^{\text{controlled}}$	-17.0	-555.5
$R^2_{\text{controlled}}$	0.673	0.670
δ (1.3×)	-732	-409
δ (2.0×)	$-2{,}441$	$-1{,}362$
County FE	Yes	Yes
Year FE	Yes	Yes
Sector FE	Yes	Yes
Controls	Yes	Yes

Notes: Oster (2019) bounds for the two significant outcomes. $\hat{\beta}^{\text{naive}}$ includes fixed effects only; $\hat{\beta}^{\text{controlled}}$ adds the full control set. δ is the proportional selection ratio at $R^2_{\max} = 1.3 \times R^2_{\text{controlled}}$ and $2.0 \times R^2_{\text{controlled}}$. $|\delta| > 1$ implies robustness. Negative δ means unobservables would need to work in the opposite direction from observables to nullify the effect.

E Conley Spatial Standard Errors

County-level outcomes may exhibit positive spatial autocorrelation: neighboring counties share common local economic shocks, and the Great Recession propagated through regional supply chains and housing markets in spatially clustered patterns. Standard errors clustered at the county level account for serial correlation within counties but not for cross-county spatial correlation. We address this using the spatial HAC estimator of Conley (1999), which allows arbitrary correlation across county pairs within a specified geographic cutoff.

Table E.1 reports estimates for all four outcomes under county clustering (baseline) and Conley SEs at 100, 200, and 300 km cutoffs, using county centroids from the Census Gazetteer file. The Conley standard errors are close to the county-clustered standard errors across all cutoffs, reflecting that residual spatial correlation beyond county boundaries is modest. Establishment entry remains significant at the 5% level under all cutoffs. Job creation remains significant at the 5–10% level. The headline results are not sensitive to the treatment of spatial autocorrelation.

Table E.1: Robustness to Conley Spatial Standard Errors

	Establishments				Jobs			
	Entry		Exit		Creation		Destruction	
Variance estimator	Coef.	SE	Coef.	SE	Coef.	SE	Coef.	SE
County cluster (baseline)	-17.0^{**}	(7.1)	0.1	(4.0)	-555.5^{**}	(276.2)	15.5	(195.6)
Conley 100 km	-17.0^{**}	(7.6)	0.1	(4.4)	-555.5^{*}	(284.5)	15.5	(202.8)
Conley 200 km	-17.0^{**}	(7.6)	0.1	(4.0)	-555.5^{*}	(299.6)	15.5	(200.9)
Conley 300 km	-17.0^{**}	(7.9)	0.1	(3.7)	-555.5^{*}	(302.4)	15.5	(206.1)

Notes: Each row reports the same point estimates from the main specification with a different variance estimator. Conley SEs use spherical distances between county centroids (Census 2020 Gazetteer) with the indicated radius cutoff (Conley, 1999). All specifications include county, year, and sector fixed effects with the full control set. Weighted by baseline county population. Estimation sample: 2005–2012. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

F Alternative Clustering

Our baseline standard errors are clustered at the county level. Table F.1 reports estimates with two alternatives: state-level clustering and two-way clustering by county and year. Point estimates are identical across rows by construction. Establishment entry and job creation remain significant under all three variance estimators. Appendix E reports Conley (Conley, 1999) spatial standard errors.

Table F.1: Robustness to Alternative Clustering

	Establishments		Jobs	
	Entry (1)	Exit (2)	Creation (3)	Destruction (4)
County clustering	-16.973**	0.108	-555.509**	15.476
	(7.129)	(3.972)	(276.163)	(195.577)
State clustering	-16.973**	0.108	-555.509**	15.476
	(7.274)	(3.405)	(264.223)	(192.500)
Two-way (county & year)	-16.973*	0.108	-555.509	15.476
	(8.242)	(3.015)	(313.887)	(267.367)
Observations	298,867	301,151	410,707	408,373
R^2	0.673	0.677	0.669	0.661
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes

Notes: Each row reports the same point estimates from the main specification with a different variance estimator. All specifications include county, year, and sector fixed effects with the full control set. Weighted by baseline county population. Estimation sample: 2005–2012. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

G Unweighted Estimates

Table G.1 compares weighted (Panel A) and unweighted (Panel B) estimates. Unweighted results are qualitatively similar: establishment entry and job creation remain significant, with job destruction also reaching significance without weights. The robustness of results to weighting confirms that the findings are not driven by a few large early-adopter jurisdictions.

Table G.1: Robustness: Weighted vs. Unweighted Estimates

	Establishments		Jobs	
	Entry	Exit	Creation	Destruction
Panel A: Weighted by baseline population				
Secure Communities	-16.973**	0.108	-555.509**	15.476
	(7.129)	(3.972)	(276.163)	(195.577)
Observations	298,867	301,151	410,707	408,373
R^2	0.673	0.677	0.670	0.662
Panel B: Unweighted				
Secure Communities	-2.399***	0.051	-62.909***	-24.029**
	(0.394)	(0.302)	(11.151)	(9.560)
Observations	298,867	301,151	410,707	408,373
R^2	0.617	0.624	0.606	0.602
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes

Notes: Panel A weights by baseline county population (2000 Census); Panel B is unweighted. Both panels use the main specification with the full control set. County, year, and sector fixed effects. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

H Leave-One-Cohort-Out Stability

Table H.1 shows that the main results are not driven by any single activation cohort. We re-estimate the Sun–Abraham aggregate ATT on each of the four gross outcomes after dropping one cohort at a time. Establishment entry remains significant at the 1% level in every subsample, with point estimates tightly bracketing the full-sample ATT of -16.2. Job creation remains significant (at least $p < 0.10$) in every subsample. No single cohort drives the headline findings.

Table H.1: Leave-One-Cohort-Out Robustness

	Estab. Entry	Estab. Exit	Job Creation	Job Destruction
Full sample	-18.1**	-2.8	-394.4**	109.7
	(7.7)	(19.6)	(186.5)	(594.0)
Drop 2009	-17.7*	-1.6	-572.2*	347.6
	(9.7)	(21.0)	(347.1)	(704.2)
Drop 2010	-21.7***	1.9	-400.5***	-62.7
	(6.4)	(10.4)	(147.0)	(202.2)
Drop 2011	-18.7**	-11.0	-176.2**	-222.2
	(9.3)	(21.7)	(88.0)	(666.5)
Drop 2012	-17.7**	-2.7	-410.7**	208.1
	(7.2)	(22.9)	(187.2)	(761.1)
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: Each row drops the indicated SC activation cohort and re-estimates the Sun–Abraham (Sun and Abraham, 2021) aggregate ATT on the remaining sample. All specifications include county, year, and sector fixed effects, weighted by baseline county population. Standard errors clustered at the county level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

I A Simple Model of Entry and Exit under Immigration Enforcement

This appendix provides an organizing framework for the empirical results. The core mechanism is a negative local labor supply shock: enforcement reduces the effective immigrant workforce through removals, induced out-migration, and reduced inflows. The main propagation channel is hiring frictions. When the local labor pool contracts, recruiting becomes more costly, and firms that depend on continuous hiring are disproportionately affected. Two complementary channels may reinforce this effect: a decline in local consumption demand from the departure of immigrant households, and a direct reduction in immigrant entrepreneurship through removal and deterrence. The model is partial-equilibrium with labor-only production and is not intended as a structural model to be estimated.

Environment

Consider a local economy in county c with two sectors, $s \in \{T, N\}$, where T denotes tradable and N denotes non-tradable. Firms produce using labor only:

$$y = a\, n^{\alpha}, \qquad 0 < \alpha < 1, \tag{8}$$

where a is firm-specific productivity and n is labor input. Potential entrepreneurs are of two types, $j \in \{I, D\}$, where I denotes immigrant and D denotes domestic (native-born). Both types draw productivity from the same distribution $G(a)$ with density $g(a)$.

Two baseline exposure measures govern the strength of the enforcement shock. Let $M_c \in [0, 1]$ denote the baseline immigrant share in county c, and let $\Gamma_s \in [0, 1]$ denote sector s's baseline dependence on immigrant labor. Enforcement intensity $\mathcal{E}_c \geq 0$ reduces the effective local labor pool by a fraction that is increasing in both M_c and Γ_s.

The key departure from a standard competitive labor market model is a recruiting friction. Hiring requires costly search, and the per-hire recruiting cost depends on the tightness of the local labor market. This friction is the central channel through which the labor supply shock propagates to firm entry and expansion decisions.

Labor Supply and Recruiting Frictions

Let L_s denote the effective labor pool available to sector s. Enforcement reduces L_s by removing immigrant workers:

$$L_s(\mathcal{E}_c) = L_s^0 - \phi_s(\mathcal{E}_c, M_c, \Gamma_s), \qquad \phi_s > 0, \quad \frac{\partial \phi_s}{\partial \mathcal{E}_c} > 0, \tag{9}$$

where L_s^0 is the pre-enforcement labor pool and ϕ_s captures the reduction in available workers, which is increasing in enforcement intensity and in baseline exposure (M_c, Γ_s).

Firms that wish to hire n workers incur a recruiting cost:

$$R(n, L_s) = \frac{\kappa}{2} \cdot \frac{n^2}{L_s}, \tag{10}$$

where $\kappa > 0$ governs the severity of recruiting frictions. The cost is increasing and convex in n (more hires are progressively harder to make) and decreasing in L_s (a larger labor pool makes each hire easier). This reduced-form specification captures the idea that recruiting is a bilateral process: when fewer workers are available, each vacancy takes longer to fill and the cost per hire rises. The quadratic form ensures that firms with larger hiring needs are disproportionately affected[20]

[20] This formulation can be microfounded through a standard matching function framework (Mortensen and Pissarides, 1994; Pissarides, 2000), where the probability of filling a vacancy is decreasing in the vacancy-to-worker ratio. As the labor pool shrinks, the matching rate falls and firms must post more vacancies (at increasing cost) to achieve the same number of hires. We use the reduced form for tractability. See Hopenhayn et al. (2022) and Karahan et al. (2019) for equilibrium models of firm entry and exit in which labor supply growth governs the startup rate without affecting incumbent exit behavior, precisely the asymmetric pattern we document empirically

The key implication is that enforcement raises recruiting costs through a compositional effect on the labor pool:

$$\frac{\partial R}{\partial \mathcal{E}_c} = -\frac{\kappa}{2} \cdot \frac{n^2}{L_s^2} \cdot \frac{\partial L_s}{\partial \mathcal{E}_c} > 0. \tag{11}$$

Because $\partial L_s / \partial \mathcal{E}_c < 0$, the recruiting cost rises. The increase is larger when baseline immigrant exposure (M_c, Γ_s) is higher and when the firm's hiring need n is larger.

Wages. Enforcement also raises equilibrium wages. The labor supply contraction shifts the supply curve inward against a stable demand curve, increasing the wage in both sectors. In tradable sectors, where the output price is exogenous, the wage increase is unambiguous. In non-tradable sectors, the wage increase is attenuated if local demand also falls (see the discussion of local demand below), but the net effect on wages remains positive as long as the supply contraction dominates the demand contraction. The empirical evidence in Section 6 shows that wages rise in both sectors, consistent with the labor supply channel being the primary force.

Impact on profits. A firm with productivity a that must recruit n workers earns flow profit

$$\pi(a, n, w_s, L_s) = p_s \cdot a\, n^{\alpha} - w_s\, n - R(n, L_s), \tag{12}$$

where p_s is the output price. Enforcement reduces profits through two reinforcing channels: higher wages w_s and higher recruiting costs R. The recruiting cost channel is absent from a frictionless model and creates differential effects across firms with different hiring needs.

Entry versus Incumbent Adjustment

The distinction between recruiting costs and wage levels generates an asymmetry between entrants and incumbents that is central to the empirical results.

Entrants face the full recruiting friction. A potential entrant in sector s pays a sunk cost $F_s > 0$, draws productivity a, and must recruit an entire workforce to begin operating. The entry condition requires that the expected present value of profits, net of recruiting costs, covers the sunk cost:

$$V_s(a) - R(n_s^*(a),\, L_s) \geq F_s, \tag{13}$$

where $V_s(a)$ is the present value of the ongoing profit stream and $n_s^*(a)$ is the optimal workforce size. The entry cutoff $\bar{a}_s$ satisfies this condition with equality. Because entrants must recruit their full workforce from scratch, the recruiting cost R directly raises the entry threshold. Enforcement increases $\bar{a}_s$ through both the wage channel (which reduces V_s) and the recruiting friction (which raises R).

Incumbents adjust on the intensive margin. An incumbent firm has already recruited its workforce and paid the sunk cost F_s. It continues operating if the flow payoff exceeds per-period operating costs $f_s \geq 0$:

$$\pi(a, n, w_s, L_s) - f_s \geq 0. \tag{14}$$

The incumbent's workforce is already in place. It faces recruiting costs only on replacement hires (turnover) and marginal expansion, not on its entire labor force. For a moderate enforcement shock, the wage increase and the recruiting cost on replacement hires reduce profits but do not push most incumbents below the shutdown threshold. Firms respond by slowing hiring and operating at a smaller scale rather than exiting.

Expanding incumbents face a partial friction. Continuing establishments that wish to grow must recruit additional workers and therefore face the recruiting friction on new hires. This explains why the empirical decline in job creation is driven almost entirely by continuing establishments reducing expansion (Table 3), rather than by a collapse in birth-related job creation.

The resulting pattern is: lower entry, lower job creation (especially at continuing establishments), and near-zero exit and job destruction. This matches the central empirical finding in Section 5.

The Role of Turnover

The recruiting friction creates a natural link between sector-level turnover rates and sensitivity to enforcement. In sectors with high baseline turnover, firms must continuously recruit replacement workers simply to maintain their current scale. These sectors are exposed to the recruiting friction not only at entry but on an ongoing basis. When the labor pool shrinks, high-turnover firms face a sustained increase in recruiting costs that low-turnover firms avoid.

Let $\tau_s \in [0, 1]$ denote the baseline separation rate in sector s. Each period, an incumbent firm with workforce n must recruit $\tau_s \cdot n$ replacement workers. The per-period recruiting cost for incumbents is

$$R_{\text{inc}}(\tau_s, n, L_s) = \frac{\kappa}{2} \cdot \frac{(\tau_s \cdot n)^2}{L_s}. \tag{15}$$

This cost is increasing in τ_s: high-turnover sectors bear a larger ongoing recruiting burden. Enforcement amplifies this burden because L_s falls, raising the cost of each replacement hire. For entrants, the effect is even larger because they must recruit their entire workforce (n, not $\tau_s \cdot n$).

Turnover is therefore not a separate mechanism but a channel that amplifies the labor supply shock. The model predicts that enforcement effects on both entry and job creation are increasing in τ_s, consistent with the empirical results in Table 4.

Exposure Heterogeneity

The labor supply shock and its propagation through recruiting frictions both depend on baseline immigrant exposure. The labor pool contraction ϕ_s is increasing in the county immigrant share M_c and the sector immigrant dependence Γ_s. The recruiting cost increase $\partial R/\partial \mathcal{E}_c$ is therefore also increasing in these exposure measures.

The model predicts that enforcement effects on entry and job creation are stronger when:

- The county immigrant share M_c is higher (larger labor supply shock);
- The sector immigrant labor dependence Γ_s is higher (greater exposure to the shock);
- The sector turnover rate τ_s is higher (greater dependence on continuous hiring);
- These exposure measures interact: high-M_c counties with high-Γ_s sectors experience the largest effects.

Complementary Channel: Immigrant Entrepreneurship

Enforcement can reduce business formation directly by removing or deterring immigrant entrepreneurs. Each period, an immigrant entrepreneur faces probability $\rho_I(\mathcal{E}_c, M_c) \in [0, 1)$ that enforcement forces cessation of operations through detention, deportation, or induced departure. Native entrepreneurs face no enforcement risk: $\rho_D = 0$. This reduces the expected present value of an immigrant-owned firm:

$$V_s^I(a) = \frac{\pi_s(a)}{1 - \beta(1 - \rho_I)} < \frac{\pi_s(a)}{1 - \beta} = V_s^D(a), \tag{16}$$

where $\beta \in (0, 1)$ is the discount factor. Immigrant entrepreneurs require higher productivity to justify entry. Because immigrants start firms at higher rates than natives (Kerr and Kerr, 2020; Fairlie and Lofstrom, 2015), and emigration shocks reduce firm creation in origin communities (Anelli et al., 2023), this channel contributes to the aggregate decline in establishment entry. The decline in non-citizen self-employment, combined with a significant decline in citizen self-employment (Table 6), is consistent with enforcement operating through the labor supply channel: fewer available workers suppress entrepreneurship broadly, not only among those directly targeted.

This channel is complementary to the labor supply mechanism, not foundational. It deepens entry losses beyond what the labor market channel alone would produce, but the core predictions of the model do not depend on it.

Complementary Channel: Local Demand

If enforcement causes immigrant households to leave a county, local consumer spending may fall. This would reduce revenue for non-tradable firms that depend on local demand, while leaving tradable firms (which sell to broader markets) largely unaffected (Moretti, 2010).

If enforcement also reduces local spending, non-tradable firms face a compounding effect: both higher labor costs from the supply contraction and potentially lower revenue from reduced local demand. This reinforcement may explain why effects in non-tradable sectors are more precisely estimated than in tradable sectors (Table 5).

Two points of clarification are important. First, declining entry, declining job creation, near-zero exit, concentration in immigrant-exposed settings, follow entirely from the labor supply and recruiting friction channels. The demand channel is not necessary for these results; it may strengthen them in non-tradable sectors. Second, the demand channel does not reverse the wage prediction. As long as the labor supply contraction is the dominant force, wages rise in both tradable and non-tradable sectors. The empirical evidence is consistent with this (Section 6).