

# Immigration Enforcement and Local Business Dynamics\*

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## Abstract

This paper examines the impact of interior immigration enforcement on local business dynamics. Exploiting the rollout of the Secure Communities program across U.S. counties between 2008 and 2013, we use county-sector-year data on establishment and employment flows from U.S. Census administrative records. Enforcement reduces establishment entry and job creation, with no statistically significant effect on establishment exit or job destruction, indicating that the effects operate primarily at the entry and hiring margins. We provide evidence consistent with a contraction in local labor supply that raises hiring frictions and constrains firms' ability to recruit workers, especially in settings reliant on continuous hiring. We also find suggestive evidence of a complementary local demand channel, with larger effects in non-tradable sectors. Establishments reduce hiring rather than exit, implying adjustment along the intensive margin. These results indicate that immigration enforcement acts as a negative local labor supply shock that propagates through business formation and local economic activity.

**JEL Codes:** K37, R23, D22, J21

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# 1 Introduction

The 1996 Illegal Immigration Reform and Immigrant Responsibility Act contributed to a substantial expansion of immigration enforcement capacity in the United States, particularly in the federal government’s ability to identify, detain, and remove undocumented immigrants. Over the following decades, enforcement capacity increased markedly, enabling local and federal authorities to enforce immigration laws more effectively. A growing empirical literature shows that these policies reduce the undocumented population in affected areas and generate meaningful changes in local labor markets (Bohn et al., 2014; East et al., 2023; Orrenius and Zavodny, 2015a; East and Velásquez, 2024). Yet this evidence is overwhelmingly focused on worker outcomes. We know much less about how enforcement-induced labor supply shocks affect firm dynamics, despite the fact that many industries rely heavily on low-skill immigrant labor.

Understanding how firms respond to labor supply shocks is central to assessing the broader economic effects of immigration policy. Firms are the primary margin through which local labor market changes translate into production, employment, and aggregate economic activity. A reduction in the availability of low-skill immigrant labor may constrain firms’ ability to hire and expand, alter the pace of business formation, or affect the reallocation of jobs across establishments. These responses, subsequently, determine how local economies adjust to enforcement-induced changes in labor supply.

In this paper, we estimate the effects of U.S. interior immigration enforcement on business entry and exit, and on job creation and destruction, using variation in the rollout of Secure Communities (SC) across counties from 2008 to 2013. Secure Communities is a federal program that automatically checks the immigration status of individuals booked into local jails against federal databases, enabling their identification and potential removal. The program is the most geographically comprehensive enforcement initiative of this period, and its activation schedule was determined federally by U.S. Immigration and Customs Enforcement (ICE) rather than by local adoption decisions, providing a source of variation

that is largely insulated from local economic conditions. We combine SC activation with U.S. Census Bureau data in a county-sector-year panel and estimate its effects using a staggered difference-in-differences design. Our identification strategy exploits within-county variation in enforcement exposure, conditional on county, year, and sector fixed effects. The key identifying assumption is that, conditional on these controls, the timing of SC activation is uncorrelated with unobserved shocks to county-sector business outcomes.

We find that interior immigration enforcement significantly contracts local business dynamism, operating primarily through the entry and hiring margins rather than firm exit. SC reduces establishment entry by about 11 percent and job creation by roughly 12 percent, with no corresponding increase in exit or job destruction. This asymmetry is central: SC does not force firms out of business, but instead prevents new firms from forming and constrains the expansion of existing ones. Consistent with a broader literature showing that adjustment to labor supply shocks occurs along the inflow margin rather than through the exit of incumbents ([Dustmann et al., 2017](#); [Ortega and Verdugo, 2022](#)), these patterns indicate a contraction in firm formation and expansion. Event study estimates show no evidence of pre-trends and a sharp, persistent decline in entry and job creation following activation.

We provide several pieces of evidence to support a labor supply channel as the primary mechanism. First, we document that SC activation reduces the local immigrant population, particularly the non-citizen and Hispanic non-citizen shares, confirming a contraction in the workforce most exposed to enforcement. Second, the effects are highly concentrated in settings where firms rely more heavily on immigrant labor. Establishment entry and job creation decline sharply in immigrant-intensive sectors and in counties with higher baseline immigrant shares, while effects are negligible in low-exposure settings. This pattern of heterogeneity is difficult to reconcile with aggregate demand shocks or general policy uncertainty, which would generate more uniform effects across sectors and locations. Instead, it is consistent with enforcement operating through the availability of labor, constraining firm formation and expansion where immigrant workers constitute a key input.

We further show that enforcement disrupts the hiring process, particularly in sectors that depend on continuous labor inflows. Sectors with higher immigrant shares also exhibit higher baseline turnover rates, indicating a greater reliance on ongoing recruitment to sustain production. In these sectors, the decline in entry and job creation is substantially larger, consistent with increased hiring frictions when the available workforce contracts. Decomposing job flows reveals that the decline in job creation is driven primarily by reduced hiring at continuing establishments rather than by changes in firm exit or the disappearance of new establishments. This pattern indicates that enforcement constrains expansion even among surviving firms, shifting adjustment toward the intensive margin rather than inducing firm closures.

Finally, we consider complementary channels through which enforcement may affect firm dynamics. We find evidence of a decline in immigrant self-employment, suggesting that enforcement directly reduces the pool of potential entrepreneurs. There is also suggestive evidence of a local demand channel, with stronger and more precisely estimated effects in non-tradable sectors that depend on local consumption. However, these channels appear secondary to the labor supply mechanism, as the strongest and most robust effects align with variation in immigrant labor exposure and hiring dependence. Taken together, the evidence indicates that immigration enforcement acts primarily as a negative local labor supply shock, with downstream effects on firm formation and growth.

The results are robust to a wide range of alternative specifications. They remain stable when accounting for labor market-level shocks by aggregating the analysis to the commuting zone level. The findings are also insensitive to sample selection and BDS suppression, alternative estimators that account for treatment effect heterogeneity, and different inference and weighting choices. Across all checks, the estimates are stable in both magnitude and significance, supporting the credibility of the identification strategy.

A growing literature shows that immigration affects firms through margins beyond wages, including establishment expansion, survival, productivity, and reallocation across firms. In

U.S. evidence, low-skill immigration increases the number of establishments, particularly among smaller and more mobile firms, consistent with adjustment along the extensive margin (Olney, 2013). At the local level, immigration inflows raise establishment counts, employment, and economic activity, with an important role for reduced firm exit (Mahajan et al., 2024; Orrenius et al., 2020). Firm-level studies from France and Switzerland similarly show that greater access to immigrant labor increases productivity, investment, exports, innovation, and growth (Mitaritonna et al., 2017; Beerli et al., 2021). More broadly, immigration contributes to productivity through task specialization and technology adoption (Peri and Sparber, 2009), and labor supply shocks are often absorbed through within-firm adjustment and firm entry and exit rather than wages alone (Dustmann and Glitz, 2015). A complementary strand emphasizes immigrants as entrepreneurs and job creators. Immigrants account for a disproportionate share of new firm formation and high-growth firms in the United States, expanding labor demand rather than merely increasing labor supply (Azoulay et al., 2022; Kerr and Kerr, 2020).

Most of this literature, however, focuses on positive immigration shocks. A smaller set of papers studies restrictions on immigrant labor. Evidence from the Mexican Bracero exclusion shows that removing migrant labor did not improve domestic labor market outcomes, consistent with firm adjustment through technology and production rather than native replacement (Clemens et al., 2018). Related evidence from E-Verify mandates shows that restricting access to undocumented labor reduces formal employment and firm activity, particularly among larger firms that are more likely to comply, pointing to both intensive and extensive margin adjustments in response to labor supply shocks (Ayromloo et al., 2020). More recent randomized evidence from the H-2B visa lottery shows that restricting access to low-skill immigrant labor reduces firm revenue, investment, and profits, with little evidence of native substitution, implying complementarity between immigrant and native workers (Clemens and Lewis, 2022).

The enforcement literature provides the closest bridge to this paper, though existing evidence largely focuses on labor demand and hiring responses rather than broader firm dynamics. Secure Communities reduced employment among likely undocumented immigrants and also lowered employment and wages for U.S.-born workers, consistent with reduced labor demand (East et al., 2023). It also generated broader equilibrium effects, including higher prices for household services and reduced labor supply among high-skilled natives (East and Velásquez, 2024). Earlier evidence on 287(g) programs similarly finds sectoral employment declines consistent with adverse labor supply shocks (Bohn and Santillano, 2017). Consistent with this, firms respond to intensified immigration enforcement by adjusting their labor demand margins, including increased reliance on alternative foreign labor channels such as guest worker programs (Amuedo-Dorantes et al., 2021).

This paper contributes to the literature on immigration, labor markets, and firm dynamics by providing the first large-scale causal evidence on the effects of a mandatory federal interior enforcement policy on business formation and growth. By linking the rollout of SC to establishment-level dynamics, the paper shows that immigration enforcement shapes not only employment and wages but also the pace of business formation and expansion, highlighting firm-level margins largely absent from the enforcement literature.

More broadly, the paper contributes to a growing literature on how firms adjust to labor supply shocks by demonstrating that such shocks can constrain economic activity without inducing firm exit, highlighting a distinct adjustment margin relative to the reallocation and exit responses emphasized in prior work. It also provides new evidence on the mechanisms underlying these effects, showing that enforcement operates through a contraction in local labor supply and disproportionately affects sectors and locations that rely on continuous hiring, while also reducing immigrant entrepreneurship. Together, these findings establish immigration enforcement as a key determinant of firm dynamics in local economies.

The remainder of the paper is organized as follows. Section 2 provides the institutional background. Section 3 describes the data. Section 4 outlines the empirical strategy. Section 5

presents the main results. Section 6 explores mechanisms. Section 7 discusses additional results. Section 8 discusses robustness. Section 9 concludes.

## 2 Institutional Background

### 2.1 Post-IIRIRA Interior Enforcement Architecture

The 1996 Illegal Immigration Reform and Immigrant Responsibility Act (IIRIRA) substantially expanded the scope of interior immigration enforcement in the United States. Prior to IIRIRA, enforcement within the interior relied primarily on federal immigration authorities with limited operational capacity. The Act broadened this framework by increasing federal oversight and strengthening cooperation between federal and local law enforcement agencies. 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.

Subsequent programs developed within this expanded enforcement architecture include Secure Communities, which links local criminal justice systems to federal immigration databases, and E-Verify, an employment verification system building on the Basic Pilot Program. State-level omnibus immigration laws further extended enforcement through coordinated legislative efforts. Overall, these policies increased the expected probability of detection and removal for undocumented individuals, altering local labor supply conditions.

### 2.2 Secure Communities: Design and Operational Mechanism

Secure Communities, introduced in 2008, became the central interior enforcement program during our study 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 (IDENT). If the individual is identified as potentially removable, ICE is notified.

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 (excluding weekends and holidays) 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.

A key feature of SC is that enforcement is triggered by contact with the criminal justice system rather than workplace inspections or employer audits, distinguishing it from employer-facing programs such as E-Verify. By increasing the probability that an arrest leads to detention and removal, SC raises the expected cost of residing and working without legal status. This generates a negative labor supply shock in local markets, with implications for firm entry, hiring, and production decisions. 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.

The expansion of Secure Communities was associated with a substantial increase in immigration enforcement activity. Following activation, the number of individuals identified as removable through fingerprint-based checks rose sharply, leading to a corresponding increase in detainers issued and removals carried out by U.S. Immigration and Customs Enforcement. This increase reflects the shift from discretionary, resource-constrained enforcement to automated, system-wide screening of individuals entering the criminal justice system. The resulting rise in the likelihood that contact with law enforcement leads to detention and removal provides a key first-stage channel through which SC affects local labor supply.

## 2.3 Federal Rollout and the Source of Identifying Variation

Secure Communities was implemented through a phased national rollout between 2008 and 2012, with the objective of achieving nationwide coverage by 2013. 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, SC was deployed through a centralized federal process and did not require counties to apply for participation.

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. In Section 4, we show that rollout timing is not systematically related to pre-existing trends in local business activity or other observable economic conditions.

A remaining concern is that earlier activation may have been targeted toward counties with larger immigrant populations or higher enforcement priorities. While such targeting cannot be fully ruled out, the centralized implementation of SC, combined with high-dimensional fixed effects, limits the scope for time-varying local confounders to drive the results. Moreover, because the program expanded rapidly to near-universal coverage, the estimates are identified primarily from short-run differences in activation timing rather than persistent cross-county differences in exposure.

In 2014, Secure Communities was discontinued and replaced by the Priority Enforcement Program (PEP), which narrowed the scope of enforcement by focusing on individuals with serious criminal convictions. Secure Communities was subsequently reinstated in 2017. To maintain a consistent policy environment and isolate variation arising from the original rollout, we restrict our main analysis to the 2008-2012 period. This window captures the phase during which substantial cross-county differences in activation status existed, while

leaving a small set of counties not yet activated that serve as a comparison group in the empirical strategy.

## **2.4 Geographic Rollout of Secure Communities**

Figure 1 illustrates the geographic expansion of Secure Communities across U.S. counties during the rollout period. The program initially expanded in counties along the southern border and in large metropolitan areas with existing federal enforcement infrastructure, before extending more broadly across the country as implementation progressed.

By the end of 2011, approximately 64 percent of U.S. jurisdictions had been activated, and by 2012 coverage was near-universal, reaching roughly 97 percent of counties. As a result, the identifying variation in our analysis is concentrated in the 2008-2012 rollout period, when substantial cross-county differences in activation status existed over a relatively short horizon.

# **3 Data**

## **3.1 Data Sources**

We use data from 2005 to 2012 across several sources. 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. We use data from 2005 to 2012, which includes several pre-treatment years prior to the initial rollout of Secure

Communities 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. The sample contains 708,149 county-sector-year observations.

We study several outcomes capturing firm dynamics. Our main outcomes are the number of firms, the number of establishments, establishment entry, and establishment exit. 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), extracted from the Integrated Public Use Microdata Series (IPUMS) ([Ruggles et al., 2023](#)), to construct measures of local population composition and self-employment. Specifically, we use ACS microdata from 2005 to 2012 to compute county-level shares of immigrants with a high school education or less, a group that closely proxies the population most exposed to immigration enforcement. We also construct measures of self-employment to examine how enforcement affects entrepreneurial activity. The ACS provides large, nationally representative samples with rich socio-demographic information, allowing us to track these outcomes over time.

In addition, we use the ACS to construct baseline measures of immigrant exposure for heterogeneity analysis. These include the county-level population share of immigrants with less than a high school education and the sectoral share of such immigrants. Since the ACS

is available at the Public Use Microdata Area (PUMA) level, we map PUMAs to counties using a probabilistic crosswalk with population weights.<sup>1</sup>

### **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. These data allow us to capture changes in labor costs across sectors and assess the extent to which enforcement-induced labor supply shocks translate into wage adjustments.

### **3.1.4 Quarterly Workforce Indicators**

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, which serve as a proxy for hiring frictions and allow us to examine whether enforcement effects are stronger in high-turnover sectors.

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<sup>1</sup>ACS IPUMS provides data at the PUMA level, which we map to counties using a probabilistic crosswalk with population weights.

### 3.1.5 Additional Datasets

We use data from the 2000 Census to construct baseline county population weights and industry shares. These baseline industry shares are used to construct a Bartik labor demand index following [Bartik \(1991\)](#), which interacts national sectoral employment growth rates with county industry composition to capture differential local demand shocks ([Autor et al., 2013a](#); [Goldsmith-Pinkham et al., 2020](#)). All regressions are weighted by baseline county population from the 2000 Census.

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 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 \(2015b\)](#), and state-level omnibus immigration laws from [Allen and McNeely \(2017\)](#) and [Luo and Kostandini \(2023\)](#). We verify these data against official records from ICE and the DHS.

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

## 3.2 Descriptive Statistics

Table 1 reports pre-treatment descriptive statistics. Counties in our sample are economically large and account for a substantial share of U.S. business activity. The average county contains 1,879 firms and 2,139 establishments, with 248 establishment entries and 210 exits

per year over 2005-2007, alongside 5,650 jobs created and 5,094 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 average cell contains 109 firms and 124 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., 2013b; Greenstone et al., 2010). The richness of this variation allows us to exploit differential exposure across sectors within the same local labor market.

The average non-citizen share is 4.5 percent, with a foreign-born low-education share of 3.3 percent and a Hispanic non-citizen share of 3.5 percent. While these averages appear modest, they mask substantial cross-county dispersion: immigrant populations in the U.S. are highly spatially concentrated, with a relatively small set of counties accounting for a large share of the foreign-born population (Card, 2001; Lewis, 2011). 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 Canonical Fixed Effects Specification

We estimate the effects of SC implementation on local business dynamics using a difference-in-differences framework with staggered treatment timing. A standard three-way fixed effects

specification would take the following form:

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

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 ( $\mu_c$ ), year fixed effects ( $\lambda_t$ ), and sector fixed effects ( $\delta_j$ ). 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.<sup>2</sup>

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,<sup>3</sup> the state minimum wage, a Bartik labor demand index that captures sector-specific local demand shocks (Bartik, 1991; Autor et al., 2013a),<sup>4</sup> and a state-level housing boom exposure measure interacted with linear and quadratic time trends.<sup>5</sup> 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

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<sup>2</sup>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.

<sup>3</sup>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.

<sup>4</sup>See Appendix Section A for details on its construction.

<sup>5</sup>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.

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. The estimation sample spans 2005-2012, which ensures sufficient pre-treatment observations for all SC activation cohorts while covering the main rollout period. Counties first treated in 2013 or later are classified as never-treated.

While equation (1) provides a useful benchmark, it can produce biased estimates in settings with staggered treatment timing and heterogeneous treatment effects (De Chaisemartin and d’Haultfoeuille, 2020; Callaway and Sant’Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak et al., 2024). We therefore rely on the interaction-weighted estimator of Sun and Abraham (2021), which is robust to such heterogeneity. We use this estimator both to recover average treatment effects on the treated and to trace dynamic effects relative to the year before SC activation. In the latter section, we show robustness using the estimators by Goodman-Bacon (2021) and Borusyak et al. (2024).

## 4.2 Average Treatment Effect and Event Study Estimates

Let  $E_c$  denote the first year of SC adoption in county  $c$ . 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:

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

where  $\mathbb{1}(E_c = g)$  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.

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. 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,

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

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.

### 4.3 Identification

Identification exploits staggered variation in the timing of SC activation across counties. The rollout followed a federally determined schedule and was not designed in response to local economic conditions. Our empirical strategy therefore 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.

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  $k < 0$ ,

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

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.

A potential concern is that the timing of SC activation may be correlated with unobserved county characteristics, such as immigration intensity, enforcement priorities, or local economic conditions. We address this in three ways. First, the federal nature of the rollout limits local discretion in adoption timing. Second, we control for other contemporaneous immigration policies and local demand conditions, including 287(g) agreements, E-Verify mandates, omnibus immigration laws, sanctuary policies, a Bartik measure of local labor demand, and housing market exposure. Third, we assess the identifying assumption empirically by examining pre-treatment trends and testing whether pre-period characteristics predict the timing of SC activation. Under these conditions, differences in outcomes across counties with different activation timing identify the causal effect of SC on local business dynamics.

In addition, we test whether pre-treatment trends in local labor markets, housing conditions, unemployment, and political characteristics predict the timing of activation, and find no systematic relationship (see Section X).

## 5 Results

### 5.1 Main Results

Table 2 reports aggregate ATTs estimated using the [Sun and Abraham \(2021\)](#) method. 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 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 never-treated counties 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.

Because many outcome variables take zero values, we estimate the regressions in levels and compute percentage effects relative to pre-treatment means. Following [Chen and Roth \(2024\)](#), log-like transformations such as  $\log(1 + y)$  or  $\operatorname{arcsinh}(y)$  yield scale-dependent estimates in this setting, as their effects depend on the units of the outcome when treatment affects the extensive margin. We therefore report effects in levels and express them relative to pre-treatment means. 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 10.8% on average per county-sector-year cell, while the coefficient for establishment exit is statistically indistinguishable from zero. Job creation falls by 11.8% per county-sector-year cell, while job destruction is statistically insignificant. The decline in establishment entry and job creation are both statistically and economically significant, whereas the absence of an effect on establishment exit and job destruction indicates that enforcement primarily operates through the entry and hiring margins rather than through firm shutdowns or separations.

These results point to a clear asymmetry in how firms adjust. Immigration enforcement does not push existing establishments out of the market or generate layoffs at scale. Instead,

the adjustment occurs through reduced firm entry and slower expansion among continuing firms, indicating that enforcement slows firm formation and growth rather than disrupting ongoing production.

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 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 ([Goodman-Bacon, 2021](#)). The specifications include the same set of controls as in the baseline.

The pre-period coefficients,  $k \in \{-4, \dots, -2\}$ , are statistically indistinguishable from zero for both headline outcomes, supporting the parallel trends assumption.<sup>6</sup> The far lead ( $k = -4$ ) shows a larger negative point estimate for both outcomes with wide confidence intervals, reflecting cohort composition. At this horizon, the earliest cohort contributes disproportionately. Table H.1 shows that the aggregate ATT is stable when omitting any single activation cohort. The Oster bounds (Section D) further indicate that the results are robust to unobserved confounding.

Following activation, establishment entry declines sharply at  $k = 0$  and remains negative throughout the post-period, with estimates at longer horizons statistically indistinguishable from zero. In contrast, the exit and destruction event studies show no systematic response, consistent with the null aggregate ATTs. This asymmetry mirrors the static results: enforcement operates through the entry and hiring margins without generating offsetting increases in exit or separations. The negative effects on entry persist over the observed horizon, although the limited post-treatment window prevents strong conclusions about longer-run dynamics.

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<sup>6</sup>Appendix C reports joint Wald tests of the pre-period coefficients for all four outcomes.

## 5.2 Heterogeneity by Immigrant Exposure

If SC operates through the labor supply channel, effects should concentrate where immigrants are both in terms of what sectors they work in and where they live. We test this by splitting 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.

The labor supply channel predicts that enforcement effects concentrate in sectors with high immigrant employment shares. We split BDS sectors at the median foreign-born low-education employment share (6.3%, computed from the ACS 1-year PUMS, 2005-2007) and estimate Sun-Abraham ATTs separately on each subsample. The list of all sectors by immigrant share is presented in Appendix Table [B.2](#).

Table [3](#) (Panels A-B) reports the results. In high immigrant-share sectors, SC activation reduces establishment entry by 17.9% and job creation by 19.9%, both statistically significant. Exit and destruction are unaffected. In low-immigrant-share sectors, all four outcomes are economically small and statistically insignificant.

Next, we apply the same median-split approach to counties, dividing at the median baseline foreign-born share. Table [3](#) (Panels C-D) reports the results. In high-immigrant-share counties, SC activation reduces establishment entry by 11.7% and job creation by 11.7 percent. In low-share counties, all outcomes are economically small and statistically insignificant.

These patterns indicate that the mechanism underlying the main effects operates through the availability of immigrant labor. The concentration of declines in establishment entry and job creation in immigrant-intensive sectors and counties suggests that enforcement does not generate a broad contraction in economic activity. If the effects were driven by aggregate demand shocks, policy uncertainty, or general equilibrium spillovers, we would expect declines across all sectors and locations. Instead, the effects are confined to settings where immigrant labor constitutes a meaningful share of the workforce. This pattern of heterogeneity

is consistent with a labor supply channel in which enforcement reduces the pool of available workers and constrains firm formation and expansion.

## 6 Mechanisms

The results in Section 5 are consistent with immigration enforcement operating as a negative local labor supply shock. Enforcement reduces the local immigrant workforce through removals, induced out-migration, and reduced inflows (Bohn et al., 2014; Amuedo-Dorantes et al., 2019; Smith, 2023). Section 6.1 provides direct evidence that SC activation lowers the non-citizen and Hispanic non-citizen population shares, confirming a contraction in the population most exposed to enforcement.

A reduction in the immigrant workforce affects local business dynamics through several channels. First, it tightens local labor markets by increasing effective labor costs and making it more difficult for firms to recruit workers. Second, it may reduce local consumption demand, particularly in non-tradable sectors that rely on local spending. Third, enforcement may reduce business formation directly by removing or deterring immigrant entrepreneurs, who start firms at higher rates than natives. These channels imply declines in establishment entry and job creation, with effects concentrated in immigrant-intensive sectors, high-immigrant locations, and sectors that depend on continuous hiring.

The relative importance of these channels generates distinct empirical predictions. A labor supply contraction should raise wages and increase hiring frictions, particularly in sectors more exposed to immigrant labor. A demand channel should disproportionately affect non-tradable sectors, where firms rely on local consumption. Finally, if enforcement reduces entrepreneurship directly, declines in business formation should be concentrated among immigrant entrepreneurs, with little corresponding decline among natives.

The remainder of this section provides evidence on these mechanisms. We first document the reduction in the local immigrant population. We then examine labor market tighten-

ing and hiring frictions, followed by the local demand channel and the role of immigrant entrepreneurship.

## 6.1 First Stage: Effects on the Local Immigrant 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, 2023). We first present evidence on the first-order effects of SC activation on the local immigrant population, the channel through which enforcement is expected to affect labor markets and firm dynamics.

We estimate Sun-Abraham event studies using the same specification as equation (2), replacing business outcomes with county-level immigrant population shares from the ACS 1-year estimates. The sample is restricted to counties with a population greater than 65,000, where 1-year estimates are available.

Figure B.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 B.3 reports Sun-Abraham aggregate ATTs. The non-citizen share declines by 0.218 percentage points, which is 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 percent level, corresponding to a 3.4% reduction. Similarly, the Hispanic non-citizen share declines by 0.211 percentage points, significant at the 5 percent 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.

These results suggest that SC activation reduced the local immigrant population, supporting the interpretation of enforcement as a negative local labor supply shock that operates through changes in the size and composition of the immigrant workforce. This contraction in labor supply provides a direct mechanism for the observed declines in establishment entry and job creation, particularly in sectors that rely more heavily on immigrant labor and in locations with greater baseline exposure.

## 6.2 Labor Market Tightening and Hiring Frictions

We first examine whether enforcement operates through a contraction in local labor supply. A reduction in the immigrant population is expected to tighten local labor markets, increasing effective labor costs and raising hiring frictions for firms, particularly in sectors that rely more heavily on immigrant labor.

Table 4 reports estimates of the effect of SC activation on wages. Average wages increase modestly following activation, with statistically significant effects in log wages. Table 5 shows that the effects are more pronounced in immigrant-intensive sectors, consistent with these sectors being more exposed to reductions in immigrant labor supply.

While the magnitude of wage changes is modest, the pattern is consistent with a tightening of local labor markets. Taken together with the decline in immigrant population and the concentration of effects in immigrant-intensive sectors, these results support the interpretation of enforcement as a negative local labor supply shock that raises effective labor costs.

In addition to increasing labor costs, the contraction in labor supply is likely to disrupt firms' ability to hire. Sectors that rely on continuous hiring are particularly sensitive to such disruptions. New establishments must hire rapidly to reach viable scale, making them especially vulnerable to reductions in available workers. More generally, sectors with high worker turnover depend on a steady flow of new hires to replace separations and sustain production. If enforcement reduces the availability of workers, these sectors should experience a disproportionate decline in entry and job creation.

Consistent with this mechanism, Figure B.2 shows that sectors with higher immigrant employment shares also have higher baseline turnover rates (Pearson  $r = 0.77$ ). This strong relationship implies that the sectors most exposed to immigration enforcement are also those most reliant on continuous hiring. We proxy this dependence using sector-level turnover rates from the QWI (2005–2007 average) and split sectors at the median. Table 6 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 indicate that enforcement operates not only by raising labor costs but also by disrupting the hiring process. Sectors that rely on continuous labor inflows are less able to replace workers and expand production when the available workforce contracts. We therefore interpret turnover not as a separate mechanism, but as a channel that amplifies the labor supply shock, explaining why immigrant-intensive sectors experience the largest declines in firm entry and job creation.

### 6.3 Adjustment Along the Intensive Margin

The results in Section 5 reveal a sharp and asymmetric response of local business activity to immigration enforcement: establishment entry and job creation decline substantially, while establishment exit and job destruction remain largely unchanged. This pattern provides a central organizing fact for understanding how firms adjust to a contraction in local labor supply. Rather than forcing firms out of the market, enforcement constrains firm formation and expansion, indicating that adjustment occurs primarily along the inflow margin. Consistent with a broader literature showing that labor supply shocks operate through the inflow margin rather than incumbent exit (Dustmann et al., 2017; Ortega and Verdugo, 2022), these patterns indicate that enforcement suppresses firm formation and hiring without inducing firm closures.

This asymmetry follows naturally from a tightening labor market with increased hiring frictions. New and expanding firms depend on timely hiring to reach efficient scale, making them particularly sensitive to disruptions in labor supply. In contrast, incumbent firms have already matched with workers and can adjust more gradually, for example, by slowing hiring or reducing expansion, allowing them to remain active even as labor constraints intensify.

To examine these predictions, we decompose job flows into those occurring at establishment births, establishment deaths, and continuing establishments. This decomposition distinguishes between adjustments driven by firm turnover and those arising from changes in the behavior of surviving firms.

Table 8 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 the time of enforcement and remains persistently lower in the immediate post-treatment period, while job creation at births remains flat. There are no systematic changes in job destruction. These dynamics show that immigration enforcement constrains firm expansion by increasing hiring frictions, shifting adjustment to the intensive margin rather than exit.

## 6.4 Local Consumption Demand Channel

We next examine whether enforcement affects firms through changes in local 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.

Table 7 reports estimates separately by sector type.<sup>7</sup> Non-tradable sectors exhibit significant declines in both establishment entry and job creation. In contrast, tradable sectors show negative but imprecise estimates, with similar magnitudes but larger standard errors.

The stronger and more precisely estimated effects in non-tradable sectors are consistent with a reduction in local consumption demand following the decline in the immigrant population. At the same time, the negative point estimates in tradable sectors suggest that labor supply effects operate across both sector types. Together, these patterns indicate that enforcement affects firms through both a contraction in local demand and a tightening of local labor markets.

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<sup>7</sup>The tradable/non-tradable classification follows the standard division in the local labor markets literature (Moretti, 2010; Autor et al., 2013a; Mian and Sufi, 2014). Results are not sensitive to alternative classifications.

## 6.5 Entrepreneurship as a Complementary Margin

If enforcement reduces establishment entry by shrinking the immigrant workforce, it may also reduce entry by removing 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. First, detention, deportation, and induced out-migration directly reduce the stock of immigrant business owners. Second, enforcement may deter those who remain from starting businesses by raising the risk associated with registering a firm, signing a lease, or hiring workers. Both channels predict a decline in immigrant entrepreneurship beyond any broader effect on local demand.

We examine this channel using county-year self-employment measures constructed from the ACS 1-year PUMS for 2005-2012. Table 9 reports Sun and Abraham (2021) aggregate ATTs for immigrant self-employment (columns 1-2) and total self-employment (columns 3-4). We study two margins: the self-employment *rate*, defined as the share of employed workers who are self-employed, and the self-employment *count*, which captures the stock of entrepreneurs.

Because the ACS identifies workers at the Public Use Microdata Area (PUMA) level rather than the county level, we allocate PUMA-level counts to counties using Census tract-to-PUMA crosswalks, following Peri and Sparber (2009) and Wilson (2020). This procedure introduces measurement error for counties that share PUMAs with neighboring counties, which should attenuate the estimated effects toward zero.<sup>8</sup>

The immigrant self-employment count declines by 1,817 (9.8% less relative to the pre-treatment mean), while the immigrant self-employment rate falls by 0.3 percentage points (a reduction of 2.6% when compared to before enforcement years), directionally consistent but too imprecise to distinguish from zero. Total self-employment declines by 1,998 (-4.2%).

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<sup>8</sup>The ACS 1-year sample is available only for counties with population above 65,000, covering roughly 800 counties that contain the vast majority of immigrants and SC enforcement activity.

The immigrant decline accounts for nearly all of the total decline (1,817 out of 1,998), implying that the remaining 181 attributable to natives is economically negligible. The total self-employment rate falls by 0.2 percentage points, driven by a compositional shift: enforcement removes immigrants, who self-employ at a higher rate (10.9%) than natives (9.0%), mechanically lowering the population-weighted average.

The concentration of self-employment losses among immigrants, with no meaningful native decline, is consistent with the direct removal channel rather than a general demand contraction that would suppress entrepreneurship across nativity groups. The level decline provides a micro-level mechanism for the BDS establishment entry results in Section 5: enforcement reduces the stock of immigrant-founded businesses, contributing to the aggregate decline in county-level firm entry.

## 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 B.4 complements these results by examining the stock of establishments and firms. SC activation reduces the total number of establishments by approximately 46 (3.8 percent of the pre-treatment mean) and the total number of firms by approximately 45 (4.4 percent). Both estimates are significant at the 1 percent level. The near-identical point estimates indicate that the decline falls almost entirely on single-establishment firms, consistent with small and young businesses being the primary margin of adjustment.

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, establish-

ment 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 show that enforcement effects are concentrated in settings where firms rely heavily on continuous hiring. We next examine which firms are most affected within these environments. Small and young establishments are likely to be particularly vulnerable, as they recruit locally, have limited ability to substitute capital for labor, and lack the scale to reallocate workers across locations.

We first examine heterogeneity by firm size using BDS county-level data. Establishments are grouped as small (1–19 employees), medium (20–499), and large (500+). Table B.5 shows that the effects are concentrated among small establishments. Entry declines by 14.6 percent and job creation by 11.4 percent, both statistically significant. Medium and large establishments exhibit weaker and less precisely estimated responses. In particular, large establishments show no entry response and only a marginally significant decline in job creation (12.6 percent). These patterns indicate that the entry margin operates primarily through small establishments, which are more dependent on local labor markets and less able 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 B.6 shows that young establishments drive the aggregate effects: entry declines by 15.8 percent and job creation by 18.4 percent, both statistically significant. In contrast, mature establishments exhibit no significant responses, and older establishments show only a marginally significant decline in job creation (13.1 percent) with no effects on entry or exit.

Taken together, these results indicate that enforcement primarily disrupts the startup margin. The decline in firm formation is concentrated among small and young establishments, while larger and more established firms adjust along the intensive margin without exiting. This pattern is consistent with a 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., 2013a). We address this concern through three complementary tests.

Table B.7 addresses this concern through 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  $-18.1$  to  $-17.8$ , and the job creation ATT from  $-394.4$  to  $-316.4$ , 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.<sup>9</sup> At the commuting zone level, establishment entry declines by 57.0 (16.6 percent,  $p < 0.01$ ) and job creation declines by 1,032 (13.9 percent,  $p < 0.01$ ), consistent in sign and proportional magnitude with the county-level results. The larger absolute magnitudes reflect the aggregation of multiple counties within each commuting zone.

## 8.2 Missing Data and Selection Bias

The BDS suppresses cells with establishment or employment counts below a disclosure threshold. If enforcement-induced contraction pushes county-sector cells below the suppression floor, differential missingness correlated with treatment could bias the estimates. We address this through three tests.

First, we restrict the sample to a balanced panel of county-sector cells observed in every year. Table B.8 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 B.9 reports SA 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.

Third, we implement trimming bounds by dropping the smallest county-sector cells and re-estimating. Table B.10 reports results after trimming the bottom 1, 5, and 10 percent 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

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<sup>9</sup>Treatment cohort at the commuting zone level is defined as the year when the cumulative population-weighted share of constituent counties under Secure Communities exceeds 50 percent. 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).

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 the event study using the imputation estimator of [Borusyak et al. \(2024\)](#). Unlike Sun–Abraham, which interacts cohort indicators with relative-time dummies, the imputation approach estimates counterfactual outcomes for treated units using only never-treated and not-yet-treated observations, then takes the difference as the unit-level treatment effect. The two estimators rest on different assumptions about how to construct the counterfactual and therefore provide independent confirmation when they agree.

Table [B.11](#) reports the BJS aggregate ATTs estimated at the county-sector-year level. Establishment entry declines by 18.8 percent, and job creation by 17.7 percent, both statistically significant. Exit and destruction are insignificant. The BJS point estimates are somewhat larger than the SA estimates (Table [2](#)), but both estimators agree on sign and significance for all four outcomes.

### 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. We do not use this specification as our baseline because the rollout of SC, while determined by DHS, often involved activating multiple counties within a state in the same period. As a result, state-by-year fixed effects could absorb a substantial portion of the identifying variation in treatment, reducing statistical power. Table [B.12](#) compares the two specifications. The state-by-year fixed effects specification delivers qualitatively similar results. 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 reported in the Online Appendix. First, to assess sensitivity to omitted variable bias, we implement Oster (2019) bounds (Appendix D). The implied  $\delta$  values are large in magnitude and negative, indicating that unobserved confounding would need to be implausibly strong and operate in the opposite direction to eliminate the estimated effects.

Second, to account for potential spatial correlation in residuals across neighboring counties, we compute Conley (1999) spatial standard errors, which allow for arbitrary correlation in errors across nearby geographic units (Appendix E). Across a range of distance cutoffs, the Conley standard errors are very similar to county-clustered standard errors, and the statistical significance of establishment entry and job creation is unchanged.

Third, to ensure that inference is not sensitive to the choice of clustering, we re-estimate standard errors using alternative clustering schemes, including state-level clustering and two-way clustering by county and year (Appendix F). Inference is stable across all specifications, with establishment entry and job creation remaining statistically significant.

Fourth, to examine whether the results are driven by the weighting scheme, we re-estimate all specifications without population weights (Appendix G). The unweighted results are qualitatively similar, with comparable magnitudes and significance levels, indicating that the findings are not driven by a small number of large counties.

Finally, to assess whether the estimates are driven by any single treatment cohort, we implement leave-one-cohort-out analyses (Appendix H). The estimates remain stable across all subsamples, with no evidence that any individual cohort drives the main findings.

Across all checks, the results remain stable in magnitude and statistical significance. Overall, these findings indicate that the estimates are not sensitive to alternative inference

procedures, weighting schemes, or sample composition. The consistency of results across specifications reinforces the credibility of the identification strategy.

## 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 10.8% and job creation by 11.8%, with no statistically significant effect on establishment exit or job destruction. The effects are concentrated in immigrant-intensive and high-turnover sectors, high-immigrant counties, and among small and young firms. Dynamic estimates show no evidence of differential pre-trends, and 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. Enforcement does not induce firm closures or large-scale separations; instead, it suppresses firm formation and constrains expansion among existing establishments. This asymmetry is difficult to reconcile with explanations based solely on aggregate demand or general equilibrium reallocation. Instead, the evidence points to a contraction in local labor supply. We document that Secure Communities reduces the non-citizen and Hispanic non-citizen population, raises wages, 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 amplify the primary labor supply channel. Overall, 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 largely absent from the existing literature. Prior work has focused on effects on workers, wages, and occupational sorting. We show that these labor market disruptions transmit to the extensive

margin of business formation and to the intensive margin of firm growth. Immigration enforcement does not reallocate activity across locations or firms in a way that preserves aggregate dynamism; instead, it reduces the number of new firms and slows the expansion of existing ones, particularly in environments most exposed to immigrant labor.

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 rather than short-run contractions.

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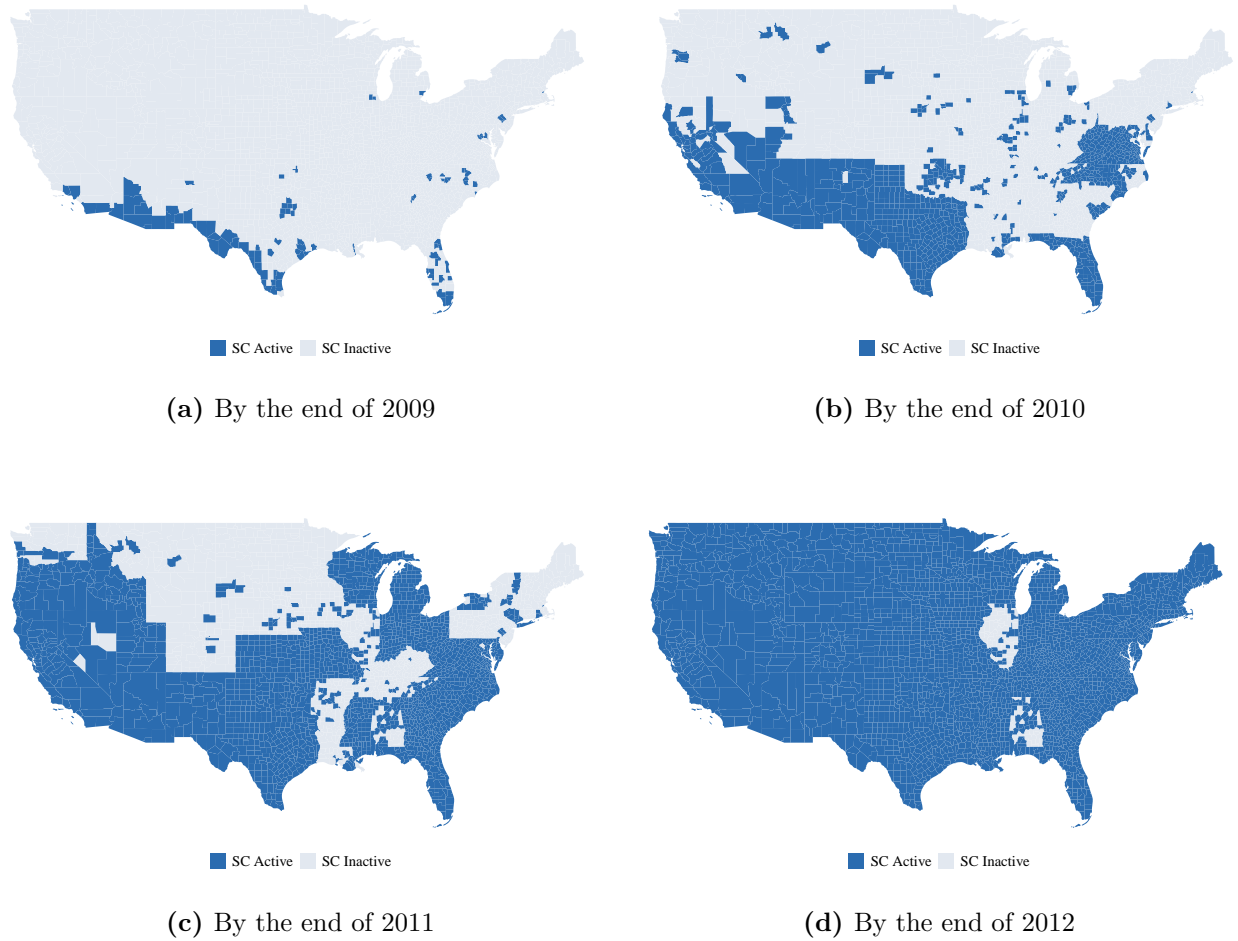
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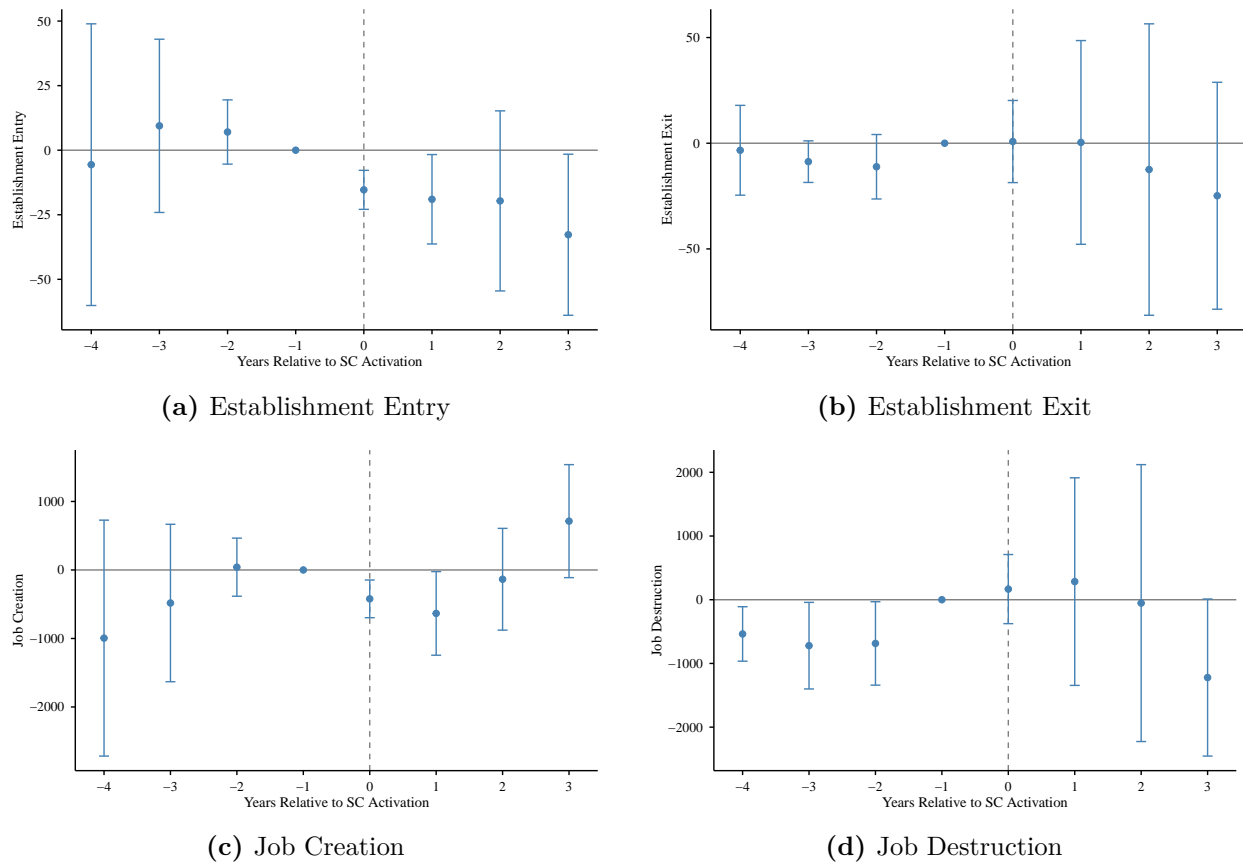
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# Figures



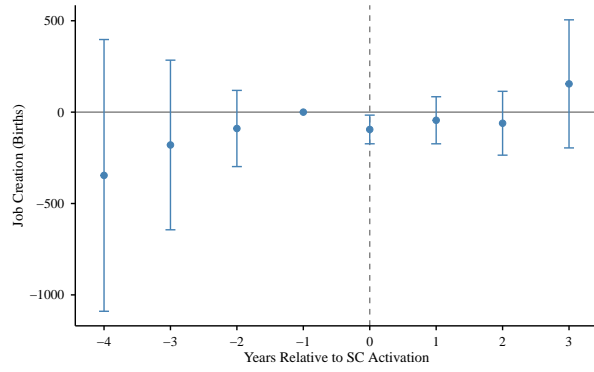
**Figure 1:** Secure Communities Rollout by County, 2009-2012

*Notes.* Each panel shades 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 percent of jurisdictions by 2011 and near-universal coverage (about 97 percent) by 2012.

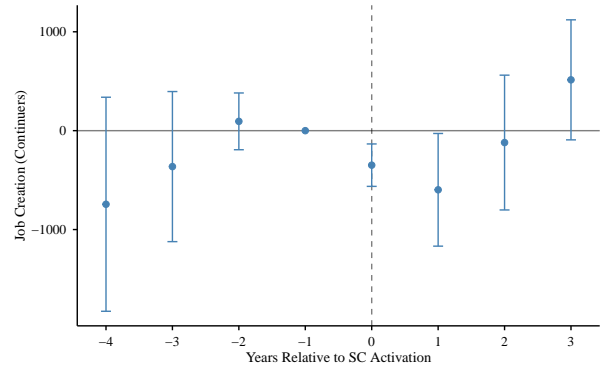


**Figure 2:** Event Study: Establishment and Employment Dynamics (Sun-Abraham)

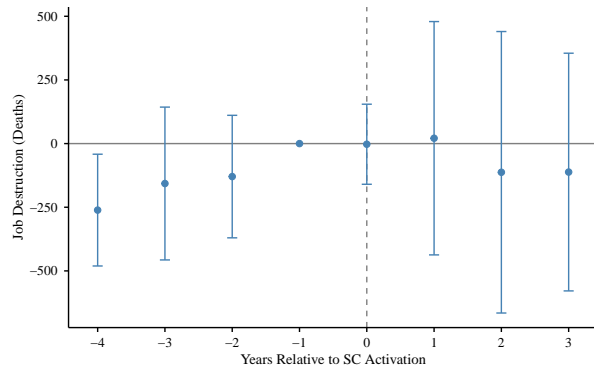
*Notes.* Sun-Abraham cohort-specific event study estimates (Sun and Abraham, 2021). Estimation sample: 2005-2012; event window  $[-4, +3]$ . Panels (a)–(b) show establishment entries and exits per county-sector-year cell; panels (c)–(d) show jobs created and destroyed. Event time  $k = 0$  marks the 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.



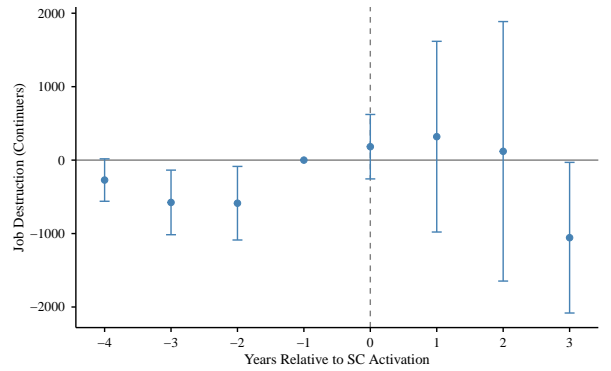
(a) Creation: Births



(b) Creation: Continuers



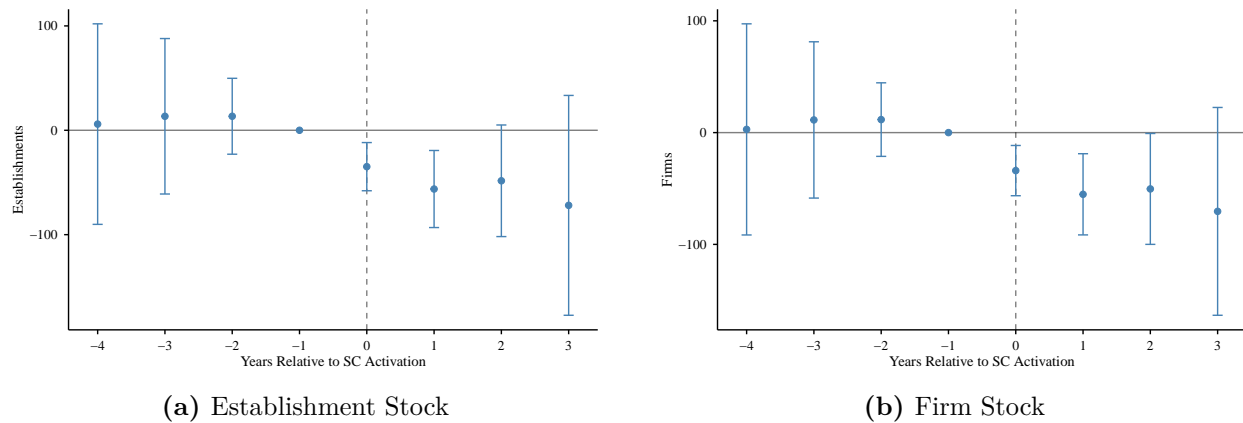
(c) Destruction: Deaths



(d) 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 window  $[-4, +3]$ ;  $k = -1$  omitted. County, year, and sector fixed effects with full controls. Weighted by baseline county population. Standard errors clustered at the county level. Bars show 95% confidence intervals.



**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. The omitted period is  $k = -1$ . Vertical bars show 95% confidence intervals based on standard errors clustered at the county level. All specifications include county, year, and sector fixed effects and the full set of controls. Regressions weighted by baseline county population.

# Tables

**Table 1:** Descriptive Statistics

	Mean	SD
<i>Panel A: Business Dynamics (County Level)</i>		
No. of Firms	1,879	5,986
No. of Establishments	2,139	7,035
Establishment Entry	248	916
Establishment Exit	210	775
Job Creation	5,650	21,371
Job Destruction	5,094	19,417
<i>N</i>	3,147	
<i>Panel B: Business Dynamics (County-Sector Level)</i>		
No. of Firms	109	440
No. of Establishments	124	512
Establishment Entry	16	69
Establishment Exit	14	58
Job Creation	328	1,630
Job Destruction	298	1,401
<i>N</i>	59,793	
<i>Panel C: Demographic Characteristics</i>		
% Foreign-Born Non-Citizen	4.46	4.34
% Foreign-Born, $\leq$ HS Education	3.32	4.99
% Hispanic Non-Citizen	3.45	3.66
<i>N</i>	3,147	

*Notes.* 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  $\leq$ HS Education from ACS 5-year estimates (2006-2010); % Hispanic Non-Citizen from ACS 1-year estimates (2005-2007).

**Table 2:** Effects of Secure Communities on Establishment and Job Dynamics

	Entry (1)	Exit (2)	Creation (3)	Destruction (4)
SC Activation	-18.1** (7.7)	-2.8 (19.6)	-394.4** (186.5)	109.7 (594.0)
Pre-treatment Mean	168.1	159.1	3,336.7	3,390.7
% Change	-10.8%	-1.8%	-11.8%	+3.2%
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 3:** Heterogeneity by Immigrant Concentration

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
<b>I. By sector immigrant share</b>				
<i>Panel A: High immigrant-share sectors (above median, 10 sectors)</i>				
SC Activation	-28.5** (11.4) [-17.9%]	-0.6 (25.1) [-0.3%]	-649.8** (271.6) [-19.9%]	-125.4 (899.7) [-2.7%]
Observations	167,648	170,002	230,055	229,067
$R^2$	0.793	0.797	0.745	0.741
<i>Panel B: Low immigrant-share sectors (below median, 9 sectors)</i>				
SC Activation	-5.9 (7.3) [-4.5%]	-5.8 (13.8) [-4.2%]	-98.4 (143.8) [-4.1%]	383.0 (292.3) [+14.8%]
Observations	131,219	131,149	180,652	179,306
$R^2$	0.560	0.558	0.601	0.595
<b>II. By county immigrant share</b>				
<i>Panel C: High immigrant-share counties (above median, 1,549 counties)</i>				
SC Activation	-19.4** (7.8) [-11.7%]	-2.5 (21.9) [-1.3%]	-390.1** (175.1) [-11.7%]	216.4 (689.8) [+5.1%]
Observations	164,889	165,686	212,141	211,202
$R^2$	0.680	0.684	0.674	0.666
<i>Panel D: Low immigrant-share counties (below median, 1,549 counties)</i>				
SC Activation	0.3 (0.3) [+3.4%]	-0.3 (0.3) [-4.0%]	-1.9 (6.7) [-1.4%]	-5.7 (7.2) [-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 percent, ACS 1-year PUMS, 2005-2007). Panels C-D split counties at the median baseline foreign-born share. Percentage changes relative to pre-treatment 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 4:** Effect of SC Activation on Average Weekly Wages: All Sectors (QCEW, Sun–Abraham)

	Wage (1)	Log Wage (2)
SA ATT	9.462 (6.667)	0.0081** (0.0037)
Pre-treatment mean % change	892 [+1.1%]	6.655
Observations $R^2$	330,300 0.705	330,300 0.838
County FE	Yes	Yes
Year FE	Yes	Yes
Sector FE	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 5:** Effect of SC Activation on Average Weekly Wages: Heterogeneity (QCEW, Sun–Abraham)

	Non-Tradable		Tradable	
	Wage (1)	Log Wage (2)	Wage (3)	Log Wage (4)
SA ATT	9.217** (4.453)	0.0121*** (0.0037)	17.830 (12.211)	0.0088 (0.0069)
Pre-treatment mean % change	749 [+1.2%]	6.490	1,150 [+1.5%]	6.955
Observations $R^2$	219,456 0.764		110,834 0.619	
<i>Panel B: Immigrant-Intensive vs. Non-Immigrant-Intensive</i>				
	Imm-Intensive		Non-Imm-Intensive	
	Wage (5)	Log Wage (6)	Wage (7)	Log Wage (8)
SA ATT	8.844* (4.516)	0.0093** (0.0043)	9.631 (13.699)	0.0065 (0.0095)
Pre-treatment mean % change	708 [+1.2%]	6.465	1,106 [+0.9%]	6.877
Observations $R^2$	197,228 0.800		133,056 0.676	
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	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 6:** Heterogeneity by Sector Baseline Turnover Rate

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
<i>Panel A: High baseline turnover sectors (above median, 10 sectors)</i>				
SC Activation	-26.6** (11.8) [-14.7%]	3.6 (28.5) [+1.7%]	-589** (238) [-19.0%]	-108 (728) [-2.7%]
Observations	163,757	165,502	220,277	218,903
$R^2$	0.720	0.723	0.657	0.657
<i>Panel B: Low baseline turnover sectors (below median, 9 sectors)</i>				
SC Activation	-9.5* (5.4) [-8.8%]	-5.7 (13.1) [-4.7%]	-191 (160) [-7.3%]	345 (463) [+10.9%]
Observations	135,110	135,649	190,430	189,470
$R^2$	0.650	0.654	0.704	0.683
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	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 percent, 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 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 7:** Heterogeneity by Tradable vs. Non-Tradable Sectors

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
<i>Panel A: Tradable sectors</i>				
SC Activation	-19.2 (16.0) [-15.4%]	15.0 (21.5) [+10.2%]	-367.5* (218.0) [-15.0%]	185.2 (494.5) [+5.6%]
Observations	100,639	101,078	143,110	141,773
$R^2$	0.389	0.422	0.535	0.532
<i>Panel B: Non-tradable sectors</i>				
SC Activation	-21.7*** (8.3) [-13.7%]	-7.5 (19.0) [-4.1%]	-384.5** (183.0) [-12.4%]	88.2 (641.8) [+2.3%]
Observations	198,228	200,073	267,597	266,600
$R^2$	0.688	0.676	0.621	0.618
County FE	Yes	Yes	Yes	Yes
Year FE	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 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 8:** Job Flow Decomposition: Births vs. Continuers

	Job Creation		Job Destruction	
	Births (1)	Continuers (2)	Deaths (3)	Continuers (4)
SC Activation	-61.3 (45.3)	-354.5** (171.0)	-19.3 (169.4)	164.6 (476.0)
Pre-treatment Mean	1,158	1,860	1,266	2,534
% Change	-5.3%	-19.1%	-1.5%	+6.5%
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	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 9:** Effect of SC Activation on Self-Employment

	Immigrant		Total	
	Rate (1)	Count (2)	Rate (3)	Count (4)
SC Activation	-0.003 (0.003)	-1,817*** (650)	-0.002** (0.001)	-1,998** (895)
Pre-treatment Mean	0.109	18,509	0.094	47,354
% Change	-2.6%	-9.8%	-2.2%	-4.2%
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
Observations	24,691	24,691	24,691	24,691
$R^2$	0.005	0.154	0.008	0.138

*Notes:* Sun–Abraham (Sun and Abraham, 2021) aggregate ATT. County-year panel, 2005–2012. Rate is the share of employed workers who are self-employed; Count is the number of self-employed workers. Columns (1)–(2): immigrant (foreign-born) workers. Columns (3)–(4): all workers (immigrant + native). Self-employment defined as own-account work in an incorporated or unincorporated business (ACS 1-year PUMS COW codes 6–7). County-level estimates constructed via PUMA-to-county crosswalks (Peri and Sparber, 2009). 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. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## ONLINE APPENDICES

# A 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., 2013a](#); [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

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

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

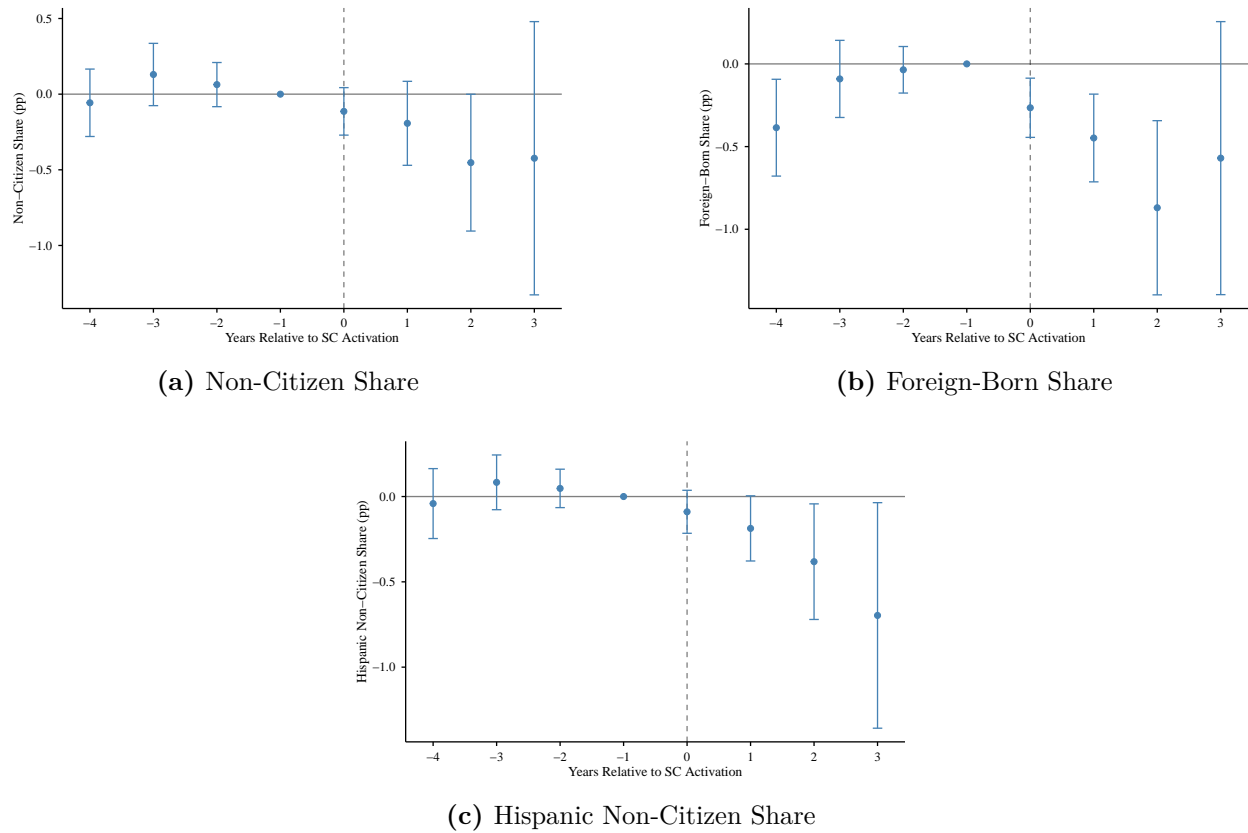
$$g_{-c,s,t} = \frac{N_{-c,s,t}}{N_{-c,s,0}}. \quad (6)$$

The Bartik index for county  $c$  in year  $t$  is then

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

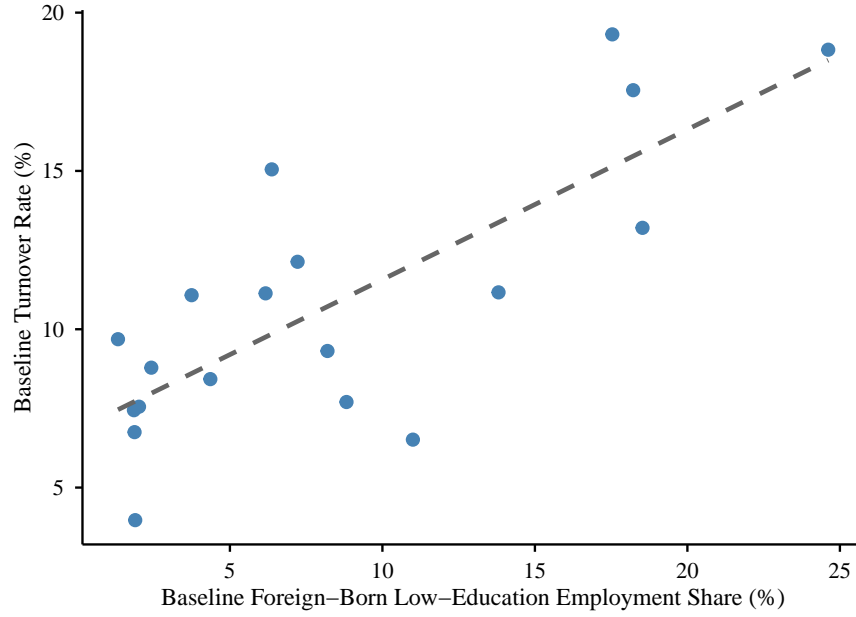
Values near one indicate that predicted labor demand in county  $c$  is close to its 2001 level; values above one indicate positive predicted demand growth. Employment data for both the baseline shares and the national growth rates come from the BDS. We use this index as a time-varying control in all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

## B Additional Figures and Tables



**Figure B.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 B.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  $r = 0.77$ .

**Table B.1:** Sectors by Baseline Turnover Rate

NAICS	Sector	Turnover Rate
<i>Panel A: Above-Median Turnover</i>		
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
<i>Panel B: Below-Median Turnover</i>		
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 B.2:** Sectors by Baseline Immigrant Share

NAICS	Sector	Immigrant Share
<i>Panel A: Above-Median Immigrant Share</i>		
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
<i>Panel B: Below-Median Immigrant Share</i>		
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 B.3:** 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 (0.133)	-0.453*** (0.144)	-0.211** (0.099)
Pre-treatment mean	7.591	13.387	5.084
% change	-2.9%	-3.4%	-4.2%
Observations	6,118	6,223	3,836

*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 (ACS B05001). Foreign-born share: foreign-born population / total population (ACS B05002). Hispanic non-citizen share: Hispanic non-citizen population / total population (ACS B05003I). All specifications include county and year fixed effects with the full control set (287(g) county/state, E-Verify, Omnibus, Bartik, sanctuary, minimum wage, housing boom exposure  $\times$  trends). Weighted by baseline county population. Standard errors clustered at the county level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

**Table B.4:** Effect of SC on Establishment and Firm Stock

	Establishments (1)	Firms (2)
SC Activated	-45.7*** (17.1)	-45.2*** (16.4)
Pre-treatment mean	1217.0	1023.0
% change	-3.8%	-4.4%
Observations	417,803	417,803
Counties	3,156	3,156
FE	County + Year + Sector	
Weights	Population (2000)	
Cluster	County	

*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 B.5:** Heterogeneity by Establishment Size

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
<i>Panel A: Small firms (1–19 employees)</i>				
SC Activation	−280.1** (112.3) [−14.6%]	−65.7 (268.5) [−2.9%]	−1,640.3*** (454.0) [−11.4%]	−71.1 (2,711.8) [−0.4%]
Observations	24,297	24,378	24,754	24,753
$R^2$	0.259	0.264	0.256	0.252
<i>Panel B: Medium firms (20–99 employees)</i>				
SC Activation	6.2 (9.7) [+4.1%]	−29.8** (12.6) [−22.6%]	−3,241.9 (2,018.6) [−21.1%]	−238.0 (4,784.3) [−1.2%]
Observations	17,472	17,013	24,534	24,532
$R^2$	0.275	0.166	0.279	0.237
<i>Panel C: Large firms (100+ employees)</i>				
SC Activation	17.4 (18.4) [+5.5%]	8.8 (45.3) [+2.5%]	−2,878.0* (1,515.6) [−12.6%]	4,481.6 (6,292.5) [+15.6%]
Observations (Large)	18,091	17,920	24,080	24,058
$R^2$ (Large)	0.250	0.187	0.250	0.210
County FE	Yes	Yes	Yes	Yes
Year FE	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: 20–499; Large: 500+. County-year panel (no sector dimension). Percentage changes relative to pre-treatment means in 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 B.6:** Heterogeneity by Establishment Age

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
<i>Panel A: Young firms (0–5 years)</i>				
SC Activation	−274.0*** (69.9) [−15.8%]	−109.6 (214.1) [−8.4%]	−3,046.3*** (841.7) [−18.4%]	−942.3 (2,902.1) [−7.5%]
Observations	24,454	23,262	24,689	24,604
$R^2$	0.283	0.276	0.276	0.235
<i>Panel B: Mature firms (6–10 years)</i>				
SC Activation	−0.9 (22.3) [−0.7%]	7.7 (37.0) [+1.8%]	−576.2 (493.2) [−12.7%]	182.6 (1,471.9) [+2.4%]
Observations	17,234	20,713	24,519	24,511
$R^2$	0.234	0.231	0.259	0.216
<i>Panel C: Old firms (11+ years)</i>				
SC Activation	14.6 (21.1) [+2.8%]	15.1 (60.7) [+1.5%]	−4,131.8* (2,486.7) [−13.1%]	4,911.0 (9,215.5) [+10.5%]
Observations	22,332	23,668	24,740	24,739
$R^2$	0.208	0.200	0.253	0.237
County FE	Yes	Yes	Yes	Yes
Year FE	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 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 B.7:** Spatial Robustness: Commuting Zone Tests

	Establishments		Jobs	
	Entry (1)	Exit (2)	Creation (3)	Destruction (4)
<i>Panel A: County + Year + Sector FE (baseline)</i>				
ATT	-18.1** (7.7)	-2.8 (19.6)	-394.4** (186.5)	109.7 (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
<i>Panel B: County + CZ <math>\times</math> Year + Sector FE</i>				
ATT	-17.8*** (5.1)	10.2 (10.0)	-316.4*** (117.7)	679.9** (301.3)
% 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
<i>Panel C: CZ + Year + Sector FE (commuting zone level)</i>				
ATT	-57.0*** (20.6)	18.6 (31.3)	-1,032.3*** (333.6)	2,339.7* (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  $\times$  year fixed effects, absorbing all time-varying confounds at the labor market level. Panel C aggregates the panel to the commuting zone  $\times$  sector  $\times$  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 B.8:** Balanced Panel Estimates

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
SC Activation	-18.1** (7.7)	-2.8 (19.6)	-394.4** (186.5)	109.7 (594.0)
Pre-treatment mean	146.7	168.4	2864.4	3641.8
% 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

*Notes:* Sun–Abraham (Sun and Abraham, 2021) aggregate ATTs estimated on the same specification as Table 2, 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 B.9:** Missing Data Selection Test

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
SC Activation	-0.0099 (0.0090)	0.0213** (0.0096)	0.0084 (0.0056)	0.0077 (0.0059)
Baseline missing rate	36.5%	36.1%	12.9%	13.4%
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 B.10:** Robustness: Trimming Bounds

	Establishment Entry	Establishment Exit	Job Creation	Job Destruction
<i>Panel A: Trim bottom 1%</i>				
ATT	-18.2** (8.0) [-12.2%]	-2.7 (19.7) [-1.6%]	-390.4** (185.2) [-13.5%]	136.4 (597.9) [+3.7%]
Observations	265,466	269,199	375,822	375,819
$R^2$	0.676	0.680	0.672	0.665
<i>Panel B: Trim bottom 5%</i>				
ATT	-17.8** (8.0) [-11.9%]	-2.5 (19.8) [-1.5%]	-431.0** (197.2) [-14.7%]	117.6 (593.5) [+3.2%]
Observations	256,804	259,266	361,272	361,272
$R^2$	0.677	0.681	0.673	0.666
<i>Panel C: Trim bottom 10%</i>				
ATT	-15.9** (7.4) [-10.5%]	-4.2 (19.3) [-2.4%]	-439.9** (196.1) [-14.8%]	132.7 (594.3) [+3.5%]
Observations	244,985	247,102	341,473	341,473
$R^2$	0.679	0.683	0.675	0.668
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	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 percent 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 B.11:** Borusyak–Jaravel–Spiess Imputation Estimates

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
BJS ATT	-27.5*** (6.5)	-3.4 (6.0)	-506.9** (234.5)	47.2 (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

*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 B.12:** Robustness: Year FE with Controls vs. State  $\times$  Year FE

	Establishments		Jobs	
	Entry (1)	Exit (2)	Creation (3)	Destruction (4)
<i>Panel A: Sun-Abraham — Year FE (primary)</i>				
ATT	-18.1** (7.7)	-2.8 (19.6)	-394.4** (186.5)	109.7 (594.0)
<i>Panel B: Sun-Abraham — State <math>\times</math> Year FE (robustness)</i>				
ATT	-17.9*** (5.2)	6.5 (12.8)	-360.7** (144.2)	510.4 (437.3)
<i>Panel C: TWFE — Year FE (primary)</i>				
ATT	-17.0** (7.1)	0.1 (4.0)	-555.5** (276.2)	15.5 (195.6)
<i>Panel D: TWFE — State <math>\times</math> Year FE (robustness)</i>				
ATT	-10.8*** (3.1)	2.6 (2.6)	-335.8*** (101.1)	268.4** (135.4)
Observations			479,712	
Counties			3,156	

*Notes:* Panels A and B report Sun–Abraham aggregate ATTs under year FE (with the full control set) and state  $\times$  year FE, respectively. Panels C and D report the corresponding static TWFE estimates. County and sector fixed effects throughout. Weighted by baseline county population. Standard errors clustered at the county level. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## C Joint Pre-Trend Tests

Table C.1 reports individual pre-period Sun–Abraham coefficients (Panel A) and joint Wald tests (Panel B) for each of the four gross outcomes. The coefficients are interaction-weighted averages across cohorts at each event time.

Panel A shows that the individual pre-period coefficients for establishment entry are small and statistically insignificant at all leads ( $k = -4, -3, -2$ ). Job creation coefficients are large in magnitude at  $k = -4$  but individually insignificant. The wide standard errors reflect thin cohort coverage at the far lead. Job destruction shows individually significant pre-period coefficients at all three leads, though this is the outcome for which the aggregate ATT is null.

Panel B reports the joint Wald test of  $H_0: \beta_{-4} = \beta_{-3} = \beta_{-2} = 0$ . Establishment entry fails to reject ( $\chi^2(3) = 1.41, p = 0.70$ ); exit, creation, and destruction reject. For job creation, the individual pre-period coefficients are all within one standard error of zero. Therefore, the joint rejection is driven by the large and imprecise far lead at  $k = -4$ , where employment flows are measured in levels with substantially higher variance than establishment counts. The rejections on exit and destruction are consistent with the individually significant pre-period coefficients in Panel A and the null aggregate ATTs on those margins.

**Table C.1:** Joint Wald Tests of Pre-Period Coefficients

	Estab. Entry (1)	Estab. Exit (2)	Job Creation (3)	Job Destruction (4)
<i>Panel A: Individual pre-period coefficients</i>				
$k = -4$	-5.6 (27.8)	-3.3 (10.8)	-994.7 (878.5)	-537.2** (218.1)
$k = -3$	9.4 (17.1)	-8.7* (5.0)	-482.3 (586.3)	-721.0** (346.7)
$k = -2$	7.0 (6.3)	-11.1 (7.8)	40.9 (216.3)	-686.0** (334.0)
<i>Panel B: Joint Wald tests</i>				
$H_0: \beta_{-4} = \beta_{-3} = \beta_{-2} = 0$	1.41 [0.704]	12.28 [0.006]	13.86 [0.003]	39.58 [0.000]

*Notes:* Panel A reports interaction-weighted (aggregated by period) Sun–Abraham (Sun and Abraham, 2021) pre-period coefficients with standard errors in parentheses. Panel B reports joint Wald tests;  $p$ -values in brackets. The test statistic is  $W = \hat{\beta}'_{\text{pre}} \hat{\mathbf{V}}_{\text{pre}}^{-1} \hat{\beta}_{\text{pre}} \sim \chi^2(q)$ . 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. \* $p < 0.10$ , \*\* $p < 0.05$ , \*\*\* $p < 0.01$ .

## 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  $\delta$  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  $R_{\max}^2$ .

Table [D.1](#) reports  $\delta$  under two assumptions for  $R_{\max}^2$ :  $1.3 \times R_{\text{controlled}}^2$  (conservative) and  $2.0 \times R_{\text{controlled}}^2$  (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,  $\delta = -1,010.5$  at  $R_{\max}^2 = 1.3 \times R_{\text{controlled}}^2$ , rising in magnitude to  $\delta = -3,368.4$  under the more generous assumption. Adding controls reduces the coefficient magnitude (from  $-22.2$  to  $-19.4$ ) while  $R^2$  barely moves ( $0.671$  to  $0.673$ ). The negative sign of  $\delta$  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 a thousand times as influential as observable selection to nullify the result.

For job creation,  $\delta = -40.7$  at the conservative  $R_{\max}^2$ , rising in magnitude to  $-135.5$  under the generous assumption. Adding controls reduces the coefficient magnitude (from  $-751.5$  to  $-471.0$ ) while  $R^2$  increases slightly from  $0.662$  to  $0.671$ . Again, the negative  $\delta$  implies that unobservables would need to work in the opposite direction from observables, and with a magnitude over forty 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	
$\hat{\beta}^{\text{naive}}$	-22.2		-751.5	
$R^2_{\text{naive}}$	0.671		0.662	
$\hat{\beta}^{\text{controlled}}$	-19.4		-471.0	
$R^2_{\text{controlled}}$	0.673		0.671	
$\delta$ (1.3 $\times$ )	-1,011		-40.7	
$\delta$ (2.0 $\times$ )	-3,368		-135.5	
County FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	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.  $\delta$  is the proportional selection ratio at  $R^2_{\text{max}} = 1.3 \times R^2_{\text{controlled}}$  and  $2.0 \times R^2_{\text{controlled}}$ .  $|\delta| > 1$  implies robustness. Negative  $\delta$  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 percent level under all cutoffs. Job creation remains significant at the 5–10 percent level. The headline results are not sensitive to the treatment of spatial autocorrelation.

**Table E.1:** Robustness to Conley Spatial Standard Errors

Variance estimator	Establishments				Jobs			
	Entry		Exit		Creation		Destruction	
	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** (7.129)	0.108 (3.972)	-555.509** (276.163)	15.476 (195.577)
State clustering	-16.973** (7.274)	0.108 (3.405)	-555.509** (264.223)	15.476 (192.500)
Two-way (county & year)	-16.973* (8.242)	0.108 (3.015)	-555.509 (313.887)	15.476 (267.367)
Observations	298,867	301,151	410,707	408,373
R <sup>2</sup>	0.67322	0.67709	0.66980	0.66174
County FE			Yes	
Year FE			Yes	
Sector FE			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
<i>Panel A: Weighted by baseline population</i>				
Secure Communities	-16.973** (7.129)	0.108 (3.972)	-555.509** (276.163)	15.476 (195.577)
Observations	298,867	301,151	410,707	408,373
$R^2$	0.673	0.677	0.670	0.662
<i>Panel B: Unweighted</i>				
Secure Communities	-2.399*** (0.394)	0.051 (0.302)	-62.909*** (11.151)	-24.029** (9.560)
Observations	298,867	301,151	410,707	408,373
$R^2$	0.617	0.624	0.606	0.602
County, Year, Sector FE			Yes	
Enforcement controls			Yes	
Economic controls			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 percent 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

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

$$y = a n^\alpha, \quad 0 < \alpha < 1, \quad (8)$$

where  $a$  is firm-specific productivity and  $n$  is labor input. Potential entrepreneurs are of two types,  $j \in \{I, R\}$ , where  $I$  denotes immigrant and  $R$  denotes 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  $\Gamma_s$ .

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:

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

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

Firms that wish to hire  $n$  workers incur a recruiting cost  $R(n, L_s)$ , where  $R$  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):

$$R(n, L_s) = \frac{\kappa}{2} \cdot \frac{n^2}{L_s}, \quad \kappa > 0. \quad (10)$$

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.<sup>10</sup>

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

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

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<sup>10</sup>This formulation can be microfounded through a standard matching function framework 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.

Because  $\partial L_s / \partial \mathcal{E}_c < 0$ , the recruiting cost rises. The increase is larger when baseline immigrant exposure  $(M_c, \Gamma_s)$  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

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

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:

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

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  $f_s \geq 0$ :

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

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 8), 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

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

This cost is increasing in  $\tau_s$ : 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  $\tau_s$ , consistent with the empirical results in Table 6.

## Exposure Heterogeneity

The labor supply shock and its propagation through recruiting frictions both depend on baseline immigrant exposure. The labor pool contraction  $\phi_s$  is increasing in the county immigrant share  $M_c$  and the sector immigrant dependence  $\Gamma_s$ . 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  $\Gamma_s$  is higher (greater exposure to the shock);
- The sector turnover rate  $\tau_s$  is higher (greater dependence on continuous hiring);
- These exposure measures interact: high- $M_c$  counties with high- $\Gamma_s$  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:  $\rho_R = 0$ . This reduces the expected present value of an immigrant-owned firm:

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

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 concentration of self-employment losses among immigrants, with no meaningful native decline (Table 9), is consistent with this direct removal channel operating alongside the labor market mechanism.

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 7).

Two points of clarification are important. First, the core predictions of the model—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 sectors. The empirical evidence is consistent with this: wages rise in both tradable and non-tradable sectors (Section 6).

## Predictions

The model yields the following predictions, which we test in Sections 5 and 6:

1. **The exposed immigrant population declines.** Enforcement reduces the local immigrant workforce, contracting the effective labor pool available to firms (first stage, confirmed in Section 6).
2. **Establishment entry declines.** The combination of higher wages and higher recruiting costs raises the productivity threshold for profitable entry, reducing the number of viable startups.

3. **Job creation declines.** Continuing establishments reduce expansion because recruiting additional workers becomes more costly. The decline in job creation is concentrated at continuing establishments rather than at births.
4. **Exit and job destruction change little.** Incumbents have already paid sunk costs and recruited their workforce. Moderate profit declines reduce hiring but do not push most firms below the shutdown threshold.
5. **Effects are stronger in immigrant-exposed sectors and counties.** The labor supply shock and recruiting cost increase are both proportional to baseline immigrant exposure ( $M_c, \Gamma_s$ ).
6. **Effects are stronger in high-turnover settings.** Sectors with high baseline turnover depend on continuous recruiting, amplifying the labor supply shock.
7. **Immigrant self-employment declines.** Enforcement directly removes and deters immigrant entrepreneurs, reducing business formation on a complementary margin beyond the labor market channel.
8. **Non-tradable sectors may be somewhat more affected.** If enforcement also reduces local spending, non-tradable firms face both higher labor costs and lower revenue, compounding the effect. This prediction is suggestive rather than a necessary implication of the model.