Employing Undocumented Workers: Immigration Enforcement Impacts on Small Businesses And The Role of Bank Monitoring

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Abstract

Using the staggered rollout of the Secure Communities program as shocks to undocumented labor supply, we show that the non-tradable and construction sectors in affected counties experienced higher wages, formal employment, and firm closures—indicating that firms previously relied on undocumented workers and subsequently substituted them with costlier formal labor. Small business-level data reveal that rising formal employment and closures were stronger among unbanked firms. Even among firms with banking relationships, those facing ex-ante weaker local bank monitoring exhibited stronger effects. Overall, these patterns suggest a deputization-like effect of bank screening and monitoring in deterring borrowers from employing undocumented workers.

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The population of undocumented immigrants in the United States reached an estimated 12 million in 2008, four times its level in 1990. An estimated 8.2 million of them were employed, constituting roughly 5.4% of the national labor force (**pew2025**, **passel2009**). While such workers are known to largely take up employment in low-skilled sectors and are often perceived to substitute for formal workers by accepting lower wages, systematic evidence on the effect of their employment remains limited. This paper examines their employment by small businesses, a setting in which such hiring is both more feasible and less detectable. Furthermore, because banks are the primary formal institutions on which small businesses rely for credit, and because they have incentives to screen and monitor risky borrowers through their local presence and ability to collect soft information, we study whether banking relationships influence small businesses' employment of undocumented labor.

The main challenge in studying the employment of undocumented workers is its inherent unobservability. Our solution is to infer such employment by combining quasi-exogenous negative shocks to the supply of undocumented workers with observed changes in the employment of formal workers. Specifically, we exploit the federal Secure Communities (SC) program as a negative shock to undocumented labor supply. The program required state and local law enforcement to share identity information of all detained, arrested, or imprisoned individuals with Immigration and Customs Enforcement (ICE), substantially increasing the risk of arrest and deportation for undocumented immigrants, even from minor encounters with local authorities. Implemented between 2008 and 2013 in a staggered manner across counties, the program resulted in 45.5 million fingerprint submissions and the removal of nearly 400,000 individuals (**ICE2014**).

Our focus on small businesses is motivated by several considerations. First, their contribution to the U.S. economy is substantial, accounting for nearly 43% of national

gross domestic product and 46% of private-sector employment in 2021 (**SBA2021**). Second, employing undocumented workers is more feasible for small businesses than for larger or publicly listed firms, as their limited scale and minimal formal reporting and auditing reduce the likelihood of detection. This risk is further reduced by the absence of market-based disciplinary mechanisms, such as stock market feedback, active investor monitoring, and reputational concerns, that typically apply to larger firms. Finally, statutory penalties do not apply to isolated or inadvertent violations—a defense more plausible for small businesses, over 75% of which employ only about six workers, than for large corporations.[1]

The key to our empirical strategy is the staggered implementation of the SC program, which occurred despite it being a federal policy, because the Department of Homeland Security (DHS) deemed a simultaneous nationwide rollout infeasible. We exploit this staggered rollout in a difference-in-differences (DiD) design and estimate causal effects using the **callaway2021** estimator, which offers interpretable causal parameters and robustness to treatment effect heterogeneity and dynamic effects. Our choice to use the CSDID approach is dictated by its flexibility and is discussed in detail in Section I.B.

We assess the empirical design for two key DiD requirements. First, treated and control counties do not exhibit discernible pre-trends in small business characteristics, including the number of establishments, wages, or employment. Second, using Cox-proportional hazard regressions, we find that SC activation in a county was not predictable based on small business–related characteristics (growth rates of establishments, average wages, average employment) or general economic characteristics (per capita income, international migration). County population and Hispanic share were

[1] Under Title 8 U.S.C. §1324a(f), any person or entity engaging in a "pattern or practice" of employing unauthorized aliens—excluding isolated or inadvertent acts—may be fined up to $3,000 per worker, imprisoned for up to six months, or both (**DOJ**).

associated with earlier SC activation, in line with **cox2013**, **miles2014**. This however does not pose a concern for our conclusions, as the Hispanic share is a relevant outcome (or mechanism) in our setting rather than a pure covariate, making it irrelevant for the parallel-trends assumption (**baker2025**). Moreover, our estimates are robust to controlling for these population variables.

We begin our empirical analysis by examining the effects of the SC program on county-level industry outcomes using the Census Bureau's Quarterly Census of Employment and Wages (QCEW) data. We then replicate these findings using firm-level data from the industry-produced National Establishment Time Series (NETS) dataset and further leverage it to obtain additional insights. This two-step strategy offers two key benefits. First, it strengthens the generalizability and credibility of our interpretation of undocumented labor employment. Second, it reinforces the reliability of our subsequent analyses that can only be conducted with the firm-level NETS data.

At the county level, we find that the SC program led to a statistically significant decline in the number of establishments, accompanied by increases in average wages and employment among firms in the non-tradable and construction sectors in treated counties relative to control counties.[2] In contrast, firms in tradable sectors did not experience similar changes. This industry-level difference is consistent with high prevalence of undocumented workers in non-tradable sectors, including construction, retail, and hospitality (**passel2009**). Moreover, the effects are stronger in counties that contain metropolitan areas, where almost 94% of undocumented immigrants are estimated to reside (**passel2009**).

Two key insights emerge from these patterns. First, because the SC program targeted undocumented workers, establishments that previously relied on them likely

[2] Our industry classification comes from **mian2014**. Non-tradable sectors include retail stores (grocery, food, furniture, clothing, used merchandise etc.), food services, restaurants etc. For brevity, we refer to non-tradable and construction sectors together as non-tradable.

responded to the reduced supply of undocumented labor and heightened risk of detection by substituting them with costlier formal workers. Given that official statistics such as QCEW are unlikely to capture undocumented workers (**bohn2017**), this substitution would increase the measured employment. Moreover, some establishments likely became unviable under higher detection risk and reduced reliance on low-cost undocumented labor, leading to increased firm closures. Similar wage and closure patterns have been documented in response to adverse local labor supply shocks arising from environmental contamination (**huang2025**).

Second, the fact that the effects were significant in non-tradable industries and metropolitan counties—where undocumented workers are commonly employed and reside (**pew2025**)—but not in tradable sectors and non-metro counties, further supports the proposed mechanism that these outcomes arise from negative shocks to undocumented labor.

The same patterns emerge when we apply the CSDID estimator with firm and year fixed effects to firm-year panel data on small businesses constructed from the NETS dataset. Relative to firms in control counties, those in treated counties increased employment by 0.6% and faced a 0.7 percentage points higher probability of closure. The effects are stronger in non-tradable industries and metro counties. We exclude firm-level covariates in these specifications to mitigate concerns about "bad controls," which arise from including covariates that may themselves respond to the treatment. At the same time, we include county-level covariates to improve precision, leveraging the CSDID approach of using only pre-treatment levels of the included covariates to avoid bad control concerns.

The remainder of the empirical analysis examines the role of banks in shaping small businesses' employment of undocumented workers. Our focus on banks is motivated by three considerations. First, banks are the dominant source of small business

financing (**FDIC2024**), and their lending decisions rely heavily on screening borrowers through the collection of firm-specific subjective intelligence, or "soft information" (**agarwal2010**), which is facilitated by personal interactions (**petersen2002**) and ex-post monitoring (**granja2022**, **heitz2022**). Screening is particularly intensive for secured lending, which requires detailed collateral valuation. A second crucial aspect of small business lending is assessing the owner's character, which may reduce the likelihood of loan approval for firms employing undocumented workers.[3] Third, banks have strong economic incentives to avoid lending to firms that employ undocumented workers, given the higher default risk due to potential non-compliance with laws. Finally, banks may also avoid engaging with such firms to protect their own reputations.

We first investigate whether bank screening exerts a deterrent effect on small business borrowers in employing undocumented workers. To do so, we examine cross-sectional differences in firms' responses to the SC program based on the presence or absence of ex-ante banking relationships. These relationships are identified using comprehensive Uniform Commercial Code (UCC) filings, which provide near-universal coverage of all secured lending in the United States and allow us to precisely link small businesses to their secured lenders (**gopal2022**).[4]

We find evidence consistent with bank screening deterring small business borrowers from employing undocumented workers. Following the activation of the SC program, treated firms without ex-ante lending relationships increased employment

[3] While it is unclear that banks' underwriting practices for small business lending explicitly verify compliance of firms' employees with immigration laws, assessing all "character traits" of borrowers—including integrity, reputation, and legal standing—is a common industry practice, which may discourage establishing a relationship with firms suspected of noncompliance. For details, see related discussion from Bank of America (https://business.bankofamerica.com/en/resources/factors-that-impact-loan-decisions-and-how-to-increase-your-approval-odds) and First Savings Bank (https://www.fsbbank.net/simplify-your-life/the-building-blocks-of-credit/).

[4] Such extensive coverage and clear identification of lending relationships are not feasible with other commonly used small business datasets, including Community Reinvestment Act (CRA) data and Small Business Administration (SBA) 7(a) and 504 loan programs.

by 0.8% and faced a 0.5 percentage point higher likelihood of closure compared with control firms. In contrast, no significant changes are observed for firms with ex-ante lending relationships. These differences are particularly pronounced for firms operating in non-tradable industries. As discussed earlier, the simultaneous rise in employment and firm closures suggests substitution of undocumented workers with costlier documented workers. The fact that this pattern appears only among unbanked firms—and not among those with prior banking relationships or among firms in tradable industries—supports the interpretation that bank screening may discourage borrower firms from employing undocumented labor.

To the best of our knowledge, the finding that bank screening may exert a deputization effect in deterring small businesses from employing undocumented workers is novel. One might question this interpretation by suggesting that banked firms were simply less likely to close after the SC program because they had access to credit. However, this explanation does not account for why unbanked firms undertook the costly decision to hire more employees despite lacking such banking relationships. Our interpretation is further reinforced by the absence of effects for firms operating in the tradable sector, where undocumented workers are known to be far less prevalent.

We further examine the deputization effect of banking relationships by distinguishing the role of bank monitoring from screening. Specifically, we assess whether the characteristic patterns of substitution for undocumented workers were stronger among firms with weaker ex-ante bank monitoring. We proxy reduced monitoring using ex-ante local branch closures of firms' lenders, given the critical role of proximity in small business lending (**agarwal2010**, **berger2005**, **granja2022**, **petersen2002**, **canales2012**). Among banked firms with below-average employment, those whose lenders reduced net branch presence in the county within three years prior to the SC program experienced significantly larger increases in both employment and exits following

treatment, relative to similar firms in control counties. In contrast, no significant treatment effects emerged for firms whose lenders did not close a branch. Moreover, these patterns are stronger for firms in non-tradable sector. Overall, these results suggest that firms subject to lower ex-ante monitoring exhibited stronger patterns of undocumented labor usage. Importantly, these cross-sectional differences cannot be attributed to credit availability effect, as both groups of firms had maintained active bank lending relationships and differed only in their lenders' local branch closures prior to the treatment.

Contribution and Literature: This is the first study to document patterns consistent with small businesses relying on undocumented workers and to emphasize the role of bank monitoring in deterring such practices. By documenting patterns consistent with this novel link between labor market informality and underwriting and monitoring processes of banks, we argue that banking relationship may foster compliance of borrowing firms not only with financial laws but also with non-financial laws.

We contribute to the recent literature at the intersection of immigration and finance. Most existing studies focus on the effects of legal immigration on public firms, startups, or immigrants' access to credit.[5] The novelty of our paper lies in its focus on *unauthorized* immigration, an area that remains remarkably underexplored. Concentrating on financial markets and firms, **cornaggia2025** show that municipal bond yields decline in tighter labor markets and rise otherwise, and **shu2025** find that both stock and municipal bond markets react negatively around ICE enforcement actions due to reduced labor supply. Focusing on labor outcomes, **east2023** show that the SC program reduced employment for likely-undocumented workers, and **howard2024** link the program to reduced construction of residential homes. In contrast, by studying small businesses and showing that they employ undocumented workers—and that

[5] See for example, **dai2025**, **aobdia2018**, **aobdia2025**, **gupta2025**, **cookson2025**.

bank monitoring deters such behavior—we contribute novel insights on the issue of unauthorized immigration. This finding on the deputization role of bank monitoring builds on and adds to the influential literature we mentioned previously regarding the crucial role of close interactions, soft information, and physical distance in facilitating bank credit to small businesses.

Our finding on the deputization role of bank monitoring is distinct from the well-documented pattern that banks' credit shocks reduce employment at borrower firms (**benmelech2021**, **chodorow2014**, **fonseca2022**). In contrast, we document an increase in employment among both ex-ante unbanked firms and likely less-monitored firms.

I. Background Information and Empirical Research Design

This section first provides background on the laws underlying the empirical research design, followed by a description of the empirical strategy and DD specification.

A. The Secure Communities Program

The SC program was launched in 2008 with the goal "to ensure that every person arrested for a crime by local law enforcement anywhere in the country is screened by the federal government for immigration violations" (**miles2014**). Prior to SC, federal immigration enforcement had focused primarily on border control, and efforts to involve state and local law enforcement in immigration enforcement were hesitated upon (**baumer2023**). As a result, identifying noncitizens within the interior typically required costly, case-by-case interviews in local jails and prisons.

The early 2000s saw a major shift in federal policies that now targeted heightened internal surveillance, leading to the implementation of the SC program. It mandated local law enforcement agencies to share fingerprint data for all individuals detained, arrested, or incarcerated with the Department of Homeland Security (DHS), ICE, and the Federal Bureau of Investigation (FBI). This integration effectively granted ICE a

universal technological presence across U.S. jurisdictions, substantially increasing the risk of detection, detention, and deportation for undocumented immigrants. The program began with a pilot initiative in 14 jurisdictions in October 2008 and was expanded to approximately a quarter of all U.S. counties by 2010 and reached full coverage in early 2013. Figure 1 illustrates the program's rollout.

[Insert Figure 1 About Here]

The staggered roll-out of the SC program across the counties, combined with several other features, makes it an attractive natural experiment for causal empirical analysis. First, the federal government, not local jurisdictions, determined the sequence of the rollout. The timing of implementation was mainly determined by immigration enforcement priorities rather than local small business activities. Accordingly, **cox2013** show that the strongest predictors of early implementation were a county's location on the southern border and the fraction of its population that was Hispanic. Second, the program's structure made informal noncompliance by local government practically impossible, as the program simply rerouted the fingerprint data to federal agencies. This inability for local governments to "opt out" or resist activation ensures that the timing of announcement and program's activation is consistent. This feature makes SC particularly well-suited for causal attribution, in contrast to the other internal enforcement programs such as 287(g) agreements or E-Verify, adopting which was voluntary, and hence prone to selection bias.

Beyond leveraging biometric data, the Secure Communities program also increased the risk of deportation by eliminating the need for a criminal conviction. The program's design meant that any non-citizen, regardless of their legal status, who was fingerprinted during a routine local arrest could be flagged for deportation. This includes individuals arrested for minor infractions, traffic violations, or even those who were innocent but fingerprinted as part of a formal booking process. Unlike

previous policies that often prioritized the deportation of individuals with serious felony convictions, SC's automated and broad-based approach significantly expanded the net of potential deportees, making it possible for individuals with no criminal record or a clean record to face removal proceedings. This created a heightened risk for deportation for all undocumented immigrants, not just those engaged in criminal activity.

B. DiD Estimator

To implement the DiD design centered around the staggered rollout of the SC program, we favor **callaway2021**'s CSDID estimator over the traditional two-way fixed effects (TWFE) model, since the latter can yield biased average treatment effect estimates under heterogeneous treatment effects and staggered treatment timing due to the negative weights problem (**de2020**, **goodman2021**, **borusyak2024**). CSDID estimator is also more appropriate in this setting over a stacking approach for two key reasons. First, CSDID allows the control units to vary across different treatment horizons, whereas stacking approach requires the control units to remain fixed across all treatment horizons over the entire event window. Given that the SC program reached nationwide coverage within only four years, stacking becomes infeasible due to a lack of consistent control units, while CSDID's flexibility in constructing counterfactual control groups across treatment horizons makes it well-suited for this setting. Second, in geographic staggered rollout settings, stacking typically identifies controls based on spatial proximity—such as bordering or same-state counties. However, since the SC program was implemented in geographically clustered phases across a haphazard set of counties that did not align with regular administrative boundaries, applying such spatial criteria would, at a minimum, discard substantial treatment variation, if not render the approach entirely infeasible.

We use the following regression specification to estimate the overall treatment effect (ATTGT), which is obtained by averaging the group-time-specific average treatment effects on the treated (ATT):

$$\mathrm{Y}_{ict} = \alpha_0 + \beta \times \mathrm{SC}_{ct} + \mathrm{Controls} + \delta_i + \gamma_t + \epsilon_{ict}, \tag{1}$$

where Y_{ict} represents the outcome variable in year t for a firm i in a county c. SC_{ct} equals zero for counties that have not yet implemented the Secure Communities program and one for those who have implemented. β is the coefficient of interest capturing the average change in the dependent variable before and after the event in the treated counties relative to the control ones. *Controls* consist of a host of firm characteristics as well as county-level socioeconomic characteristics and vary across specifications. We include both the county fixed effects (δ_c) and year fixed effects (γ_t) to flexibly account for local time-invariant characteristics and arbitrary shocks at the county level. Standard errors are clustered at the county level in all specifications to account for correlation in error terms.

To construct event study plots, we estimate the dynamic version of equation (1) as

$$Y_{ict} = \sum_{\tau=-4,\, \tau\neq-1}^{2} \beta_\tau \mathbb{1}(\tau) + \mathrm{Controls} + \delta_i + \gamma_t + \epsilon_{it}, \tag{2}$$

where $\mathbb{1}(\tau)$ equals one when the relative year to the event year is τ. All other terms are as previously defined.

B.1. Was the Staggered Rollout of Secured Communities Predictable?

One natural concern to our empirical strategy is that the Secure Communities Program was not implemented in specific counties randomly. To examine this concern, utilize the timing of the implementation of the Secure Communities Program and use the Cox hazard model to analyze the determinants of the implementation. The "failure event" in the model is the implementation of the SC Program in a county, such that a county is excluded from the sample after the program's activation. The dependent variable is

the number of years from 2008 until a county had the Secure Communities Program implemented. As shown in Table II, column (5), larger total population and a larger Hispanic population predict earlier program implementation, consistent with **cox2013**, **miles2014**. Importantly, however, the timing of the rollout was not predictable based on county-level economic outcomes such as per-capita income, the growth rate in the number of establishments, average establishment wages, or average employment per establishment (**east2023**).

[Insert Table II About Here]

II. Data and Summary Statistics

We utilize data from a variety of sources. The primary data is the rollout dates of the Secure Communities Program from the **ICE2014**, which we described earlier. Our second main data is about small businesses. We use the 2021 version of the National Establishment Time-Series (NETS) database, an establishment-level dataset compiled by Walls and Associates based on D&B credit registry records. NETS provides annual information for a broad segment of the US economy, including establishment-level employment, NAICS industry classifications, and closure status. Following **chava2023**, we start with standalone U.S. businesses with valid PAYDEX scores and exclude establishments with only one employee, as well as firms in finance, real estate, utilities, and professional services, since these sectors are less likely to employ undocumented immigrants. In addition, we restrict the sample to firms with fewer than 500 employees between 2007 and 2013, and exclude public, nonprofit, and foreign-owned firms. After applying these filters, our final sample consists of 6.6 million small businesses over the sample period.

We then combine our establishment data with information on firms' borrower-lender relationships. Loan data are obtained from state-level public records filed under the Uniform Commercial Code (UCC). The UCC is a set of laws governing commercial

transactions, with Article 9 specifying creditor rights in business lending. Creditors have the right to file a public record with the UCC registry that declares a loan and its collateral, which is referred to as a "UCC filing." These filings are important in determining a secured lender's priority in case of borrower default. Secured lenders without an active UCC filing are considered unsecured creditors by law. Given the legal importance and low cost of filing (typically $15 to $25), secured lenders have strong incentives to file, and most routinely do so.

Our UCC data come from a commercial vendor that compiles filings from all 50 US states and Washington D.C. for the years 2006 to 2022. Each filing includes information on the borrower and lender (names and addresses), as well as the filing and expiration dates. The vendor also provides the data with DUNS numbers, NAICS codes, geographic identifiers, and firm-level characteristics from external sources. To construct our sample, we merge these UCC filings with the NETS data using the DUNS number and restrict the sample to loans with filing dates between 2006 and 2016. Of the 6.6 million small businesses in NETS, 592,680 firms have a recorded borrowing relationship within three years before the implementation of the Secure Communities Program. Following **gopal2022**, we identify the lender type, including whether the lender is a bank.

Data on bank branch closures are obtained from the Federal Reserve System's publicly available National Information Center (NIC) database. The NIC provides information on bank branches, ownership structures, and regulatory activity. We merge small businesses with branch closure information using UCC filings' standardized lender names available from **gopal2022**.

Finally, we complement our analysis with a range of county-level socio-economic indicators drawn from publicly available sources. Population counts come from the Census Bureau's annual county resident population estimates. Information on

establishments and payroll at the county–industry level is drawn from the Quarterly Census of Employment and Wages (QCEW). In addition, we incorporate county-level personal income data from the Bureau of Economic Analysis.

Finally, given that the geographic distribution of the undocumented population is highly uneven across states (**passel2009**), we restrict the sample to the states that together accounted for 95% of the undocumented immigrant population in 2008. This restriction yields 28 states, which forms our baseline sample.[6] This sample criterion sharpens the identification of the SC program's effects on industry outcomes by reducing estimation noise.

[Insert Table I About Here]

Table I shows the summary statistics for the key variables within the sample states. Panel A reports firm-level characteristics. A typical firm in our sample employs approximately seven workers and its mean probability of exit is 6%. Panel B reports county-level variables. Panel B presents county-level data on the number of small business establishments, average wages, employment, population, and per capita income.

III. Results

Our empirical analysis focuses on the employment of undocumented workers by small businesses, which is an inherently unobservable activity. Hence, to ensure credible interpretations, we structure the analysis in two stages. In the first stage, we draw on reliable and widely used public data to examine outcomes aggregated at the county level. Establishing the key empirical patterns in these official data lends credibility to our interpretations and motivates our subsequent firm-level analysis, for which we use the industry-produced NETS dataset. Although this dataset is subject to imputation

[6] The 28 states are: CA, TX, FL, NY, NJ, AZ, GA, IL, NC, VA, MD, CO, NV, MA, WA, TN, OR, PA, IN, UT, MI, MN, CT, AL, OH, WI, NM, and KS.

concerns, it has been used extensively in small business literature.[7] We replicate the key aggregate patterns in this dataset and then leverage its rich firm-level information to uncover additional insights. We further alleviate the imputation concerns through multiple empirical strategies.

A. County-Level Results

Using QCEW data and the 4-digit NAICS industry classification into tradable and non-tradable sectors from **mian2014**, we construct county-level sectoral aggregates for three outcomes: total establishments, average wages per establishment, and average employment per establishment.[8] We estimate treatment effects using the CSDID estimator without covariates based on the following county-level analogue of equation (1):

$$Y_{ct} = \alpha_0 + \beta \times SC_{ct} + \delta_c + \gamma_t + \epsilon_{ct}. \tag{3}$$

Table III reports the average treatment effect on the treated (ATT), aggregated across treatment cohorts and post-treatment periods, separately for the three outcomes and for each sector. Panel A reports sectoral effects, with columns (1) to (3) corresponding to the non-tradable sector and columns (4) to (6) to the tradable sector. Relative to counties without the SC program, treated counties experience a 1.7% decline in the number of establishments, a 3.1% increase in average wages per establishment, and a 3.7% increase in average employment per establishment in the non-tradable sector. By contrast, the tradable sector, which is less exposed to the SC program because it predominantly employs skilled formal workers and therefore serves as placebo group reflecting the effects of contemporaneous macroeconomic changes, shows no statistically significant effects across any of the three outcomes (columns (4) to (6)).

[7] See for example, **barrot2020**, **denes2023**, **addoum2020**, **addoum2023**, **chava2023**, **acharya2023**.

[8] As mentioned earlier, for brevity, we refer to non-tradable and construction industries together as "non-tradable", and the tradable and others as "tradable." Also, our estimates are based on the 28 states that accounted for 95% of the undocumented population in 2008, listed in footnote 6.

Finally, the unemployment rate increases by 0.112 percentage points following the SC program. We return to it below when discussing the segregated estimates reported in Panel B and Panel C.

[Insert Table III About Here]

A notable feature of the undocumented population is that nearly 94% reside in metropolitan areas (**passel2009**). Accordingly, we expect the effects of the SC program, which targets this population, to be stronger in counties classified as metropolitan. Using the 2006 urban–rural classification from NCHS, we define a county as metro if it contains a large central metro, large fringe metro, or medium metro. Panels B and C of Table III report the above results estimated separately for metro and non-metro counties. In line with the intuition, the treatment effects on all three outcomes are significant in non-tradable industries in metro counties (Panel B, columns (1) to (3)), whereas largely non-significant in tradable industries of metro counties (Panel B, columns (1) to (3)) and both sectors in non-metro counties (Panel C). The coefficient in column (7) of Panel B indicates that the SC program increased the unemployment rate in metro counties by 0.214 percentage points, whereas there is no significant change in the unemployment rate in non-metro counties (Panel C, column (7)).

Figure 2 shows the event-study plots corresponding to the estimates for the non-tradable industries in metro counties.[9] Panel A to C of the figure suggest that the treated and control counties were not experiencing significantly diverging trends prior to the treatment with respect to the three industry outcomes and alleviate the concern that the parallel trends assumption might not hold. Panel D affirms the same for unemployment rate.

[Insert Figure 2 About Here]

[9] The specification for event-study plots at the county level follows the county version of equation (2): $Y_{ct} = \sum_{\tau=-4,\,\tau\neq-1}^{2} \beta_\tau \mathbb{1}(\tau) + \gamma_c + \eta_t + \epsilon_{ct}$, where $\mathbb{1}(\tau)$ equals one when the relative year to the event year is τ.

To interpret the observed changes in industry patterns following implementation of the SC program, we rely on two key premises. First, official data collected by the Census Bureau in the QCEW and in unemployment statistics do not capture undocumented workers, as such workers and their employers are unlikely to participate or report accurately in official surveys (**bohn2017**). Second, the SC program represents a negative labor supply shock to undocumented workers by substantially increasing their risk of arrest and deportation, even from minor interactions with local law enforcement.

Under these premises, the simultaneous rise in employment and wages alongside firm closures suggests that firms previously employing undocumented workers reduced or eliminated these positions and substituted formal employees. This substitution, likely imperfect, accounts for both the increase in reported employment and the higher wages, reflecting the greater cost of formal labor. The decline in the number of establishments likely results from higher labor costs rendering some firms financially unviable, ultimately leading to closures. The absence of these effects for tradable-sector industries in the same counties that are also exposed to the SC program further supports these interpretations, since these industries are less likely to employ undocumented workers. Finally, the seemingly paradoxical increase in both average employment per establishment and the overall unemployment rate likely reflects that many establishments closed: while employment in surviving firms rose, population-level unemployment increased due to the closures.

Economically, the estimated effects are non-trivial. The 1.7% decline in non-tradable sector firms corresponds to the closure of roughly 35,000 establishments nationwide, with potential losses in production, service provision, and tax revenues from the closed firms. Moreover, the estimated increase in the unemployment rate of 0.112 percentage points across all counties corresponds to approximately a 1.4% rise relative to the

sample mean unemployment rate of 7.9%. Because the official unemployment rate primarily reflects unemployment among formal (i.e., non-undocumented) workers (**bohn2017**), this result implies that employment outcomes for formal workers did not improve following adverse shocks to undocumented labor. In other words, undocumented labor does not substitute away the employment that otherwise could have been taken by formal labor. These findings are consistent with the results in **east2023** and **howard2024**.

For the rest of the analysis, we refer to the concurrent rise in (formal) employment and firm closures as the *characteristic pattern* signaling firms' prior reliance on undocumented workers before the SC program.

B. Firm-Level Evidence

We next examine whether the characteristic patterns indicative of undocumented worker employment, identified in county-aggregated industry data, are observable at the firm level using the NETS dataset. Because this dataset does not contain wage information, we focus on firm-level employment counts and an indicator for permanent firm closure (*exit*). Firm closures are particularly informative, as they are well-measured and not subject to the imputation issues that can affect employment counts. We estimate these outcomes using the CSDID estimator and the specification in equation (1). This specification includes firm and year fixed effects, making the estimates robust to time-invariant differences across both firms and counties, such as proximity to the Southern border. Standard errors are clustered at the county level, which corresponds to the level of treatment assignment.

[Insert Table V About Here]

Table V presents the results. Columns (1) to (4) report estimates for firms across all sample counties. The first two columns are without controls, while columns (3) and (4) include three county-level covariates: the natural logarithms of per capita

income, population, and Hispanic population. The coefficients in columns (3) and (4) indicate that firm-level employment increased by 0.6%, while the firm closure rate rose by 0.7 percentage points, equivalent to an 11.4% increase relative to the pre-event closure rate of 6.12%. Columns (5) to (8) present the results for metro counties, and columns (9) to (12) for non-metro counties. Comparison of columns (7) with (11) for employment increase (0.7% versus 0.3%) and (8) with (12) for firm-closure rate (0.8% with 0.6%) confirms that the SC program's effects are stronger in metro than in non-metro counties.

[Insert Figure 3 About Here]

Figure 3 presents the event-study plots for firm-level employment and firm closure corresponding to columns (7) and (8) of Table V. Panels A and B show that both employment and firm closures increased following the policy. Notably, for two of the three pre-treatment periods, the outcomes in untreated counties are lower than in treated counties. The reason for this difference becomes apparent from the cohort-specific dynamic treatment effects plotted below the respective panels. The cohort-specific trends indicate that the difference is driven by the two earliest cohorts treated in 2009 and 2010. As shown in Figure 1, these cohorts were located near the Southern border, where large populations of undocumented workers reside. Consequently, firms in these counties may have relied more heavily on undocumented labor and fewer formal workers in the pre-treatment period, resulting in lower measured formal employment and a lower likelihood of closure due to relatively lower wage expenses of such workers.

Two additional observations mitigate concerns about the level differences in the two pre-treatment periods. First, the deviations of these coefficients from zero are small

relative to the magnitude of the treatment effect. Second, these differences run in the opposite direction of the treatment effect.[10]

Overall, our results indicate that the observed patterns of employment and firm closures at both the county and firm levels following the implementation of the SC program are consistent with firms' ex-ante reliance on undocumented workers. Moreover, because undocumented workers tend to reside in MSA counties (**passel2009**), as reaffirmed by the stronger observed effects in both county- and firm-level analyses, we henceforth focus on the sample of firms located in MSA counties.

C. Heterogeneity by Industry Type

We next examine whether firm-level effects on employment and closure rates are stronger for firms operating in non-tradable industries, a pattern previously observed in county-level outcomes. Using the CSDID estimator and the specification in equation (1), we estimate effects on employment and firm closures separately for firms in non-tradable and tradable industries. Table VI presents the results. Columns (1) and (2) show that firms in non-tradable industries exhibit the characteristic pattern of rising employment alongside higher closure rates: employment increased by 0.4%, while closure rates rose by 0.8 percentage points following the SC program. In contrast, the effects for firms in tradable industries are not statistically significant, as shown in columns (3) and (4). Given that non-tradable industries tend to employ undocumented workers, these results indicate that the SC program affects outcomes primarily through shocks to undocumented labor supply. Additionally, by replicating the industry-level differences in SC program treatment effects observed in county-aggregated measures (Table III) using QCEW data, these results help alleviate coverage concerns associated with the NETS dataset.

[10] Minor differences in the pre-treatment period, though not ideal, are not uncommon. For example, pre-trends in **lovenheim2019** also run opposite to the treatment effect, as noted in (**rambachan2023**).

[Insert Table VI About Here]

D. Bank Screening and Small Business Employment of Undocumented Workers

We now leverage the firm-level data to examine whether bank screening may have a deterrent effect on small businesses from employing undocumented workers. Considerable incentives exist for banks to avoid lending to firms that employ such workers. First, firms employing undocumented workers face higher legal and operational risks, and thus greater default risk. Second, banks may refrain from lending to such firms to protect their reputation. Third, the screening of small business loans often involves the collection of soft information through close interactions and site visits (**agarwal2010**, **petersen2002**, **heitz2022**), during which process banks may acquire such information. Such screening is particularly more elaborate in the case of secured loans, which are larger in size and require detailed information about the business for collateral valuation. Moreover, an established practice in the banking industry is to assess the owner's character, which would be undermined if the firm employs undocumented workers (see footnote 3 for details).

Building on these considerations, we argue that banks not only have strong incentives to avoid lending to such firms but are also uniquely positioned to acquire relevant information through their screening practices. As one of the few formal institutions with a continuous local presence, banks regularly and closely interact with small businesses, placing them in an advantageous position to detect such risks. This leads to our screening hypothesis: firms successfully screened by banks and maintaining an established banking relationship are less likely to employ undocumented workers than those without such relationships.

Identifying the deterrent role of bank screening in borrowers' employment of undocumented workers in a causal manner is challenging, as it would ideally require observing the outcomes of bank screening for firms randomly assigned to different

banks, with some employing such workers and others not. While such an ideal experiment is infeasible, we shed light on this mechanism by examining cross-sectional differences in the effects of the SC program between small businesses with and without an ex-ante banking relationship for a secured loan. If banks exert a deterrent effect, the *characteristic patterns* of rising employment and closures following the SC program should be more pronounced among firms without such relationships, since those with the relationship have already been screened and are thus less likely to employ undocumented workers, according to our screening hypothesis. Notably, the credit availability alone—absent the screening effect on undocumented workers—cannot explain why the ex-ante unbanked firms, despite lacking access to credit, would undertake the financially costly adjustment of increasing employment after the quasi-exogenous activation of the SC program in the county.[11]

To test the prediction, we draw on the UCC dataset. Its near-universal coverage allows us to cleanly observe both whether a small business had a secured loan and the identity of the lender. We classify the firms into two groups. Those with a valid UCC record for a secured bank loan taken within the three years prior to the SC program until the pre-treatment year are classified as having a banking relationship, while firms with no UCC record are classified as having no access to a secured bank loan.[12] Using the CSDID estimator and regression equation (1), we estimate the effect of the SC program on employment and closure separately for the two groups.

[Insert Table VII About Here]

[11] Specifically, the screening hypothesis explains both patterns: higher employment, reflecting substitution for previously undocumented workers, and higher closures, driven by the increased wage costs of formal labor.

[12] Small business owners may also take unsecured personal loans to finance their operations. However, since such loans are unsecured and do not require evaluation of business operations or site visits, the deterrent effect of banks does not apply. Therefore, for testing the cross-sectional prediction, these firms should be classified as having no banking relationship.

Table VII presents the results. Columns (1) and (2) show that among ex-ante unbanked firms, employment and exit rates increased by 0.8% and 0.5 percentage points, respectively, following the SC program compared to unbanked firms in control counties. In contrast, the corresponding effects for ex-ante banked firms in the treated counties relative to the control are statistically insignificant and negligible in magnitude in columns (3) and (4). These pronounced *characteristic patterns* observed only among unbanked firms are consistent with the screening hypothesis, suggesting that banking relationships deter borrower firms from employing undocumented workers.

To further reaffirm the screening hypothesis, we leverage the cross-sectional differences across industries. Our previous results indicate that the effect of the SC program is stronger for non-tradable sectors, because of the folk wisdom that undocumented workers tend to take employment in these sectors. Combining this observation with the screening hypothesis suggests that the *characteristic patterns* should be more pronounced across ex-ante banked and unbanked firms in these sectors than those in tradable sectors. We test this prediction next by now examining the differences across firms in the two industry sub-samples.

[Insert Table VIII About Here]

Table VIII presents the results. Columns (1) and (2) show that unbanked firms in non-tradable industries in treated counties exhibit the characteristic rise in employment and closures—0.5% and 0.6 percentage points, respectively—relative to unbanked firms in control counties. Columns (3) and (4) indicate an absence of these patterns among ex-ante banked firms in non-tradable industries. Finally, columns (5) to (8) show that tradable-sector firms exhibit no increase in the characteristic patterns for either ex-ante banked or unbanked firms in treated counties relative to control counties.

Overall, these differential effects support our screening hypothesis that bank screening may deter small businesses from employing undocumented workers.

E. Bank Monitoring and Small Business Employment of Undocumented Workers

Even after successfully screening small businesses, banks continue to monitor borrower performance through site visits, interactions, and documentation (**heitz2022**). However, screening is unlikely to be perfect, leading some borrowers to employ undocumented workers, despite having a banking relationship. Consequently, the ongoing bank monitoring may deter such employment, potentially by using credit availability as a disciplining mechanism. This deterring effect of bank monitoring is particularly plausible for smaller firms, which typically are more dependent on bank credit and may escape stricter screening.[13]

Accordingly, we focus on firms with active banking relationships and below-sample-mean employment to assess the deterrent effect of monitoring on the employment of undocumented workers. This analysis is inherently challenging because both the intensity of bank monitoring and the availability of credit are jointly determined and may simultaneously affect firm employment and closure. Importantly, the two mechanisms influence these outcomes in distinct ways. The credit availability mechanism primarily affects firms that relied on undocumented workers prior to the SC program by helping them finance the higher cost of formal labor, thereby reducing closures but increasing formal employment after the program. However, the bank monitoring mechanism operates through deterrence. Therefore, firms with more intensive ex-ante monitoring are less reliant on undocumented workers and therefore less affected by the SC program for both employment and closure. By contrast, firms subject to less intensive ex-ante monitoring are more likely to employ undocumented workers and thus are more exposed to the program, experiencing simultaneous increases in *both employment and closures*. We rely on this distinction in outcomes to test for the deterrence effect of bank monitoring.

[13] The distinction between the deterrent effects of monitoring and screening is that screening operates on the extensive margin, whereas monitoring operates on the intensive margin.

Specifically, we examine changes in employment and firm closures among firms that maintained access to secured bank credit but differed in the monitoring capacity of their lenders during the pre-treatment period. We compare firms whose lenders reduced the net number of local branches in the county between three and one year prior to the activation of the SC program with firms whose lenders did not. Because both groups retained access to credit until the treatment, differences in their responses capture the effect of reduced ex-ante monitoring arising from reduction in local branches, while holding credit availability relatively constant. Measuring branch closures prior to the treatment also ensures that these decisions were independent of the SC program's activation in the county. We therefore test whether, among smaller firms employing below-mean workers in the pre-treatment year, those whose lenders closed local branches ex-ante exhibited more pronounced effects on employment and exit than those whose lenders did not.

[Insert Table IX About Here]

Table IX reports regression results based on equation (1) using the CSDID estimator. We find that firms whose lenders had closed branches ex-ante experienced significantly larger increases in both employment and exit following SC implementation (columns (1) and (2)) compared with firms whose lenders did not (columns (3) and (4)). The difference is particularly pronounced among firms operating in non-tradable industries. Specifically, employment increased by 2.3% for firms experiencing ex-ante lender branch closures, compared with 1.2% for firms whose lenders did not close branches (columns (5) versus (7)). Closure rates rose by 0.3 percentage points for the former (column (6)), while no statistically significant increase was observed for the latter (column (8)).

An empirical limitation of these results is the drop in sample size, which arises from the relatively small number of counties experiencing net branch closures and the

imperfect matching of lender names in UCC filings and bank branch data. Nonetheless, these findings support the view that bank monitoring plays a deterrent role in firms' employment of undocumented workers.

F. Can Decline in Consumption Explain the Characteristic Patterns?

A potential concern is that firm closures may have increased following the SC program due to reduced consumption resulting from out-migration of undocumented populations from treated counties. To examine this, we re-estimate the results using specifications that account for firm-level sales, as declining consumption would affect firms through decline in sales. Because sales in NETS are subject to imputation issues, we compute annual sales quartiles of firm-level sales and include them as fixed effects. Tables A.1 to A.5 in the Online Appendix show the results corresponding to Tables V to IX. All conclusions remain unchanged.

IV. Conclusion

In this paper, we examine the employment of undocumented workers by small businesses. We circumvent the inherent unobservability of undocumented labor by combining quasi-exogenous shocks resulting from the Secure Communities (SC) program with detailed data on formal worker employment. We leverage the staggered rollout of this program across counties and **callaway2021**'s difference-in-differences estimator to draw causal conclusions.

Our key finding is that following the activation of the SC program, which served as a negative shock to undocumented labor, both formal employment and firm closures increased in county-aggregated and firm-level outcomes. These patterns are consistent with firms previously relying on undocumented workers, who were likely replaced with costlier formal workers, raising measured employment. The higher wages of formal labor may have rendered some firms unviable, leading to increased

closure rates. These effects are particularly pronounced in non-tradable sectors and metropolitan counties, consistent with the concentration of undocumented workers in these industries and areas.

We also document patterns consistent with deputization-like effect of bank screening and ongoing monitoring on small business' employment of undocumented workers. Banks, through their local presence, expertise in gathering soft information, and incentives to limit default risk and protect reputation, can influence undocumented labor employment by the borrower firms. Consistent with the deterrent effect of bank screening, increased employment and closures following the SC program were concentrated among firms without an ex-ante banking relationship. Among banked firms, those experiencing reduced ex-ante monitoring due to local branch closures of their lenders experienced larger increases in employment and closures, consistent with the deterrent effect of monitoring.

Overall, our study provides novel evidence that banks can influence borrower firms' informal labor practices beyond their traditional credit intermediation role and may encourage their compliance with even non-financial regulations.

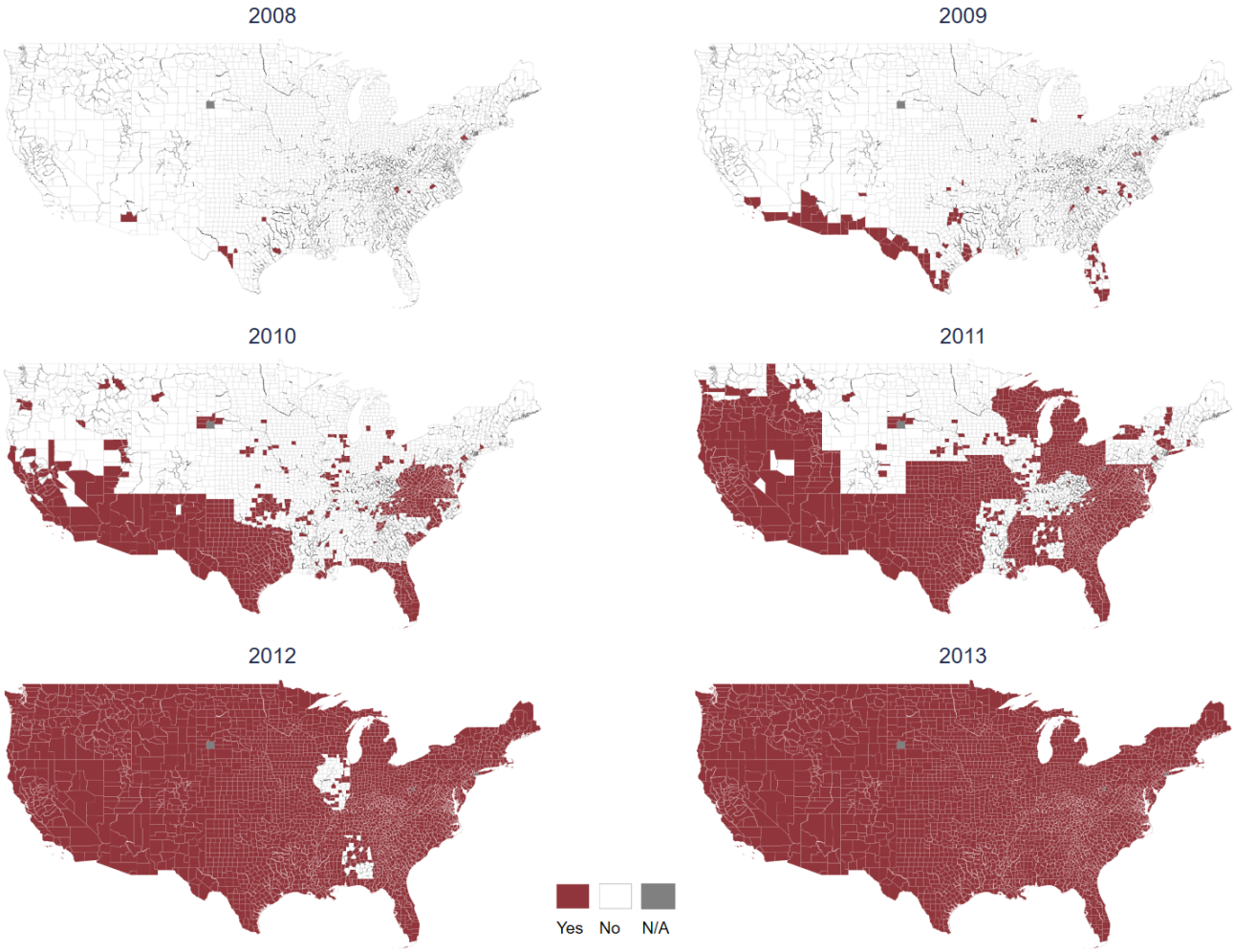


Figure 1. Staggered Rollout of Secure Communities

This figure shows the counties that have implemented the Secure Communities program within each year.

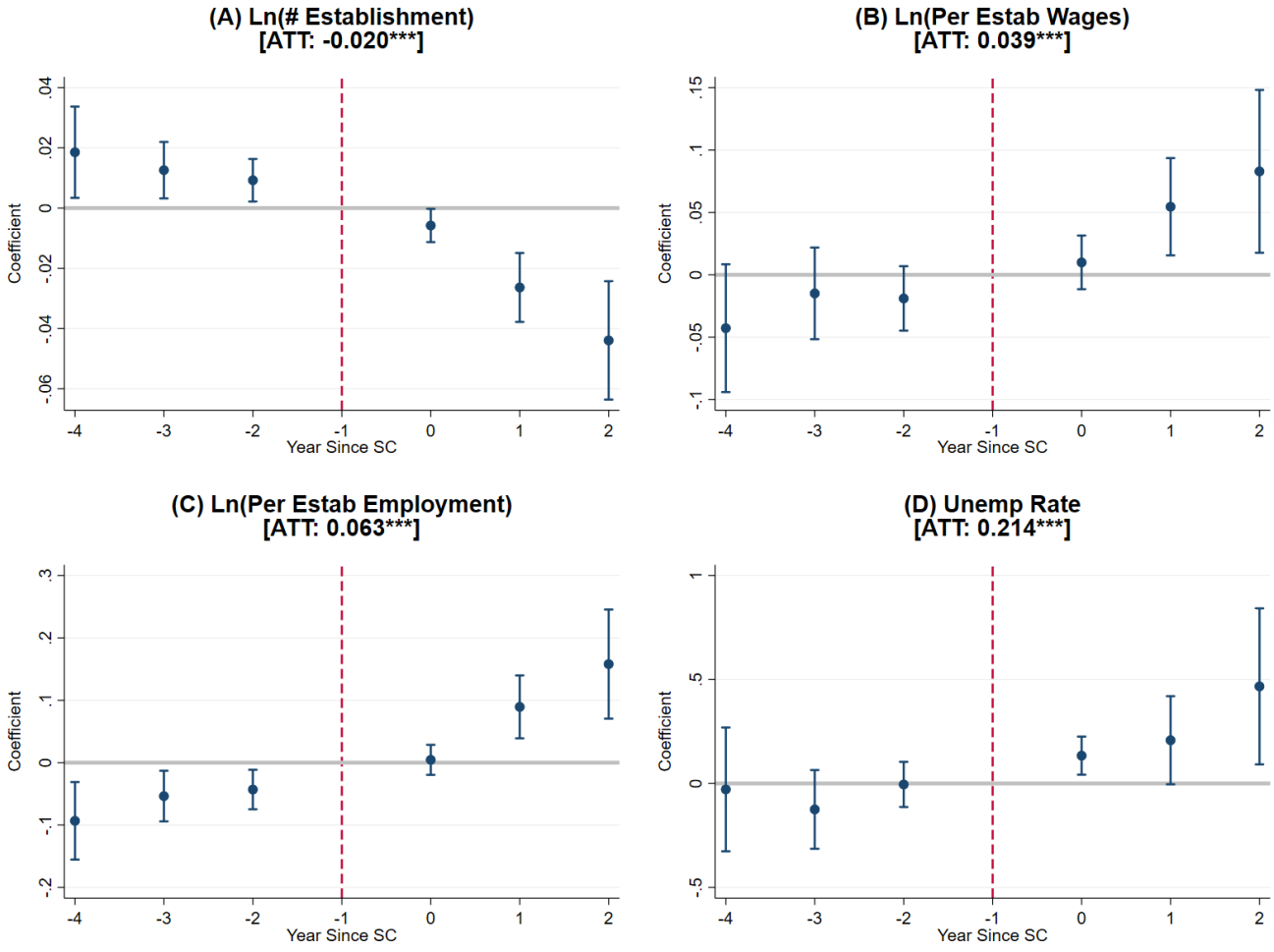


Figure 2. Event-study Plots: County-Level Industry Outcomes

This figure plots the estimated β_τ coefficients and their 95% confidence intervals from the county-level version of equation (2), as described in footnote 9. These estimates are obtained using the **callaway2021** CSDID estimator without covariates and correspond to those reported in Table III, Panel B, columns (1) to (3) and (7). The outcomes are denoted in respective panels. Panels A to C show outcomes for non-tradable and construction sectors, and Panel D shows the plot for unemployment rate (across all industries). The horizontal axis denotes years relative to the implementation of the SC program, with $\tau = 0$ representing the first treated year. A county is classified as treated in years when the SC program was operational for at least half of the year. Confidence intervals are based on standard errors clustered at the county level.

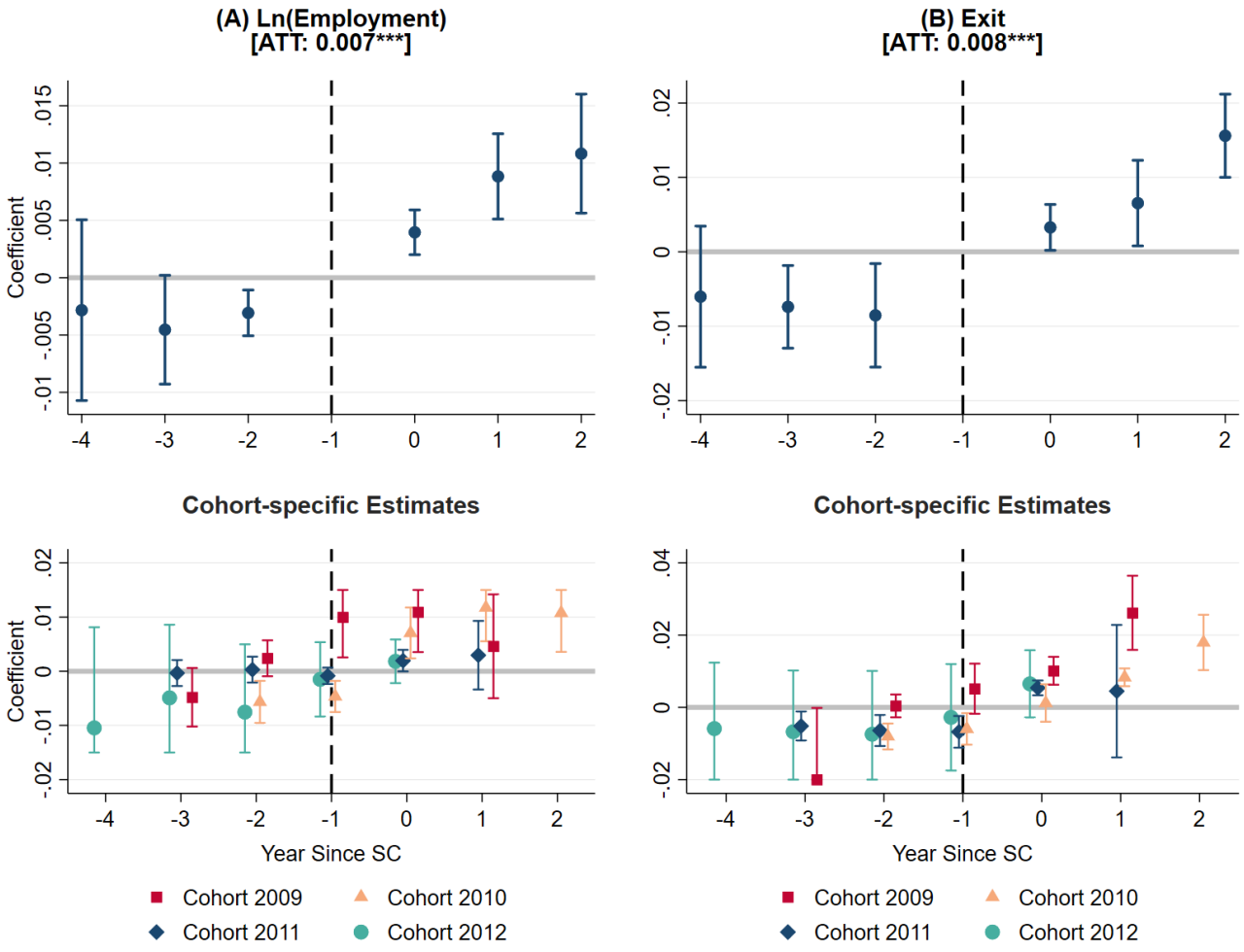


Figure 3. Event-Study Plots: Firm-level Outcomes for Small Businesses

This figure shows the coefficients β_τ and their 95% confidence intervals from regression equation (2) for firm-level employment and closure. The estimates are obtained using the **callaway2021** CSDID estimator and correspond to those reported in Table III, columns (7) and (8). The horizontal axis denotes years relative to the treated year, with $\tau = 0$ representing the first treated year. Confidence intervals are based on standard errors clustered at the county level. Cohort-specific dynamic treatment effects are displayed below the respective panels.

Table I
Summary Statistics

This table reports summary statistics for 2007–2013. *Emp* is the firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. The top three variables in Panel B are county-level per capita income, population, and Hispanic population. We use the natural logarithm of one-year lagged values of these three variables as *Controls* in the regressions. *# Est* is the number of establishments in a county. *Avg Wages* is the average wages paid by each establishment. *Avg Emp* is the average employment of each establishment. These variables are presented separately for non-tradable and construction sector, and tradable sector.

	N	Mean	P25	Median	P75	SD
	(1)	(2)	(3)	(4)	(5)	(6)
A. Firm×Year-Level Variables						
Emp	22,798,247	7.14	2.00	3.00	6.00	11.33
Exit	22,798,247	0.06	0.00	0.00	0.00	0.24
B. County×Year-Level Variables						
Per Capita Income (000's)	13,545	35.13	29.30	33.41	38.58	9.29
County Population (000's)	13,545	128.84	14.64	34.34	96.13	380.37
Hispanic Population (000's)	13,863	12.94	0.25	0.87	3.85	82.76
Non-Tradable & Construction:						
# Est	13,860	882.55	102.00	252.00	683.00	2338.16
Avg Wages (000's)	13,859	187.10	83.27	157.38	269.52	134.47
Avg Emp	13,859	7.21	3.88	6.58	10.32	4.24
Tradable:						
# Est	13,712	125.79	13.00	32.00	90.00	421.23
Avg Wages (000's)	13,697	472.99	0.00	100.57	609.73	928.29
Avg Emp	13,697	8.66	0.00	2.80	12.90	13.75

Table II
The Determinants of SC Rollout: Cox Proportional Hazard Model

This table presents results from Cox proportional hazard model estimations. The reported coefficient estimates are hazard ratios. The "failure event" is the implementation of the Secure Communities program in a county, and the county is excluded from the sample post the event. The dependent variable is the number of years from 2008 until a county had implemented SC. In column (1), we include the one-year lagged growth rates of # *Est*, *Avg Wages*, and *Avg Emp*, defined in Table I. In columns (2) through (5), we gradually include the county-level natural logarithm of one-year lagged population, hispanic population, and per capita income, and lagged net international migration measured in thousands. All models include year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

	(1)	(2)	(3)	(4)	(5)
L.# Est Growth	-0.565	-0.488	-0.581	-0.472	-0.491
	(0.385)	(0.441)	(0.442)	(0.456)	(0.456)
L.Avg Wages Growth	-0.446	0.181	0.211	0.255	0.246
	(0.292)	(0.338)	(0.330)	(0.334)	(0.333)
L.Avg Emp Growth	1.058**	0.322	0.250	0.223	0.246
	(0.453)	(0.533)	(0.517)	(0.517)	(0.514)
L.Ln(County Population)		0.148***	-0.084***	-0.083***	-0.087***
		(0.010)	(0.017)	(0.017)	(0.017)
L.Ln(Hispanic Population)			0.194***	0.196***	0.194***
			(0.012)	(0.012)	(0.012)
L.Ln(Per Capita Income)				-0.060	-0.071
				(0.052)	(0.052)
L.Net Intl. Migration					0.018
					(0.021)
FE: Year	Y	Y	Y	Y	Y
Observations	10,764	10,666	10,657	10,657	10,657

Table III
County-Level Industry Outcomes

All columns report estimates from equation (3) using CSDID estimator. Panels A to C report results for all counties, metropolitan counties, and non-metropolitan counties, respectively. *Ln(# Estab)* is the natural logarithm of the number of establishments in a county. *Ln(Avg Wages)* is the natural logarithm of the average wages paid by each establishment. *Ln(Avg Emp)* is the natural logarithm of the average employment of each establishment. *Unemp Rate %* is the number of unemployed people as a percentage of the labor force. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. All models include county and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

A: All Counties							
Industry Type:	Non-Trda & Cons.			Tradable			All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome:	Ln(# Est)	Ln(Avg Wages)	Ln(Avg Emp)	Ln(# Est)	Ln(Avg Wages)	Ln(Avg Emp)	Unemp Rate %
SC	-0.017***	0.031**	0.037**	0.010	0.005	0.003	0.112**
	(0.003)	(0.014)	(0.016)	(0.007)	(0.070)	(0.060)	(0.054)
FE: Year, County	Y	Y	Y	Y	Y	Y	Y
Observations	11,878	11,575	11,575	11,732	7,494	7,494	11,853
B: Metropolitan Counties							
Industry Type:	Non-Trda & Cons.			Tradable			All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome:	Ln(# Est)	Ln(Avg Wages)	Ln(Avg Emp)	Ln(# Est)	Ln(Avg Wages)	Ln(Avg Emp)	Unemp Rate %
SC	-0.020***	0.039***	0.063***	-0.006	0.182	0.136	0.214***
	(0.005)	(0.014)	(0.019)	(0.011)	(0.162)	(0.140)	(0.081)
FE: Year, County	Y	Y	Y	Y	Y	Y	Y
Observations	3,420	3,413	3,413	3,419	2,878	2,878	3,399
C: Non-Metropolitan Counties							
Industry Type:	Non-Trda & Cons.			Tradable			All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Outcome:	Ln(# Est)	Ln(Avg Wages)	Ln(Avg Emp)	Ln(# Est)	Ln(Avg Wages)	Ln(Avg Emp)	Unemp Rate %
SC	-0.015***	0.025	0.020	0.015	-0.089	-0.064	0.071
	(0.004)	(0.019)	(0.020)	(0.009)	(0.059)	(0.051)	(0.068)
FE: Year, County	Y	Y	Y	Y	Y	Y	Y
Observations	8,458	8,162	8,162	8,313	4,616	4,616	8,454

Table IV
County-Level Labor Market Outcomes: ACS Data

All columns report estimates from equation (3) using CSDID estimator. Columns (1) to (4) present results for workers in the non-tradable and construction sectors, columns (5) to (8) present results for workers in tradable sectors, and column (9) covers all industries. *LEFB* refers to low-education foreign-born workers, while *LEUB* refers to low-education US-born workers. *HEFB* refers to high-education foreign-born workers, while *HEUB* refers to high-education US-born workers. *R.Wages* is the county-level mean annual wages of low-education workers relative to that of high-education workers within the same foreign-born or U.S.-born group. *Formal Emp/1K* denotes formal employment per 1,000 county population, where formal employment includes LEUB, HEFB, and HEUB. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. All models include county and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Industry Type:	Non-Tradable & Construction				Tradable				All
Labor Type:	LEFB		LEUB		LEFB		LEUB		All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Outcome:	Ln(Emp)	R.Wages	Ln(Emp)	R.Wages	Ln(Emp)	R.Wages	Ln(Emp)	R.Wages	Formal Emp/1K
SC	-0.103**	0.025	-0.048**	0.063*	-0.076	0.007	0.030	0.001	-35.635**
	(0.043)	(0.135)	(0.021)	(0.033)	(0.046)	(0.040)	(0.030)	(0.011)	(18.195)
FE: Year, County	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,222	1,143	1,334	1,318	1,172	1,100	1,323	1,318	1,334

Table V
Firm-level Outcomes for Small Businesses

All columns report estimates from regression equation (1) using CSDID estimator. Columns (1) to (4) present results for firms in all counties, columns (5) to (8) for firms in metropolitan counties, and columns (9) to (12) for firms in non-metropolitan counties. $Ln(Emp)$ is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Counties:	All				Metropolitan				Non-Metropolitan			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.005***	0.009***	0.006***	0.007***	0.005***	0.008***	0.007***	0.008***	0.003***	0.006***	0.003***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
FE: Year	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	16,871,835	16,871,835	16,702,474	16,702,474	12,936,376	12,936,376	12,834,037	12,834,037	3,935,459	3,935,459	3,868,437	3,868,437

Table VI
Firm-level Effect Heterogeneity by Industry

All columns report estimates from regression equation (1) using CSDID estimator. Columns (1) and (2) report results for firms in non-tradable and construction sectors, while columns (3) and (4) show results for firms in tradable sectors. $Ln(Emp)$ is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Industry Type:	Non-Tradable & Construction		Tradable	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.004***	0.008***	0.002	0.003
	(0.001)	(0.002)	(0.003)	(0.005)
Controls	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
Observations	4,891,939	4,891,939	968,914	968,914

Table VII
Firm-level Effect Heterogeneity by Ex-ante Banking Relationship

All columns report estimates from regression equation (1) using CSDID estimator. Columns (1) and (2) report results for *No Secured Loan* firms, while columns (3) and (4) show results for *Secured Loan* firms. *No Secured Loan* denotes firms without any secured loan between years $t-3$ and $t-1$ relative to the activation of the Secure Communities program. *Secured Loan* denotes firms that obtained a secured bank loan during the same period. This status is fixed for each firm throughout the sample period. $Ln(Emp)$ is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Ex-Ante Bank Relation:	No Secured Loan		Secured Loan	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.008***	0.005**	0.004	0.000
	(0.001)	(0.002)	(0.003)	(0.001)
Controls	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
Observations	11,176,146	11,176,146	726,685	726,685

Table VIII
Firm-level Effect Heterogeneity by Ex-ante Banking Relationship and Industry

All columns report estimates from regression equation (1) using CSDID estimator. Columns (1) to (4) present results for firms in non-tradable and construction sectors, while (5) to (8) cover firms in tradable sectors. *Ex-ante Bank Relation* indicates whether a firm in the respective sample had an ex-ante banking relationship. *No Secured Loan* denotes firms without any secured loan between years $t-3$ and $t-1$ relative to the activation of the Secure Communities program. *Secured Loan* denotes firms that obtained a secured bank loan during the same period. This status is fixed for each firm throughout the sample period. *Ln(Emp)* is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Industry Type:	Non-Tradable & Construction				Tradable			
Ex-Ante Bank Relation:	No Secured Loan		Secured Loan		No Secured Loan		Secured Loan	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.005***	0.006**	-0.001	0.000	-0.001	-0.003	0.003	0.001*
	(0.002)	(0.002)	(0.003)	(0.000)	(0.003)	(0.007)	(0.007)	(0.001)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y	Y	Y	Y	Y
Observations	4,250,402	4,250,402	289,682	289,682	756,673	756,673	96,010	96,010

Table IX
Firm-level Effect Heterogeneity by Ex-ante Bank Monitoring

All columns report estimates from equation (1) using the CSDID estimator for firms with a pre-existing, continuing secured bank loan obtained within three to one years prior to the SC program and below-average employment in year $t-1$. Columns (1) to (4) present results for firms in all sectors, while (5) to (8) cover firms in non-tradable and construction sectors. *Ex-Ante Branch Closure* is coded as *Yes* if any of the firm's lender banks closed branches (net of new openings) in the firm's county after the loan was obtained, and *No* otherwise. This status is fixed for each firm throughout the sample period. *Ln*(*Emp*) is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Sample:	All				Non-Tradable & Construction			
Ex-Ante Branch Closure:	Yes		No		Yes		No	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.018***	0.004***	0.012	0.004	0.023***	0.003**	0.012**	0.004
	(0.006)	(0.001)	(0.008)	(0.003)	(0.007)	(0.001)	(0.006)	(0.003)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y	Y	Y	Y	Y
Observations	32,660	32,660	64,535	64,535	12,573	12,573	26,243	26,243

Employing Undocumented Workers: Immigration Enforcement Impacts on Small Businesses And The Role of Bank Monitoring

Online Appendix

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A. Additional Tables

Table A.1
Firm-level Outcomes for Small Businesses [Including Sales Quartile Fixed Effects]

All columns report estimates after including sales quartile fixed effects in regression equation (1) using CSDID estimator. Columns (1) to (4) present results for firms in all counties, columns (5) to (8) for firms in metropolitan counties, and columns (9) to (12) for firms in non-metropolitan counties. $Ln(Emp)$ is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm, year, and sales quartile fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Counties:	All				Metropolitan				Non-Metropolitan			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.003***	0.008***	0.005***	0.007***	0.003***	0.008***	0.005***	0.007***	0.003***	0.006***	0.002**	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Controls	N	N	Y	Y	N	N	Y	Y	N	N	Y	Y
FE: Year	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
FE: Sales Quartile	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	12,936,160	12,936,160	12,833,824	12,833,824	12,936,160	12,936,160	12,833,824	12,833,824	3,935,221	3,935,221	3,868,203	3,868,203

Table A.2
Firm-level Effect Heterogeneity by Industry [Including Sales Quartile Fixed Effects]

All columns report estimates after including sales quartile fixed effects in regression equation (1) using CSDID estimator. Columns (1) and (2) report results for firms in non-tradable and construction sectors, while columns (3) and (4) show results for firms in tradable sectors. $Ln(Emp)$ is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm, year, and sales quartile fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Industry Type:	Non-Tradable & Construction		Tradable	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.004***	0.008***	0.000	0.001
	(0.001)	(0.002)	(0.002)	(0.005)
Controls	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
FE: Sales Quartile	Y	Y	Y	Y
Observations	4,891,889	4,891,889	968,908	968,908

Table A.3
Firm-level Effect Heterogeneity by Ex-ante Banking Relationship [Including Sales Quartile Fixed Effects]

All columns report estimates after including sales quartile fixed effects in regression equation (1) using CSDID estimator. Columns (1) and (2) report results for *No Secured Loan* firms, while columns (3) and (4) show results for *Secured Loan* firms. *No Secured Loan* denotes firms without any secured loan between years $t-3$ and $t-1$ relative to the activation of the Secure Communities program. *Secured Loan* denotes firms that obtained a secured bank loan during the same period. This status is fixed for each firm throughout the sample period. $Ln(Emp)$ is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm, year, and sales quartile fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Ex-Ante Bank Relation:	No Secured Loan		Secured Loan	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.006***	0.005**	0.006**	0.000
	(0.001)	(0.002)	(0.003)	(0.001)
Controls	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
FE: Sales Quartile	Y	Y	Y	Y
Observations	11,175,965	11,175,965	726,670	726,670

Table A.4
Firm-level Effect Heterogeneity by Ex-ante Banking Relationship and Industry [Including Sales Quartile Fixed Effects]

All columns report estimates after including sales quartile fixed effects in regression equation (1) using CSDID estimator. Columns (1) to (4) present results for firms in non-tradable and construction sectors, while (5) to (8) cover firms in tradable sectors. *Ex-Ante Bank Relation* indicates whether a firm in the respective sample had an ex-ante banking relationship. *No Secured Loan* denotes firms without any secured loan between years $t-3$ and $t-1$ relative to the activation of the Secure Communities program. *Secured Loan* denotes firms that obtained a secured bank loan during the same period. This status is fixed for each firm throughout the sample period. $Ln(Emp)$ is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm, year, and sales quartile fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Industry Type:	Non-Tradable & Construction				Tradable			
Ex-Ante Bank Relation:	No Secured Loan		Secured Loan		No Secured Loan		Secured Loan	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.005***	0.006**	0.000	0.001	-0.002	-0.003	0.000	0.001*
	(0.001)	(0.002)	(0.003)	(0.000)	(0.003)	(0.007)	(0.009)	(0.001)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y	Y	Y	Y	Y
FE: Sales Quartile	Y	Y	Y	Y	Y	Y	Y	Y
Observations	4,250,356	4,250,356	289,678	289,678	756,671	756,671	96,010	96,010

Table A.5
Firm-level Effect Heterogeneity by Ex-ante Bank Monitoring [Including Sales Quartile Fixed Effects]

All columns report estimates after including sales quartile fixed effects in regression equation (1) using CSDID estimator for firms with a pre-existing, continuing secured bank loan obtained within three to one years prior to the SC program and below-average employment in year $t-1$. Columns (1) to (4) present results for firms in all sectors, while (5) to (8) cover firms in non-tradable and construction sectors. *Ex-Ante Branch Closure* is coded as *Yes* if any of the firm's lender banks closed branches (net of new openings) in the firm's county after the loan was obtained, and *No* otherwise. This status is fixed for each firm throughout the sample period. $Ln(Emp)$ is the natural logarithm of firm's employment. *Exit* is an indicator taking the value of one if the firm exits in year $t+1$, and zero otherwise. *SC* is an indicator taking the value of one for counties that have implemented the Secure Communities program, and zero otherwise. Controls are the three variables listed and defined in Table I. All models include firm, year, and sales quartile fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

Sample:	All				Non-Tradable & Construction			
Ex-Ante Branch Closure:	Yes		No		Yes		No	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.018***	0.004***	0.012*	0.004	0.072***	0.003**	0.013**	0.005
	(0.006)	(0.001)	(0.007)	(0.003)	(0.014)	(0.001)	(0.006)	(0.003)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y	Y	Y	Y	Y
Observations	32,660	32,660	64,535	64,535	12,573	12,573	26,243	26,243