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

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Abstract

The staggered rollout of the Secure Communities program, a shock to undocumented labor, raised per-firm wages and (formal) employment, alongside firm closures in sectors more reliant on undocumented workers, as well as the overall unemployment rate. These reflect firms substituting undocumented workers with costlier formal labor, rendering marginal firms unviable and raising overall unemployment. Small business–level data show that unbanked firms exhibit stronger increases in employment and closures. Among banked firms, effects are stronger for those facing weaker ex-ante local bank monitoring. Overall, these patterns imply a deputization-like effect of banking relationships in deterring borrowers from employing undocumented workers.

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Undocumented immigrants, or unauthorized immigrants, are individuals residing in the U.S. without legal status. In 2008, their population was estimated at 12 million, with approximately 8.2 million employed, constituting 5.4% of the national labor force (Pew Research (2025), Passel and D'Vera Cohn (2009)). 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 (ICE (2014)).

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 (SBA (2021)). 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 Callaway and Sant'Anna (2021) 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 establish-

[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 (n.d.)).

ments, average wages, average employment) or general economic characteristics (per capita income, international migration). County population and Hispanic share were associated with earlier SC activation, in line with Cox and Miles (2013) and Miles and Cox (2014). 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 (Baker et al. (forthcoming), Sec. 4.1). 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] These patterns suggest that, because the SC program targeted undocumented workers, establishments that previously relied on them responded to the reduced labor supply and heightened risk of detection by substituting toward costlier formal workers. Given that official statistics such as QCEW are unlikely to capture undocumented workers (Bohn and Santillano (2017)), this substitution would increase measured employment. Moreover, some establishments

[2] Our industry classification comes from Mian and Sufi (2014). 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.

likely became unviable under higher detection risk and reduced reliance on low-cost undocumented labor, leading to increased firm closures. Similar patterns of rising wages and closures have been documented in response to adverse local labor supply shocks arising from environmental contamination (Huang and Kumar (2025)).

Moreover, firms in tradable sectors, where undocumented workers are much less likely to be employed, did not experience changes in establishments, wages, or employment. Similarly, the effects were significant only in counties that contain metropolitan areas, where nearly 94% of undocumented immigrants are estimated to reside (Passel and D'Vera Cohn (2009)), but not in non-metropolitan counties. These differential effects point to the role of undocumented labor in driving the results. Moreover, sectors and regions less reliant on undocumented workers serve as placebos. The absence of effects in these groups alleviates concerns that contemporaneous policies or broader economic shocks drive the results.

We also find that the unemployment rate increased by 0.11 percentage points across all counties, with the increase nearly twice as large in metropolitan counties and no significant effect in non-metropolitan counties. These patterns point to an imperfect substitution between undocumented and formal labor, implying that shocks to undocumented labor are unlikely to improve employment prospects for formal workers and may, in fact, worsen them. Using individual responses in the ACS, East et al. (2023) similarly report a decline in employment among U.S.-born workers following the SC program. We document declines in employment in official nationwide statistics, thereby generalizing their ACS-based evidence, which covers fewer than one-third of counties, and show that these declines are driven in part by a firm-closure channel.

Similar employment and closure 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.

In the remainder of the analysis, we examine 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 (FDIC (2024)), and their lending decisions rely heavily on screening borrowers through the collection of firm-specific subjective intelligence, or "soft information" (Agarwal and Hauswald (2010)), which is facilitated by personal interactions (Petersen and Rajan (2002)) and ex-post monitoring (Granja, Leuz, and Rajan (2022), Heitz, Martin, and Ufier (2022)). 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.

[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/).

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 (Gopal and Schnabl (2022)).[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 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

[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.

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 (Agarwal and Hauswald (2010), Berger et al. (2005), Granja, Leuz, and Rajan (2022), Petersen and Rajan (2002), Canales and Nanda (2012)). 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, Cornaggia, Cornaggia, and Israelsen (2024) show that municipal bond yields decline in tighter labor markets and rise otherwise, and Li et al. (2025) find that both stock and municipal bond markets react negatively around ICE enforcement actions due to reduced labor supply. Focusing on labor outcomes, East et al. (2023) show that the SC program reduced employment for likely-undocumented workers, and Howard, Wang, and Zhang (2024) link the program to reduced construction of residential homes. In contrast, by studying small businesses and showing that they employ undocumented workers—and that 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 (Benmelech, Bergman, and Seru (2021), Chodorow-Reich (2014), Fonseca and Van Doornik (2022)). In contrast, we document an increase in employment among both ex-ante unbanked firms and likely less-monitored firms.

[5] See for example, Dai et al. (2025), Aobdia, Srivastava, and Wang (2018), Aobdia, Carnes, and Munch (2024), Gupta (2025), and Cookson, Guttman-Kenney, and Mullins (2025).

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" (Miles and Cox (2014)). 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 (Baumer and Xie (2023)). 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, Cox and Miles (2013) 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 Callaway and Sant'Anna (2021)'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 (De Chaisemartin and d'Haultfoeuille (2020), Goodman-Bacon (2021), Borusyak, Jaravel, and Spiess (2024)). 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

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

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 Cox and Miles (2013) and Miles and Cox (2014). 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 (East et al. (2023)).

[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 ICE (2014), 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 Chava, Oettl, and Singh (2023), 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 Gopal and Schnabl (2022), we identify the lender type, including whether the lender is a bank.

Data on bank branch closures are obtained from the Federal Deposit Insurance Corporation (FDIC) Summary of Deposits (SOD) dataset. The SOD is an annual survey that provides detailed information on branch locations, deposits, and bank characteristics for all FDIC-insured institutions. We use the unique branch identifier, UNINUMBR, to track branches and their locations, even when ownership changes due to mergers or acquisitions. Branch closure is defined as the termination of the UNINUMBR series. We merge small businesses with branch closure information using UCC filings' standardized lender names available from Gopal and Schnabl (2022).

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 (Passel and D'Vera Cohn (2009), Table B1), 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. To ensure credible interpretations, we structure the analysis in two stages. First, we use reliable and authoritative QCEW data produced by BLS to document effects of the SC program on labor and firm outcomes aggregated at the county level. Next, we turn to the firm-level NETS dataset, which, while subject to imputation concerns, is widely used in small business literature.[7] We replicate key aggregate patterns from the public data in the NETS dataset to validate its reliability and coverage, and then use its detailed firm-

[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.

[7] See for example, Barrot and Nanda (2020), Denes et al. (2023), Addoum, Ng, and Ortiz-Bobea (2020), Addoum, Ng, and Ortiz-Bobea (2023), Chava, Oettl, and Singh (2023), and Acharya, Bhardwaj, and Tomunen (2023).

level data for deeper insights. Finally, we alleviate its 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 Mian and Sufi (2014), 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)). Finally, the unemployment rate increases by 0.112 percentage points following the SC program.

[Insert Table III About Here]

[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.

A notable feature of the undocumented population is that nearly 94% reside in metropolitan areas (Passel and D'Vera Cohn (2009)). 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)).

Overall, the absence of employment, wage, and closure effects for firms in tradable sectors and in non-metropolitan counties point to the role of the undocumented labor channel, as these firms, while similarly and simultaneously exposed to the SC program, are much less reliant on undocumented workers. Moreover, these differential effects make it unlikely that contemporaneous policy changes or broader economic shocks explain the results.

Economically, the 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%.

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]

To interpret the outcomes above, we rely on two key premises. First, official data collected by the Census Bureau in QCEW and unemployment statistics do not capture undocumented workers, as such workers and their employers are unlikely to participate or report accurately in official surveys (Bohn and Santillano (2017)). 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 increase in employment and wages alongside firm closures is consistent with substitution from undocumented to formal labor. This substitution, likely imperfect, raises measured employment and per-establishment wages due to the higher cost of formal workers. The decline in establishments follows from higher labor costs rendering marginal firms unviable. Finally, despite rising employment per firm, the unemployment rate increases, likely because job losses from exiting firms, which entail the loss of their entire workforce, outweigh the substitution-induced increase in formal employment at surviving firms.

A limitation of the interpretations above is that they make inferences about adjustments in wages and employment of undocumented and documented groups separately, while relying only on equilibrium outcomes aggregated across the two

[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.

groups. We strengthen these interpretations by providing group-specific evidence on the effects of the SC program using ACS data. Following the literature (East et al. (2023), for example), we proxy undocumented workers using low-education foreign-born (LEFB) individuals and formal workers using low-education U.S.-born (LEUB) individuals. We construct county-level employment and relative wages for individuals employed in non-tradable and tradable industries for both groups. Relative wages are defined as the ratio of a group's mean annual wage (LEFB or LEUB) to that of the corresponding high-education group.

[Insert Table IV About Here]

Table IV presents the results. In non-tradable sectors, employment of likely-undocumented workers declines by about 10 percent in treated counties relative to controls (column (1)), roughly twice the decline observed for low-education U.S.-born workers (column (2)). At the same time, wages for LEFB workers remain unchanged (column (2)), while wages for LEUB workers increase by 6.3 percent (column (4)). Aggregating these dynamics implies a shift toward relatively higher employment of formal workers at higher wages, consistent with the equilibrium county-level patterns documented in Table III, which we argued to arise from firm's substitution and exit. Further consistent with earlier results, we find no changes in employment or wages in tradable sectors (columns (5) to (8)), alongside rising overall unemployment evident in a reduction of about 36 jobs per 1,000 population in column (9).

To summarize, we document that enactment of the SC program leads to rising per-firm employment and wages, firm closures, and an increase in the overall unemployment rate. For the remainder of the analysis, we refer to the concurrent rise in (formal) employment and firm closures as the *characteristic pattern*, which signals firms' reliance on undocumented workers before the SC program. An important policy implication of the increase in unemployment is that undocumented and formal labor are not perfect substitutes. Therefore, policies aimed at reducing undocumented labor may not improve, and may even worsen, employment prospects for formal workers. East et al. (2023) reach a similar conclusion by documenting a decline in the employment share of U.S.-born workers in ACS data following the SC program. We complement their findings by documenting analogous declines in nationwide official unemployment statistics and attributing them to a firm-closure channel.

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

[10] Minor differences in the pre-treatment period, though not ideal, are not uncommon. For example, pre-trends in Lovenheim and Willén (2019) also run opposite to the treatment effect, as noted in (Rambachan and Roth (2023), footnote 10).

SC program are consistent with firms' ex-ante reliance on undocumented workers. Moreover, because undocumented workers tend to reside in MSA counties (Passel and D'Vera Cohn (2009)), 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.

[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 (Agarwal and Hauswald (2010), Petersen and Rajan (2002), Heitz, Martin, and Ufier (2022)), 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]

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

[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.

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 (Heitz, Martin, and Ufier (2022)). 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.[13] Accordingly, we focus on firms with active banking relationships to assess the deterrent effect of monitoring on the employment of undocumented workers. This analysis is challenging because monitoring intensity and credit availability are jointly determined and may simultaneously affect firm employment and closure. However, the two mechanisms imply distinct outcome patterns. The credit availability channel primarily affects firms that relied on undocumented workers prior to the SC program by enabling them to finance costlier formal labor, thereby increasing employment and reducing closures. In contrast, monitoring channel operates through deterrence. Firms subject to more intensive ex-ante monitoring are less reliant on undocumented workers and thus less affected by the SC program, whereas those facing weaker monitoring are more reliant and experience larger increases in both employment and closures. We exploit this distinction to test for the deterrent effect of bank monitoring.

Specifically, we examine changes in employment and firm closures among firms that maintained access to secured bank credit but differed in their lenders' monitoring capacity 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 SC program with those whose lenders did not. Because both groups retained credit access until treatment, differences in outcomes capture variation in ex-ante monitoring driven by local branch closures, holding credit availability relatively constant. Measuring closures prior to treatment ensures these decisions are independent of the program's rollout. We therefore test whether firms exposed to ex-ante branch closures exhibit more pronounced changes in employment and exit than those that are not.

[Insert Table IX About Here]

[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.

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)). A notable 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 with other banking datasets. 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 Callaway and Sant'Anna (2021)'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, and overall unemployment rate increased. 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. Importantly, the increase in unemployment rate implies that employment loses from closure of firms exceed gains at surviving firms, with the implication that policies aimed at reducing undocumented workers may not improve employment prospects of formal workers. Finally, these effects were 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.

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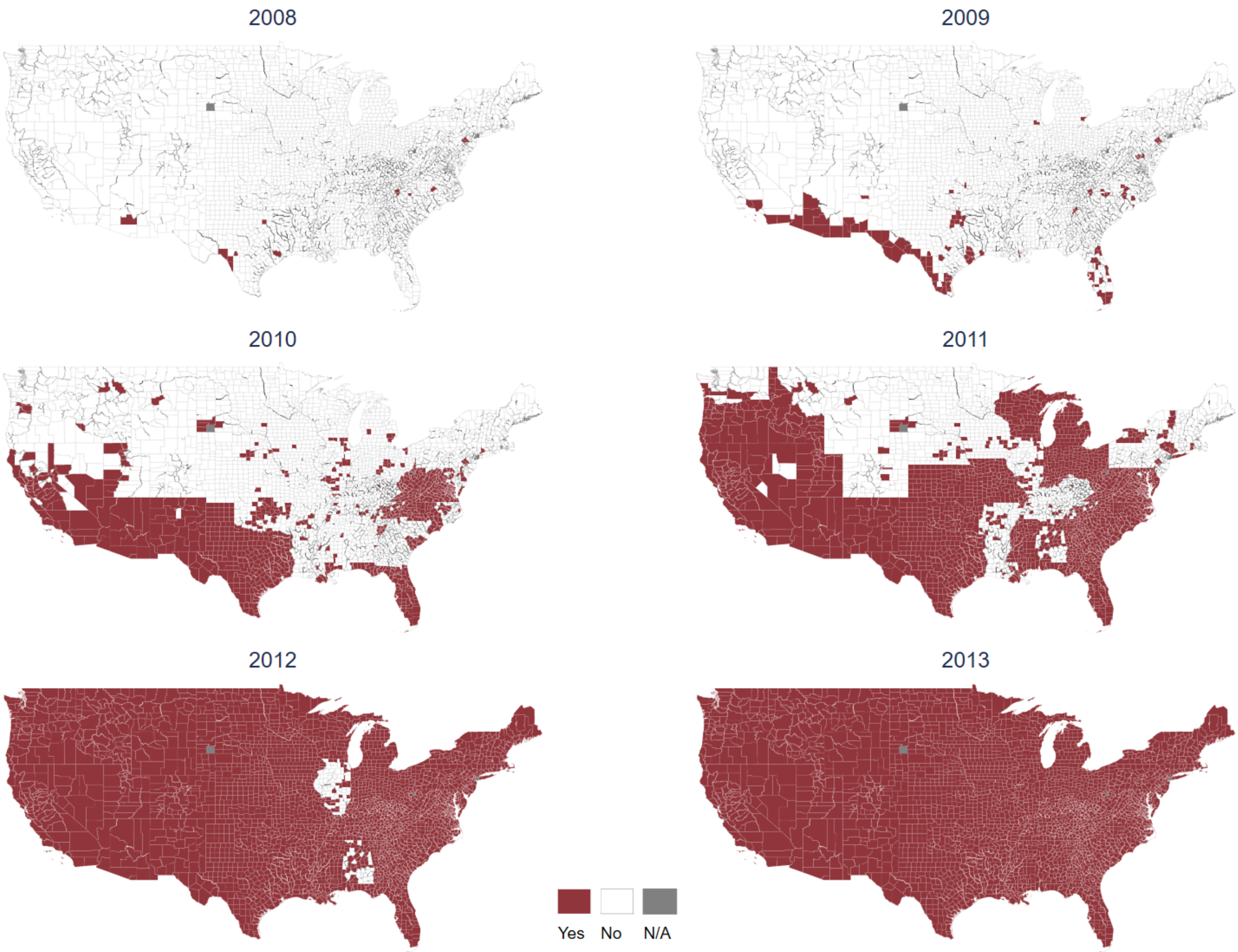

Figure 1. Staggered Rollout of Secure Communities

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

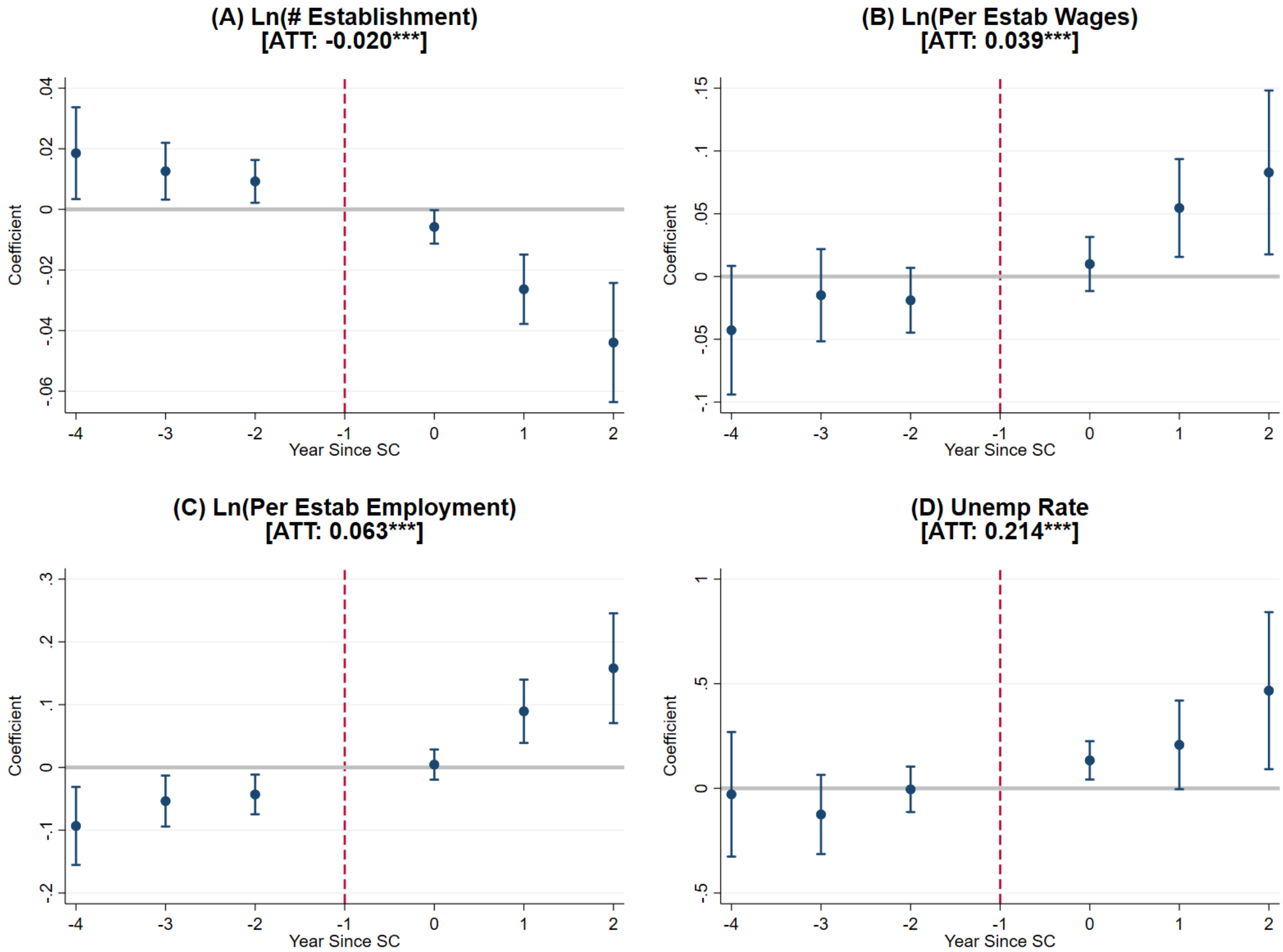

Figure 2. Event-study Plots: County-Level Industry Outcomes

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 Callaway and Sant'Anna (2021) 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.

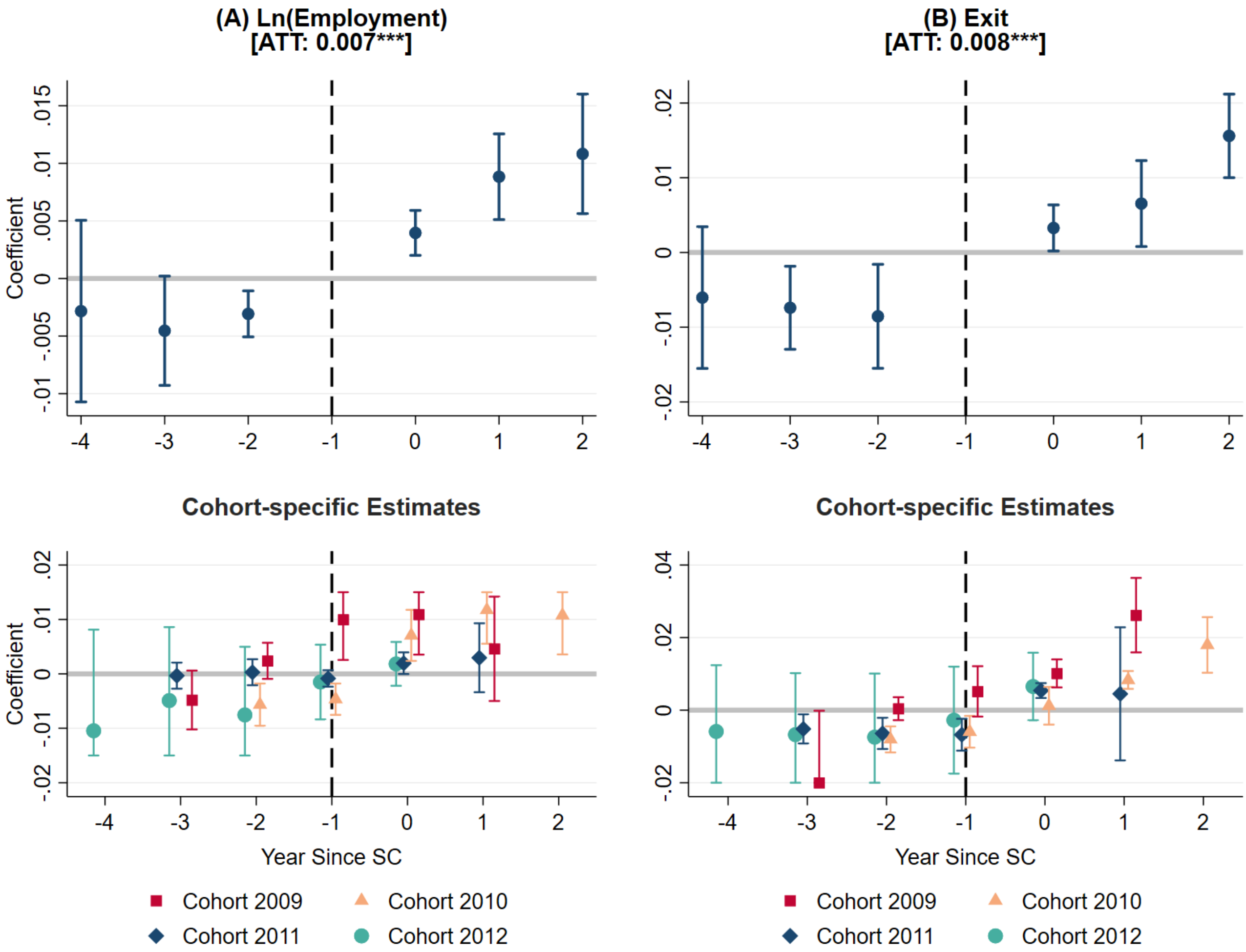

Figure 3. Event-Study Plots: Firm-level Outcomes for Small Businesses

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 Callaway and Sant'Anna (2021) 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 (1)	Mean (2)	P25 (3)	Median (4)	P75 (5)	SD (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 $t-1$. *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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Ex-Ante Branch Closure:	Yes		No	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.007**	0.003*	-0.003	0.001
	(0.003)	(0.002)	(0.006)	(0.001)
Controls	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
Observations	76,188	76,188	237,740	237,740

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