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 the level in 1990. About 8.2 million of them, constituting roughly 5.4% of the national labor force, were understood to be employed then (Pew Research (2025), Passel and D'Vera Cohn (2009)). While it is widely perceived that undocumented workers take up low-skilled jobs and may even substitute for formal workers by accepting lower wages, systematic evidence on their employment remains limited. This paper examines their employment by small businesses—a context where such hiring is both more feasible and less detectable. Furthermore, because banks are the key formal institution on whom small businesses depend for credit, and because they have strong incentives to screen and monitor risky borrowers leveraging their consistent local presence and expertise in collecting soft information, we examine whether banking relationships influence small businesses' employment of undocumented labor.

The main challenge in studying the employment of undocumented workers is its inherent unobservability. We address this by combining quasi-exogenous shocks to the supply of undocumented workers with the resulting changes in the employment of formal workers to infer the usage of undocumented workers. Specifically, we exploit the federal Secure Communities (SC) program as a negative undocumented labor shock. 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 rollout 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, in addition to their substantial contribution to the U.S. economy.[1] First, employing undocumented workers is more feasible for small businesses than for larger or publicly listed firms, as their limited scale and minimal formal reporting or auditing reduce the likelihood of detection. Market-based disciplining mechanisms, such as stock market feedback, active investor monitoring, and reputational concerns, are largely absent for small firms, further lowering this risk. Finally, statutory penalties exclude isolated or inadvertent violations—a defense more plausible for small businesses, over 75% of which employ only about six workers, than for large corporations.[2]

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 over stacking is dictated by feasibility considerations 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 general economic characteristics (per capita income, internal migration) or small business–related characteristics (growth rates of establishments,

[1] Small businesses accounted for nearly 43% of national gross domestic product and 46% of private-sector employment in 2021 (SBA (2021)).

[2] 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.)).

average wages, average employment). Overall county population and Hispanic share were associated with earlier SC activation, a pattern also noted by Miles and Cox (2014). This 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 remain robust when controlling for these population variables.

We begin our empirical analysis by examining the effects of the SC program on county-level firm outcomes using the Census Bureau's Quarterly Census of Employment and Wages (QCEW) data. We then replicate these findings using firm-level, industry-produced National Establishment Time Series (NETS) dataset and further leverage the latter to uncover additional insights. This two-step strategy offers two key benefits. First, it enhances the generalizability and credibility of our interpretations regarding the employment of undocumented labor. 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 program led to a statistically significant decline in the number of establishments but an increase in average wages and employment among firms in the non-tradable and construction sectors of treated counties relative to control counties.[3] In contrast, firms in tradable sectors did not experience similar changes. Two key insights emerge from these patterns. First, since the SC program targeted undocumented workers, establishments previously relying on them likely responded to the reduced supply of such labor combined with the heightened risk of them getting detected by substituting them with costlier formal workers. Since official statistics such as QCEW are unlikely to capture undocumented workers (Bohn and Santillano (2017)), such substitution would increase measured employment.

[3] For brevity, we refer to the non-tradable and construction sectors interchangeably as non-tradable.

Moreover, some establishments became unviable with heightened detection risk and reduced reliance on low-cost undocumented workers, leading to an increase in firm closures. Similar wage and closure patterns have been documented to result from adverse local labor supply shocks in the context of environmental contamination (Huang and Kumar (2021)). Second, the fact that the effects were significant in non-tradable and construction sectors—where undocumented workers are commonly perceived to be employed (Pew Research (2025))—but not in tradable sector, further ties the effects to undocumented workers.

The same patterns hold when we employ the CSDID estimator with firm fixed effects on 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 0.5% and faced a 0.7% higher probability of closure. To mitigate concerns about bad controls that arise from including co-variates that themselves may respond to the treatment, we exclude firm-level co-variates in these specifications. At the same time, we include county-level co-variates to improve precision, while leveraging CSDID's approach of utilizing only the pre-treatment levels of co-variates to avoid bad control concerns.

We devote the remainder of the empirical analysis to examining the role of banks in shaping small businesses' employment of undocumented workers. Our focus on banks is motivated by two 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)), 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. Another 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.[4] Second, banks have strong economic incentives to avoid lending to firms that employ undocumented workers, given the heightened legal and financial liabilities such borrowers may pose. Finally, banks may also refrain from 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 (Gopal and Schnabl (2022)).[5]

We find evidence consistent with bank screening deterring small business borrowers from employing undocumented workers. Following the activation of SC, firms in non-tradable sectors in treated counties without ex-ante lending relationships increased employment by 0.4% and faced 0.3% higher likelihood of closure relative to same-sector unbanked firms operating in control counties. In contrast, non-tradable sectors firms with pre-existing lending relationships exhibited no such changes. As discussed earlier, the simultaneous rise in employment and firm closures suggests substitution of undocumented workers with costlier documented ones. The fact that

[4] 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/).

[5] 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.

this pattern is observed only among unbanked firms, but not among those with prior banking relationships, indicates that bank screening may discourage borrower firms from relying on undocumented labor.

The finding that bank screening may exert a deputization effect on small businesses is, to the best of our knowledge, new. One may question our interpretation by arguing that all that went on is that banked firms were simply less likely to close after the SC program because they had access to credit. However, this explanation fails to account for why unbanked firms undertook the costly decision to hire more employees despite not having access to a banking relationship. Further reinforcing our interpretation, we find no rise in employment or closures for firms in treated counties relative to control operating in the tradable sector, where undocumented workers are known to be much less prevalent.

We further probe the deputization effect of banking relationships by focusing on the role of bank monitoring, as distinct from screening. Specifically, we examine whether the characteristic patterns of substitution for undocumented workers were stronger among firms with weaker ex-ante monitoring. We proxy reduced monitoring using ex-ante local branch closures, 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-median employment, those whose lenders reduced net branch presence in firms' county within three years prior to SC showed significantly larger increases in both employment and exits following the treatment relative to such firms in control counties. In contrast, no significant treatment effects emerged for firms whose lenders did not close a branch. This implies that firms subject to lower ex-ante monitoring exhibited stronger patterns of undocumented labor usage. We also note that this cross-sectional difference in response cannot be attributed to credit availability, as both groups had 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 uncovering 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.[6] 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.

[6] 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).

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.

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

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

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, the rollout timing of the Program is predicted by several economic and demographic factors. However, the growth in local employment, closures, and loans to small businesses do not appear to predict the Program's implementation.

[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 Reserve System's publicly available National Information Center (NIC) database. NIC contains information on bank branch information, ownership structures, and regulatory activity. We merge this data with the small business dataset at the county level.

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.

[Insert Table I About Here]

Table I shows the summary statistics for the key variables. 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 concerns, it has been proved useful in several small business research.[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

We draw on the QCEW data to construct county-aggregated total number of establishments, average wages per establishment, and average employment per establishments. We merge these outcomes with the industry classification into tradable and non-tradable from Mian and Sufi (2014).[8] We employ the CSDID estimator without

[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).

[8] As mentioned earlier, for brevity, we group the non-tradable and construction industries together as "non-tradable", and the tradable and others as "tradable."

covariates to examine changes in the three outcome variables separately for the two industry groups. Panels A to C of Figure 2 present the corresponding event study plots for the three outcomes, with each panel showing the plots for the two industry groups separately.[9] The plots suggest that the non-tradable industries in the counties that adopted SC experience a decline in the number of establishments (Panel A) along with an increase in average wages and average employment per establishment (Panel B and C respectively). The tradable industry, however, did not experience statistically significant changes for any of the outcomes. These plots also suggest that the treated and control counties were not experiencing diverging trends prior to the treatment and alleviate the concern that the parallel trends assumption might not hold. Panel D shows the effect of the program on unemployment rate. While there was a slight increase in unemployment rate in three years after the program, the average effect is statistically not significant.

[Insert Figure 2 About Here]

To aid interpretation of the magnitudes of the treatment effect, we present the average treatment effect across all treatment cohorts and years (ATT) separately for the three outcomes and two industries in Table (III). The specification is county-level equivalent of the specification in equation (1).[10] The estimates in columns (1) to (3) indicate that relative to the counties without SC program, those with the program experience a 1.7% decline in the number of establishments, a 4% increase in average wages paid by establishments, and 5.8% increase in average employment by establishments in the non-tradable sector. At the same time, tradable sector firms in treated counties did not see any significant change relative to those in control counties, as indicated by the estimates in columns (4) to (6). Finally, the estimate in column (7)

[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.

[10] The regression specification for county-level outcomes is: $\mathrm{Y}_{ict} = \alpha_0 + \beta \times \mathrm{SC}_{ict} + \mathrm{Controls}_{ct} + \delta_c + \gamma_t + \epsilon_{ict}$.

indicates that unemployment rates, which can be assumed to represent formal workers, did not change in response to the SC enactment.

[Insert Table III About Here]

The interpretation of these patterns rests on two key premises. First, the official QCEW data collected by the Census Bureau do not capture undocumented workers, as such workers and their employers are unlikely to participate or report truthfully in official surveys (Bohn and Santillano (2017)). Second, the SC program constitutes 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.

Given these premises, the simultaneous rise in firm closures, employment, and wages following the SC program suggests that firms previously employing undocumented workers reduced or eliminated such positions and substituted them with formal employees. This substitution—likely imperfect—accounts for both the rise in reported employment and the increase in wages, given the higher cost of formal labor. Moreover, the accompanying decline in the number of establishments likely results from higher wage bills rendering some firms financially unviable and leading to their closure.

Two additional observations support the above interpretation. First, we do not find statistically significant effects for tradable-sector firms in the same counties, which are also exposed to the SC program, presumably because they are less likely to employ undocumented workers. Second, a decline in local consumption is inconsistent with our findings of rising employment in non-tradable sectors. One might argue that the program prompted out-migration of undocumented workers, reducing local consumption and leading to closures in non-tradable sectors. However, this mechanism would imply lower employment, which contradicts our observed increase.

We henceforth 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. Economically, these patterns are important yet nuanced. The 1.7% decline in non-tradable sector firms translates into the closure of approximately 35,000 establishments nationwide, leading to potential losses in production, service provision, and tax revenues from profitable firms. The impact on formal employment, however, depends on two opposing forces: firm closures likely reduce both formal and undocumented jobs, while the surviving firms' substitution of undocumented with formal workers increases formal employment. Our finding in column (7) that the overall unemployment rate did not decline, or equivalently, that employment prospects of formal workers did not improve following negative shocks to undocumented workers, implies that formal workers are not perfect substitutes for undocumented labor. This aligns with the conclusions of East et al. (2023) and Howard, Wang, and Zhang (2024).

B. Firm-Level Evidence

Having established the aggregate patterns at the county level, we next examine whether similar characteristic patterns indicative of undocumented worker employment are observable in firm-level outcomes constructed from NETS data. This also lends credibility to our subsequent analysis of the role of bank screening and monitoring, which can only be assessed at the firm level. Since wage data are unavailable in this dataset, we assess the changes in characteristic patterns using the natural logarithm of employment and an indicator for permanent firm closure (*exit*). We focus on the natural logarithm of employment and an indicator for permanent firm closure (*exit*). Focusing on firm closure measure is particularly useful because these are well-measured and not subject to the imputation issues that may affect employment measures. We use the CSDID estimator and the specification in equation (1) for these outcomes. This specification allows for firm and year fixed effects, rendering the estimates robust to time-invariant differences not only across firms but also across counties, such as their proximity to the Southern border. Standard errors are clustered at the county level, the level of treatment assignment.

[Insert Table IV About Here]

Table IV presents the results. Columns (1) and (2) show the estimates without any controls, indicating that the activation of the SC program leads to a 0.5% increase in firm-level employment and a 0.8% increase in the probability of permanent firm closure. In terms of magnitude, the closure rate represents an 11.4% increase relative to the mean pre-event closure rate. Adding three controls for county characteristics—the natural logarithm of per capita income, population, and Hispanic population—in columns (3) and (4) produces essentially identical estimates.

[Insert Figure 3 About Here]

Figure 3 presents the dynamic effects for these variables. Panels A and B are consistent with the above results, showing increases in both employment and firm exits following the policy. Two of the three pre-treatment periods exhibit a slightly downward-sloping pre-trend. The reason for this becomes apparent from the cohort-specific dynamic treatment effects plotted below the respective panels. These plots suggest that the negative pre-trend is due to the contribution from the two earliest cohorts who received treatment in 2009 and 2010. Recall from Figure 1 that these cohorts were located near the Southern border of the country, where large populations of undocumented workers are understood to be present. Consequently, firms in these counties may have employed more undocumented workers and fewer formal workers in the pre-treatment period, resulting in lower measured formal employment and a lower likelihood of closure in that period due to comparatively lower wage expenses. This also explains why the latter two cohorts do not experience diverging pre-trends.

Two further observations mitigate concerns about the downward pre-trend. First, the deviations of the pre-treatment coefficients from zero are small relative to the magnitude of the treatment effect. Second, the pre-trends run in the opposite direction of the treatment effect.[11]

Overall, our results indicate that the patterns of employment and firm closures observed in both county- and firm-level panels following the activation of the SC program are consistent with firms' prior reliance on undocumented workers.

C. Banks 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 4 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

[11] Similar minor deviations in the pre-treatment period are not uncommon. For example, pre-trends in Lovenheim and Willén (2019) also run opposite to the treatment effect, a fact noted in (Rambachan and Roth (2023), footnote 10).

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.[12]

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

[12] 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.

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.[13] 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 V About Here]

Table V presents the results. Columns (1) and (2) show that among ex-ante unbanked firms, employment and exit rates increased by 0.7% and 0.4%, 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 VI About Here]

Table VI presents the results. In columns (1) and (2), we observe that unbanked firms in non-tradable industries in treated counties exhibit the characteristic rise in

[13] 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.

employment and closures by 0.4% and 0.3% respectively compared to unbanked firms in control counties. Columns (3) and (4) suggest an absence of these patterns among ex-ante banked firms in non-tradable industries. In contrast, 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 those in control counties. Naturally, in this sector, the differences between ex-ante banked and unbanked firms in treated counties relative to their counterparts in control counties are negligible (that is, columns (5) versus (7) and (6) versus (8)). These results also replicate the county-level industry differences in the effect of SC program we documented in Table (III) using the Census Bureau's QCEW data, further alleviating the coverage concerns of the NETS data.

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

D. Banks 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)). Since screening is unlikely to be perfect, some borrowers may still employ undocumented workers. Consequently, bank monitoring may deter such employment, potentially by using credit availability as a disciplining mechanism. This effect is particularly plausible for smaller firms, which typically undergo less stringent and less elaborate screening than larger firms and are more dependent on bank credit.[14] Accordingly, we focus on firms with active banking relationships and below-median employment to assess the deterrent effect of monitoring on borrowers' employment of undocumented workers. This analysis is inherently challenging

[14] 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.

because both monitoring intensity and credit availability are likely to be jointly determined. However, employment and closure outcomes are affected differently by the two mechanisms. The credit availability mechanism would reduce closures among firms that previously relied on undocumented workers by aiding them finance hiring formal workers. In contrast, the deterrence effect would imply a concurrent rise in both employment and closures among such firms, with stronger effects among those subject to less intense ex-ante monitoring. We therefore rely on this distinction to test for the deterrence effect of bank monitoring.

Specifically, we examine changes in employment and firm closures among businesses that maintained access to secured credit from banks 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 within three to one year prior to the activation of the SC program with those whose lenders did not. Because both groups retain access to credit until the treatment, differences in their responses capture the effect of reduced ex-ante monitoring rather than differences in credit availability. Measuring branch closures prior to the treatment also ensures that those differences are independent of the SC program's activation in the county.

[Insert Table VII About Here]

We therefore test whether, among smaller firms employing below-median 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. Table VII reports the 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 the SC implementation (columns (1) and (2)) than those whose lenders did not. The effect was about twice as large for employment (4% versus 2.9%) and four times as large for exit (0.4% versus -0.1%). Overall, these findings are consistent with the view that bank monitoring plays a deterrent role in firms' employment of undocumented workers.

E. Robustness

E.1. Time-varying Covariates

Our results in Tables V to VII include time-varying covariates. Such specifications may suffer from the bad control problem if the covariates themselves respond to the treatment (Caetano et al. (2022)), and their causal interpretation requires the stronger conditional parallel trends assumption (Baker et al. (forthcoming)). Although the CSDID estimator mitigates this issue by using the levels of covariates only in the pre-treatment year, we further address potential concerns by re-estimating the models without covariates. The estimates presented in Tables A.1 to A.3 in the Online Appendix remain essentially unchanged.

E.2. Can Decline in Consumption Explain the Characteristic Patterns?

A potential concern with our results is that firm closures may have increased after the SC program due to reduced consumption following the out-migration of undocumented populations from treated counties. Several pieces of evidence suggest that this is not the main mechanism. First, our specifications control for county GDP, which captures local consumption levels. Second, one of our key findings is the increase in formal employment—an adjustment firms are unlikely to undertake if overall demand were declining. Third, we re-estimate our results while accounting for firm-level sales. Because sales data in NETS suffer from imputation issues, we address this by computing annual sales quartiles and including them as fixed effects. Tables A.4 to A.7 in the Online Appendix replicate Tables IV to VII, and the conclusions remain unchanged.

IV. Conclusion

This paper provides the first systematic analysis of undocumented worker employment by small businesses. Our central finding is that small businesses in non-tradable sectors are more likely to employ undocumented workers, and that bank screening and ongoing monitoring exert a deputization-like effect, deterring such employment. By highlighting the role of formal financial institutions, our study provides novel evidence on how banks can influence informal labor practices beyond their traditional credit allocation role.

A key novelty of this paper lies in addressing the inherent unobservability of undocumented worker employment. We combine quasi-exogenous shocks to undocumented labor resulting from the Secured Communities program with detailed data on formal worker employment to uncover patterns of undocumented labor utilization. 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. We find that both formal employment and firm closures increased at the county and firm levels. These characteristic patterns are consistent with firms previously relying on undocumented workers, whom they subsequently replaced with formal employees. The higher wages of formal labor render some firms financially unviable, leading to increased closures. These effects are particularly pronounced in non-tradable sectors, consistent with the conventional understanding that undocumented workers are concentrated in such industries.

Our focus on banking relationships is motivated by the observation that banks are uniquely positioned to acquire information about small firms' employment of undocumented workers and act on it in their lending decisions, leveraging their screening and ongoing monitoring practices, local presence, expertise in collecting soft information, and incentives to screen out riskier borrowers and protect their reputation.

Consistent with the deterrent effect of screening, we find that the characteristic increases in employment and closures following the SC program were concentrated among firms without an ex-ante banking relationship. Even among firms with active banking relationships, we find a greater rise in employment and closures for those whose lenders had closed branches in the county prior to the program, reducing their monitoring capacity, consistent with the deterrent effect of monitoring.

By documenting the link between banking relationships and borrowers' use of undocumented labor, our findings carry important policy implications. They suggest that banks, through their screening and monitoring practices, may play a role in shaping the compliance of borrower firms even with non-financial regulations.

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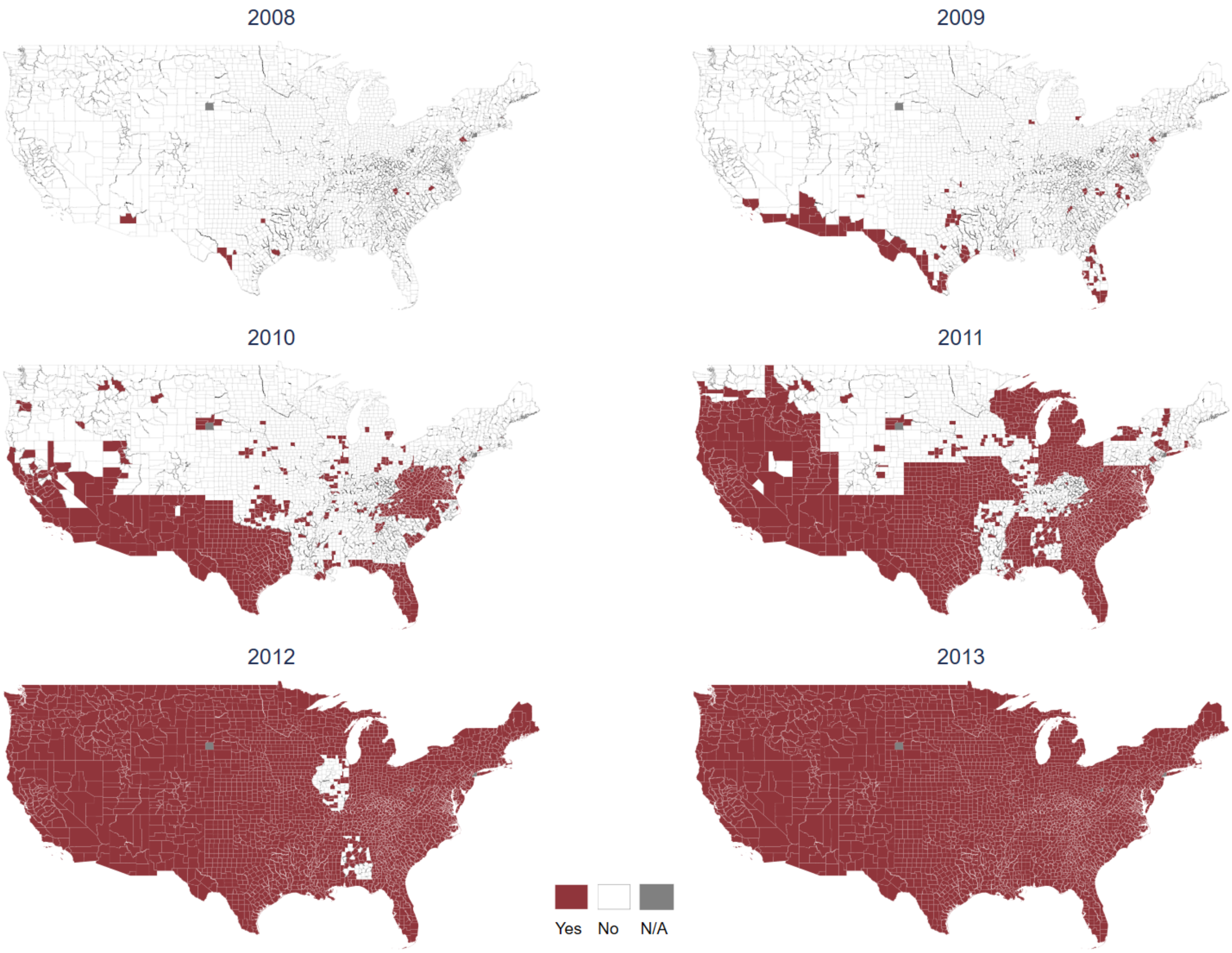

Figure 1. Staggered Rollout of Secure Communities

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

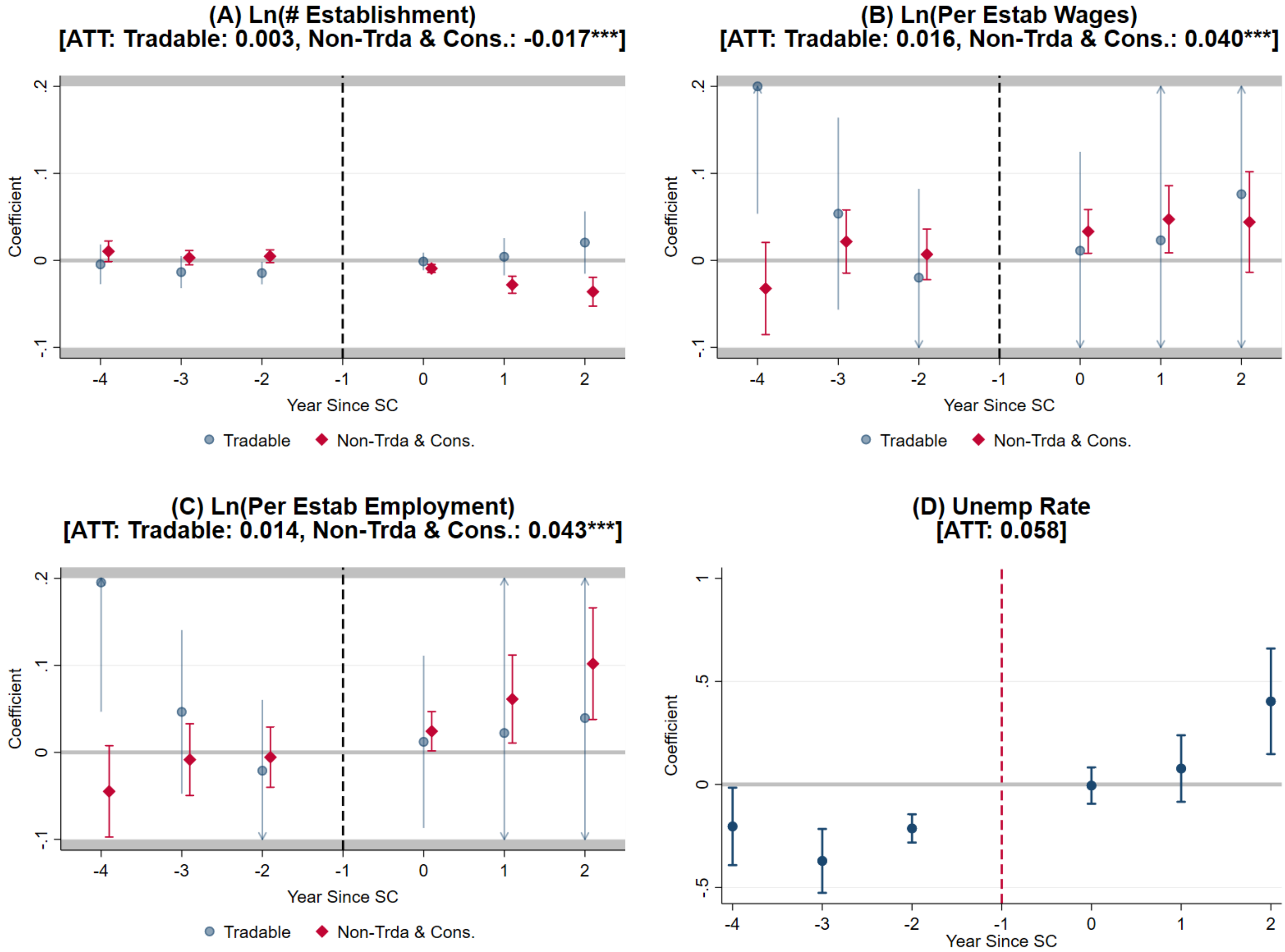

Figure 2. Effect on County-Level Outcomes

This figure plots the estimated β_τ coefficients and their 95% confidence intervals obtained from the county version of equation (2), as described in footnote 9. These are obtained using the estimator from Callaway and Sant'Anna (2021) and without control. The outcome variables are indicated in the respective panels. The horizontal axis denotes years relative to the implementation of SC, with $\tau = 0$ representing the first treated year. A county is classified as treated in years when SC was operational for at least half of the year. Confidence intervals are based on standard errors clustered at the county level. Panels A to C show the corresponding outcomes for the non-tradable and construction sector, and the tradable sector. Panel D reports the unemployment rate for all industries.

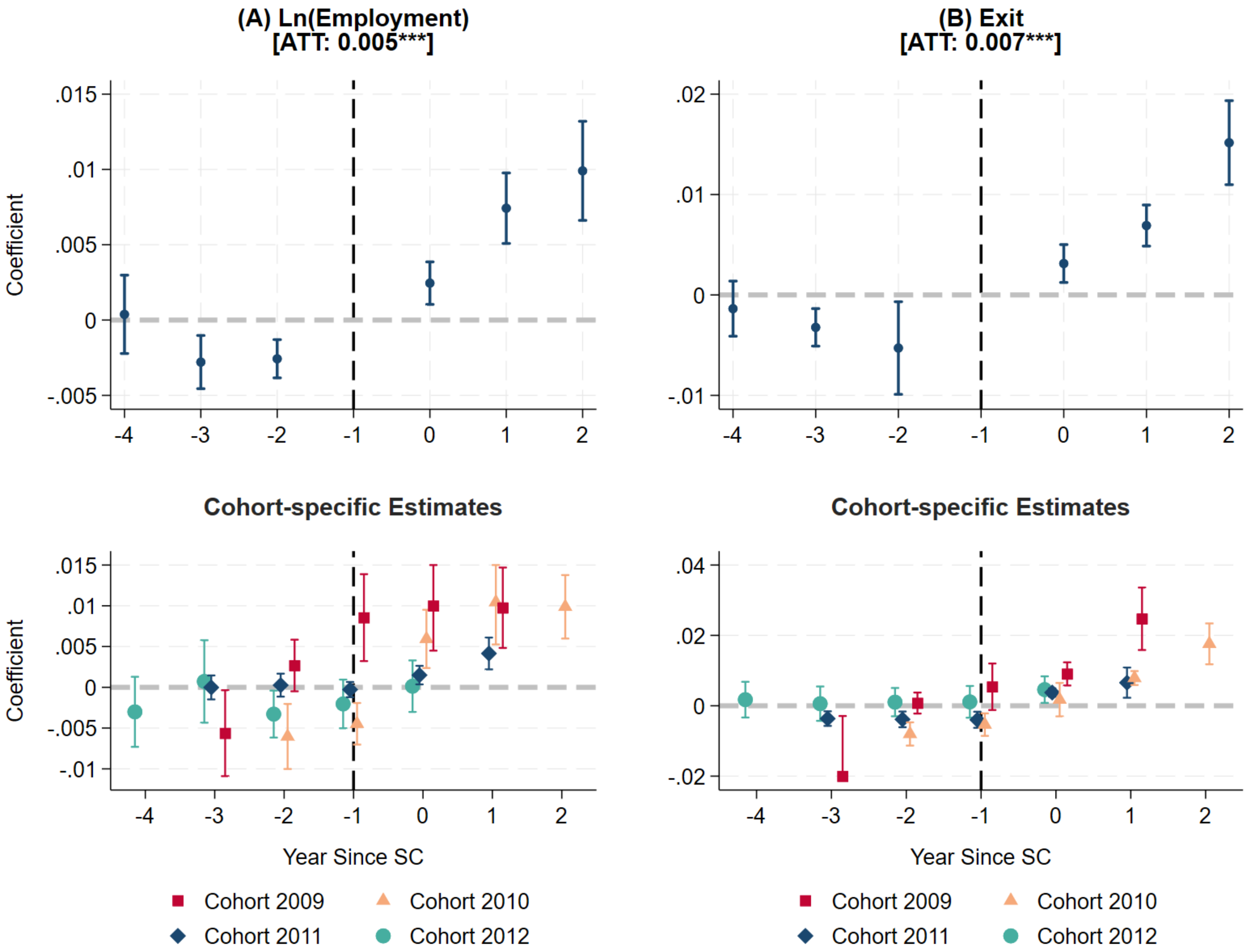

Figure 3. Effect on Small Businesses

This figure shows the coefficients β_τs and 95% confidence intervals obtained from the regression equation (2) for two firm-level variables indicated in the respective panels using the estimator from Callaway and Sant'Anna (2021). The horizontal axis shows years relative to the treated year, with $\tau = 0$ indicating the first treated year. The confidence intervals are based on standard errors clustered by county. Cohort-specific dynamic treatment effects are shown 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 the non-tradable and construction sector, and the tradable sector.

	N	Mean	P25	Median	P75	SD
	(1)	(2)	(3)	(4)	(5)	(6)
A. Firm×Year-Level Variables						
Emp	26,677,908	7.16	2.00	3.00	6.00	11.30
Exit	26,677,908	0.06	0.00	0.00	0.00	0.24
B. County×Year-Level Variables						
Per Capita Income (000's)	21,336	34.95	28.95	33.18	38.65	9.50
County Population (000's)	21,336	96.66	11.16	25.85	66.66	309.80
Hispanic Population (000's)	21,654	8.67	0.15	0.49	2.22	66.50
Non-Tradable & Construction:						
# Est	21,651	682.61	81.00	192.00	508.00	1929.85
Avg Wages (000's)	21,650	168.68	69.97	136.52	243.76	129.86
Avg Emp	21,650	6.64	3.45	5.91	9.42	4.15
Tradable:						
# Est	21,326	95.84	10.00	25.00	66.00	344.88
Avg Wages (000's)	21,301	408.84	0.00	47.88	465.06	878.62
Avg Emp	21,301	7.64	0.00	1.46	10.22	14.09

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 (4), we gradually include the natural logarithm of one-year lagged control variables. County-level per capita income, population, and hispanic population are defined in Table I. *Net Intl. Migration* is the 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)
L.# Est Growth	-0.565	-0.472	-0.472	-0.491
	(0.385)	(0.456)	(0.456)	(0.456)
L.Avg Wages Growth	-0.446	0.255	0.255	0.246
	(0.292)	(0.334)	(0.334)	(0.333)
L.Avg Emp Growth	1.058**	0.223	0.223	0.246
	(0.453)	(0.517)	(0.517)	(0.514)
L.Ln(Per Capita Income)		-0.060	-0.060	-0.071
		(0.052)	(0.052)	(0.052)
L.Ln(County Population)		-0.083***	-0.083***	-0.087***
		(0.017)	(0.017)	(0.017)
L.Ln(Hispanic Population)		0.196***	0.196***	0.194***
		(0.012)	(0.012)	(0.012)
L.Net Intl. Migration				0.018
				(0.021)
FE: Year	Y	Y	Y	Y
Observations	10,764	10,657	10,657	10,657

Table III
County-Level Industry Outcomes

All columns report estimates from county-level version of the regression equation (1), as specified in footnote 10, using CSDID estimator. *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 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-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***	0.040***	0.043***	0.003	0.016	0.014	0.058
	(0.003)	(0.014)	(0.014)	(0.007)	(0.073)	(0.062)	(0.053)
Controls	N	N	N	N	N	N	N
FE: Year	Y	Y	Y	Y	Y	Y	Y
FE: County	Y	Y	Y	Y	Y	Y	Y
Observations	18,556	17,987	17,987	18,247	10,461	10,461	18,531

Table IV
Effect on Small Businesses

All columns report estimates from regression equation (1) using CSDID estimator. *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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

	(1)	(2)	(3)	(4)
	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.005***	0.008***	0.005***	0.007***
	(0.001)	(0.001)	(0.001)	(0.001)
Controls	N	N	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
Observations	19,984,525	19,984,525	19,815,112	19,815,112

Table V
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 the 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	0.007***	0.004***	0.002	0.000
	(0.001)	(0.001)	(0.002)	(0.000)
Controls	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
Observations	16,738,871	16,738,871	1,338,749	1,338,749

Table VI
Effect Heterogeneity by Ex-ante Banking Relationship and Industry

All columns report estimates from regression equation (1) using CSDID estimator. Columns (1)–(4) present results for firms in non-tradable and construction sectors, while (5)–(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	0.004***	0.003**	-0.001	0.000	0.002	0.004	0.001	0.001*
	(0.001)	(0.001)	(0.002)	(0.000)	(0.003)	(0.002)	(0.003)	(0.000)
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	6,583,250	6,583,250	556,525	556,525	1,077,638	1,077,638	153,431	153,431

Table VII
Effect Heterogeneity by Ex-ante Bank Branch Closure

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-median 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 two 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	0.044***	0.004***	0.029***	-0.001
	(0.006)	(0.001)	(0.004)	(0.002)
Controls	Y	Y	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
Observations	27,365	27,365	100,881	100,881

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
Effect Heterogeneity by Ex-ante Banking Relationship - No Controls

All columns report estimates from regression equation (3) 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. All models include firm and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

$$\text{Y}_{ict} = \alpha_0 + \beta \times \text{SC}_{ict} + \delta_c + \gamma_t + \epsilon_{ict} \tag{3}$$

Ex-Ante Bank Relation:	No Secured Loan		Secured Loan	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.005***	0.005***	0.003	0.000
	(0.001)	(0.001)	(0.002)	(0.000)
Controls	N	N	N	N
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
Observations	16,887,206	16,887,206	1,348,022	1,348,022

Table A.2
Effect Heterogeneity by Ex-ante Banking Relationship and Industry - No Controls

All columns report estimates from regression equation (3) using CSDID estimator. Columns (1)–(4) present results for firms in non-tradable and construction sectors, while (5)–(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. All models include firm and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

$$\text{Y}_{ict} = \alpha_0 + \beta \times \text{SC}_{ict} + \delta_c + \gamma_t + \epsilon_{ict}$$

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	0.004***	0.004***	0.001	-0.000	0.002*	0.005***	0.002	0.001*
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.003)	(0.000)
Controls	N	N	N	N	N	N	N	N
FE: Year	Y	Y	Y	Y	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y	Y	Y	Y	Y
Observations	6,646,180	6,646,180	560,888	560,888	1,084,851	1,084,851	154,196	154,196

Table A.3
Effect Heterogeneity by Ex-ante Bank Branch Closure - No Controls

All columns report estimates from equation (3) 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-median 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. All models include firm and year fixed effects. Standard errors are clustered by county and reported in parentheses. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$.

$$\mathrm{Y}_{ict} = \alpha_0 + \beta \times \mathrm{SC}_{ict} + \delta_c + \gamma_t + \epsilon_{ict}$$

Ex-Ante Branch Closure:	Yes		No	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.041***	0.004***	0.032***	-0.001
	(0.006)	(0.001)	(0.004)	(0.002)
Controls	N	N	N	N
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
Observations	27,737	27,737	101,568	101,568

Table A.4
Effect on Small Businesses

All columns report estimates from regression equation (4) using CSDID estimator. *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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

$$\mathrm{Y}_{ict} = \alpha_0 + \beta \times \mathrm{SC}_{ict} + \mathrm{Controls}_{ct} + \delta_c + \gamma_t + \eta_{s(i)} + \epsilon_{ict} \tag{4}$$

	(1)	(2)	(3)	(4)
	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.005***	0.008***	0.004***	0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
Controls	N	N	Y	Y
FE: Year	Y	Y	Y	Y
FE: Firm	Y	Y	Y	Y
FE: Sales Quartile	Y	Y	Y	Y
Observations	19,984,525	19,984,525	19,814,566	19,814,566

Table A.5
Effect Heterogeneity by Ex-ante Banking Relationship

All columns report estimates from regression equation (4) 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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

$$\mathrm{Y}_{ict} = \alpha_0 + \beta \times \mathrm{SC}_{ict} + \mathrm{Controls}_{ct} + \delta_c + \gamma_t + \eta_{s(i)} + \epsilon_{ict}$$

Ex-Ante Bank Relation:	No Secured Loan		Secured Loan	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.005***	0.004***	0.003*	0.000
	(0.001)	(0.001)	(0.002)	(0.000)
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	16,738,394	16,738,394	1,338,717	1,338,717

Table A.6
Effect Heterogeneity by Ex-ante Banking Relationship and Industry

All columns report estimates from regression equation (4) using CSDID estimator. Columns (1)–(4) present results for firms in non-tradable and construction sectors, while (5)–(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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

$$Y_{ict} = \alpha_0 + \beta \times SC_{ict} + Controls_{ct} + \delta_c + \gamma_t + \eta_{s(i)} + \epsilon_{ict}$$

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	0.004***	0.003**	0.000	0.001*	0.001	0.003	0.001	0.001*
	(0.001)	(0.001)	(0.002)	(0.000)	(0.002)	(0.002)	(0.003)	(0.000)
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	6,583,118	6,583,118	556,516	556,516	1,077,615	1,077,615	153,427	153,427

Table A.7
Effect Heterogeneity by Ex-ante Bank Branch Closure

All columns report estimates from equation (4) 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-median 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 two 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. $^{*}p < 0.1$; $^{**}p < 0.05$; $^{***}p < 0.01$.

$$\mathrm{Y}_{ict} = \alpha_0 + \beta \times \mathrm{SC}_{ict} + \mathrm{Controls}_{ct} + \delta_c + \gamma_t + \eta_{s(i)} + \epsilon_{ict}$$

Ex-Ante Branch Closure:	Yes		No	
	(1)	(2)	(3)	(4)
Outcome:	Ln(Emp)	Exit	Ln(Emp)	Exit
SC	0.043***	0.004***	0.030***	-0.001
	(0.008)	(0.001)	(0.004)	(0.002)
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	27,365	27,365	100,881	100,881