Targeted Credit Access and Entrepreneurship *

Spencer D. Sween

University of California, Santa Barbara

spencersween@ucsb.edu

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Abstract

Entrepreneurial entry is often constrained by limited credit access, especially in disadvantaged regions and among underserved groups. Because productive startups can generate substantial local gains in employment and income, there is strong policy interest in alleviating credit market frictions. The U.S. Community Development Financial Institutions (CDFI) program supports mission-driven lenders that expand credit access for targeted populations and areas. This paper examines how local CDFI access influences productive startup formation and labor market outcomes. Using a semiparametric difference-in-differences design that exploits staggered CDFI entry across regional credit markets, I find that CDFI presence significantly increases startup formation without lowering the average firm quality. The effects are strongest in areas with higher minority shares. These results suggest that CDFIs mitigate long-standing credit constraints, fostering productive entrepreneurship.

*Preliminary draft—please do not circulate.

1 Introduction

Entrepreneurship is widely recognized as a driver of economic growth, innovation, and local development. New firm formation can stimulate employment, raise incomes, and generate productivity spillovers that enhance the performance of existing businesses. In underserved communities, entrepreneurship-promoting policies may serve as one instrument among others to encourage more inclusive economic growth, although the effectiveness of such approaches depends on broader institutional and structural conditions.

Access to external finance remains a major barrier to entrepreneurial entry. Many prospective founders lack established banking relationships, sufficient collateral, or robust credit histories, which limits their ability to secure loans. These frictions are especially acute among minority populations, reflecting persistent racial disparities in wealth, financial inclusion, and credit market discrimination (Bennett & Chatterji, 2023; Fairlie et al., 2022a; Kim et al., 2021). Spatial disparities also persist, as neighborhoods with histories of redlining or limited bank presence, often in rural or low-income areas, exhibit systematically lower rates of business formation. The result is a durable entrepreneurship gap across regions and demographic groups. Importantly, these gaps do not simply reflect differences in entrepreneurial ability or idea quality. For example, Black adults are roughly half as likely as White adults to own a business, even though entrepreneurial idea quality appears comparable across groups (Bennett & Robinson, 2024a; Fairlie et al., 2022b). This pattern suggests a misallocation of entrepreneurial talent, which may entail sizable aggregate welfare losses (Hsieh et al., 2019). Recognizing these losses provides a rationale for public policies aimed at expanding opportunities for innovation and business creation in disadvantaged communities.

Policy efforts to address financial access frictions typically operate along three margins. The first regulates incumbent banks, most notably through the Community Reinvestment Act's disclosure and examination regimes. The second subsidizes potential borrowers, for instance through Small Business Administration loan guarantee programs. The third expands alternative credit supply via mission-oriented, nonbank financial institutions. This paper examines the entrepreneurial impact of this third approach, focusing on the Community Development Financial Institutions (CDFI) program. CDFIs are government-certified, mission-driven lenders that must direct at least 60 percent of their activity toward low-income areas or historically underserved populations. In addition to providing flexible capital, they often deliver advisory and technical assistance to help borrowers develop business plans, manage finances, and sustain operations. By design, CDFIs fill gaps left by traditional intermediaries and extend credit where conventional lending relationships are weak.

I construct a ZIP-code–level panel spanning 1988 to 2016 by combining firm formation and quality measures from the Startup Cartography Project (Andrews et al., 2022). I also include establishment counts, employment, and wage measures from the Census Bureau's ZIP Code Business Patterns data. Using regulatory disclosure records from the U.S. Treasury, I infer local CDFI market entry based on transaction histories of newly certified or financially awarded lenders. The empirical strategy exploits the staggered expansion of CDFI lending across regional credit markets to estimate how greater credit access affects startup formation, predicted startup quality, and growth outcomes such as IPOs and acquisitions. I then assess whether these entrepreneurship effects translate into broader labor market gains.

I adopt an identification strategy that addresses the selection concerns complicating an unconditional difference-in-differences approach in this context. Because CDFIs may enter markets based on recent entrepreneurial dynamics or credit conditions, I condition on detailed regional characteristics and compare treated areas to not-yet-treated neighborhoods. The not-yet-treated regions serve as the comparison group because they are more similar in pre-treatment characteristics and outcome trajectories. I condition on a rich set of baseline covariates and employ doubly robust estimators for the average treatment effects on the treated, using flexible machine-learning methods to model the first-stage nuisance functions. For robustness, I also estimate an alternative specification that conditions on lagged outcome paths up to the time of treatment, following Callaway and Li, 2023.

My results show that CDFI entry increases new startup formation over the subsequent decade without any detectable decline in predicted startup quality. The effects are strongest in areas with larger minority population shares and in lower-income neighborhoods with higher poverty and unemployment. I find no significant change in the number of growth outcomes, such as initial public offerings or acquisitions, suggesting that the additional entrants are comparable in potential but not disproportionately high-growth firms. To assess whether these new businesses persist, I examine establishment counts from the ZIP Code Business Patterns data and find suggestive evidence of sustained increases in total establishments. Finally, I document modest gains in local employment-to-population ratios, indicating that expanded CDFI access not only stimulates firm creation but also supports broader labor market improvements. However, average wages are not affected.

These findings align with models of entrepreneurial selection under credit constraints, such as Evans and Jovanovic, 1989, in which relaxing borrowing limits allows high-ability but low-wealth individuals to enter and expand productive firms. By expanding capital access toward more entrepreneurs, CDFIs increase local output and labor demand, leading to gains in employment. New firm creation generates direct hiring and indirect spillovers through supplier linkages and household spending, consistent with local multiplier effects (Glaeser and Kerr, 2009; Moretti, 2010). The strongest effects in minority and low-income areas are in line with evidence from Fairlie et al., 2022a and Bennett and Robinson, 2024a, which show that unequal access to capital, rather than differences in entrepreneurial ability, accounts for much of the racial gap in business ownership.

The remainder of the paper is organized as follows. Section 2 reviews the related literature and provides background on the CDFI program. Section 3 describes the data sources and sample construction, while Section 4 outlines the empirical strategy. Section 5 presents the main results, and Sections 6 and 7 discuss underlying mechanisms and conclude.

2 Background and Literature

This study contributes to the literatures on place-based development credit access, and local entrepreneurship. It adds to research on financial frictions and business formation by providing new quasi-experimental evidence that expanded credit access can stimulate high-growth firm formation. Prior work documents the roles of wealth, collateral constraints, and credit access in shaping entrepreneurial entry (Evans & Jovanovic, 1989; Hurst & Lusardi, 2004). Recent work highlights the underrepresentation of minorities among firm founders despite comparable entrepreneurial idea quality (Bennett & Robinson, 2024b; Fairlie et al., 2022a). The paper further speaks to the literature on place-based economic development through government policy (Freedman, 2012; Freedman et al., 2023).

2.1 CDFI Program

Community Development Financial Institutions are specialized financial intermediaries dedicated to expanding economic opportunity in low-income and minority communities by improving access to affordable credit and financial services. The CDFI program was established under the Riegle Community Development and Regulatory Improvement Act of 1994 and is administered by the U.S. Department of the Treasury CDFI Fund. The Treasury supports CDFIs through certification and financial assistance from grants and subsidized bond programs. In addition to providing flexible capital, many CDFIs offer advisory and technical assistance such as credit counseling, business planning, and financial education to help borrowers develop and sustain successful enterprises.

CDFI certification is an institution-level designation that conveys both credibility as a mission-oriented lender and access to federal resources. To qualify, an organization must be a legal, non-governmental financing entity with a primary mission of community development, provide financial products and related development services, and serve one or more eligible target markets. Certified institutions are eligible for the Treasury's Financial Assistance and Technical Assistance awards and frequently gain improved access to program-related investments from foundations, banks, and impact investors.

Certification interacts with the Community Reinvestment Act policies. Banks can receive CRA consideration for lending to, investing in, or partnering with certified CDFIs, which are recognized as qualified community development entities. This linkage encourages banks to channel capital into underserved areas indirectly through CDFIs, even when they lack a local lending presence. For CDFIs, CRA-motivated partnerships with banks are an important source of capital and help sustain lending in their target communities.

CDFIs maintain a diverse funding structure that combines earned income from loan interest and fees, retained earnings, Treasury awards, philanthropic grants, program-related investments, state and local development funds, and private impact investment notes. Some also participate in federal initiatives such as the New Markets Tax Credit and the Small Business Administration 7(a) and 504 loan programs. This blend of public and private capital allows CDFIs to offer longer maturities, more flexible underwriting, and patient capital designed to accommodate borrowers with limited collateral or nontraditional credit histories.

Despite the program's long history, rigorous evidence on the economic effects of CDFIs remains limited. Harger et al., 2019 study CDFI and New Markets Tax Credit activity in California and find little correlation with new business formation. Swack et al., 2015 documents expanded credit access in CDFI-served areas but finds only modest gains in local economic indicators. More recently, Gong et al., 2023 finds that certified CDFIs increase lending more rapidly than comparable non-certified lenders, particularly in areas with unmet credit demand, though the study does not assess broader economic outcomes.

2.2 Target Investment Market Definitions

CDFI certification requires that institutions serve one or more target investment markets, which specify where and to whom certified lenders must direct their financing. Target markets take two primary forms: investment areas, defined geographically, and targeted populations, defined by borrower characteristics rather than location. At least 60 percent of a CDFI's total financing activity must occur within its designated markets, which lenders select at certification to align with their mission and operational footprint.

Investment areas are census tract–based geographies that exhibit economic distress and unmet credit needs. A geography qualifies as an investment area if it meets at least one of several distress criteria based on the most recent census or American Community Survey data. These include a poverty rate of 20 percent or higher; a median family income at or below 80 percent of an appropriate area benchmark; an unemployment rate at least 1.5 times the national average; significant population or migration loss—defined as a population decline of at least 10 percent between the two most recent censuses or a net migration loss of 5 percent over five years; or full inclusion within a federally designated Empowerment Zone or Enterprise Community. Investment areas must also demonstrate unmet lending or equity investment needs, and in multi-tract configurations, at least 85 percent of the population must reside in tracts that individually meet one or more distress criteria.

Targeted populations are defined by income or group identity rather than geography. The CDFI Fund recognizes two categories: low-income targeted populations and other targeted populations. Individuals qualify as members of a low-income targeted population if their family income is at or below 80 percent of the area median for metropolitan areas or, for nonmetropolitan areas, 80 percent of the greater of the area or the state nonmetropolitan median. Other targeted populations include demographic groups that have historically faced limited access to mainstream financial services, including African Americans, Hispanics, Native Americans, Native Alaskans, Native Hawaiians, and other designated groups. Eligibility for these categories depends on borrower characteristics rather than place of residence.

3 Data

The primary data on firm formation and entrepreneurial dynamics come from the Startup Cartography Project (SCP), which provides a comprehensive and spatially detailed record of new firm creation in the United States. The SCP identifies all newly registered employer corporations, partnerships, and LLCs, recording each firm's founding year, industry, initial employment, and subsequent growth outcomes. The dataset covers formally registered employer firms—entities that hire at least one paid employee in their first year of operation—and excludes self-employment, sole proprietorships, and other nonemployer establishments. This restriction ensures that the entrepreneurship measures capture growth-oriented firm creation.

By focusing on formal employer startups, the SCP isolates the segment of entrepreneurial activity most relevant for regional productivity and employment growth. In addition to entry counts, the SCP provides regional aggregates of subsequent firm outcomes—including acquisitions and initial public offerings—allowing measurement of both the scale and quality of new firm formation. The resulting dataset forms a balanced ZIP–year panel spanning 1988–2016, offering consistent coverage of new employer startups across U.S. localities.

Labor market conditions are measured using the Census Bureau's ZIP Code Business Patterns (ZBP) data, which report annual employment, establishment counts, and average payroll per employee from 1994 onward. Demographic and socioeconomic characteristics are drawn from the American Community Survey (ACS) 2008–2012 five-year estimates, aggregated to ZIP codes. These include population, median household income, educational attainment, racial composition, and other population share measures.

Information on access to CDFI credit comes from the U.S. Treasury's CDFI Fund Transaction Level Reports (TLR). The TLR dataset records all loan and investment transactions reported by certified CDFIs in connection with CDFI Fund awards. Each record identifies the borrower or project's census tract, transaction year, loan amount, product type, and originating CDFI. These data provide nationally standardized information on the timing and intensity of CDFI activity dating back to the program's start in 1996. Because reporting is self-reported and required only for recent CDFI Fund financial award recipients, coverage is less complete for non-recently-awarded lenders. Nonetheless, the TLR remains the most comprehensive national dataset on CDFI lending activity.

3.1 Outcome Measures

Startup rate per 1,000 residents. The extensive margin of entrepreneurship is defined as the number of new employer establishments formed in ZIP i during year t, normalized by local population in thousands:

$$SR_{ict} = \frac{N_{ict}}{\text{Population}_{ict}/1000},$$

where N_{ict} is the number of new startups. Because the SCP excludes self-employed and nonemployer entities, SR_{ict} reflects the rate of formal firm creation in a neighborhood.

Startup quality score. The second measure captures the expected growth potential of startups at the time of founding. For each new firm j in ZIP i, county c, and year t, the SCP provides a predicted probability of achieving a "growth event"—defined as reaching an initial public offering or a major acquisition within six years—based on a logistic regression model estimated on historical firm outcomes. Let Z^*_{jict} denote a vector of firm-level characteristics, including industry, founding size, founding year, and early employment expansion. The model predicts

$$p_{jict} = \sigma(Z^{*\prime}_{jict}\gamma_0), \qquad \sigma(x) = \frac{1}{1+e^{-x}},$$

where γ_0 represents the estimated parameters from the population logistic regression. The SCP reports the ZIP–year average,

$$\bar{p}_{ict} = \begin{cases} \frac{1}{N_{ict}} \sum_{j=1}^{N_{ict}} p_{jict}, & \text{if } N_{ict} > 0, \\ 0, & \text{if } N_{ict} = 0, \end{cases}$$

which summarizes the ex ante quality of startups formed in location i at time t. To link startup quality to the extensive margin of entry, I define the composite measure $Q_{ict} = \bar{p}_{ict} \times SR_{ict}$, representing the number of startups per 1,000 residents with high predicted growth potential.

Growth outcomes. The SCP also tracks realized "growth events", which are cases in which a startup reaches an IPO or a major acquisition within six years of founding. Aggregating these firm-level events to the ZIP–year level yields the number of growth achieving firms.

Firm Survival Outcomes. Because the SCP provides information on firm births and expansions but not exits, I supplement it with ZIP-level bankruptcy filings from the Federal Judicial Center and establishment counts from the ZBP.

Labor market outcomes. Labor market indicators from the ZBP include total employment, establishment density, and average payroll per employee. From these, I construct log average wage and employment-to-population ratio measures.

3.2 CDFI Access and Treatment Group Construction

The timing and spatial pattern of CDFI entry are derived from the TLR data. County-level entry is defined as the first year in which any certified CDFI reports a transaction within the county. Because CDFIs often lend across ZIP and tract boundaries, treatment is assigned at the county level and applied to all ZIP codes within that county, yielding a ZIP–year treatment indicator that reflects the first year of local access to CDFI credit.

This treatment definition aims to capture the opening of a new, locally accessible credit channel rather than the intensity of subsequent lending. Counties represent a plausible approximation of the functional market area for small business credit: most community banks and CDFIs organize operations at a regional or county scale, and many borrowers plausibly travel within the same county to access financial institutions. By focusing on county-level first entry, the analysis interprets treatment as the timing of the earliest local opportunity for CDFI engagement, consistent with the idea that entry expands the feasible lender choice set for entrepreneurs within that credit market.

This aggregation proxies for the effective geographic reach of CDFI lending. Ideally, one would use the Treasury's official investment area designations, but these are not publicly available and often refer to target populations rather than fixed geographies. County-level aggregation therefore provides a practical measure of CDFI market presence.

To evaluate whether this county-level approach plausibly reflects a single local credit market, I compute for each CDFI–year a loan-weighted average distance between the census tracts where loans were reported in the TLR data. Figure 1 plots the cumulative distribution of this measure by year, truncated at 100 miles. The distributions are highly concentrated: in a typical year, the median loan-weighted average distance is roughly 20–30 miles, about two-thirds of CDFIs operate with average distances under 50 miles, and more than 80 percent fall below 100 miles. Only a small share of organizations lend over broader, multi-regional footprints. The shape and stability of these distributions over time indicate that CDFIs consistently serve geographically compact markets.

The final panel includes roughly 31,000 ZIP codes observed annually from 1988 to 2016, excluding sparsely populated or nonresidential areas. Approximately 88 percent of the sample eventually gain CDFI exposure within their counties. Adoption cohorts are defined by the county's first entry year: 49 percent of treated ZIPs gain access between 1996 and 1999, 44 percent between 2000 and 2009, and 7 percent between 2010 and 2015. This staggered rollout generates substantial spatial and temporal variation in exposure to mission-driven credit.

While the TLR provides detailed information on the timing, location, and amount of lending, it does not fully capture loan purposes or non-award CDFI activity. Consequently, the resulting measure should be interpreted as identifying the onset of access to CDFI-supported finance rather than the total volume of mission-driven credit extended within a county.

4 Impacts of CDFI Access on Startup Activity

To assess how mission-oriented financial access—delivered through Community Development Financial Institutions—affects new entrepreneurial activity, I exploit the staggered expansion of CDFIs across regional credit markets. I proxy for CDFI entry by defining it as the first year in which a CDFI lender records a loan within a county, based on the TLR disclosures. While outcomes are measured at the ZIP-code level, treatment is assigned at the county level to reflect the plausible geographic reach of CDFI operations. This assignment provides a conservative measure of exposure and mitigates misclassification from untreated ZIP codes that nonetheless fall within an institution's effective investment market.

The core empirical strategy compares trends in entrepreneurial and economic outcomes between ZIP codes located in counties with and without CDFI access, employing a difference-in-differences (DiD) framework. My main identification strategy rests on a parallel-trends assumption: in the absence of CDFI entry, treated and untreated areas would have experienced similar trajectories in startup formation and growth outcomes. Because CDFI entry is not randomly assigned—and may be systematically influenced by demographic composition, local financial conditions, and recent trends in population or economic activity—credible identification requires the construction of well-matched comparison groups and the inclusion of covariates that account for both the selection process into treatment and the evolving determinants of the enterperpreneurial outcome dynamics.

To this end, I motivate limiting treatment group comparisons to ZIP codes in not-yet-treated counties and condition on a rich set of pre-treatment covariates. These include long-run structural characteristics that shape financial access and entrepreneurial capacity—such as average incomes and employment rates, population composition, and local banking infrastructure—as well as state fixed effects that absorb common macroeconomic and policy shocks over time. I also incorporate measures of evolving local conditions, such as population growth and infrastructure development, to account for recent dynamics that may jointly influence the timing of CDFI entry and subsequent outcome trajectories. Finally, I benchmark the DiD strategy with an alternative design that conditions on lagged outcome dynamics that may influence CDFI entry in a given time period and affect the post-treatment outcome paths. Together, these approaches help mitigate concerns about observable selection and differential trends that could bias unadjusted comparisons. The remainder of this section formalizes the identifying assumptions and outlines the empirical implementation.

Setup and Target Parameters. Let i index ZIP codes, c counties, and $t \in \{1, \ldots, T\}$ years. Denote the county-level treatment cohort assignment by $G_{ic} \in \{1, \ldots, T, \infty\}$, where G_{ic} is the first year in which a CDFI transaction occurs in county c, and $G_{ic} = \infty$ for never treated ZIPs. Let Y_{ict} be the observed outcome and $Y_{ict}(g)$ the potential outcome if the county containing ZIP i received CDFI access in year g. Define X_{ic} as a vector of baseline characteristics and V_{ict} as a vector of time-varying covariates capturing evolving local conditions.

My first goal is to quantify the causal effect of non-bank financial access, proxied by CDFI presence, on new startup formation, firm composition, and growth outcomes. These outcomes include (i) the number of new startups per 1,000 residents, (ii) the predicted number of IPO-quality startups per 1,000 residents, and (iii) the annual number of IPO-achieving firms. To assess firm survival, I analyze the changes in the stock of new business, measured by the total number of establishments in each ZIP code. In the labor market analysis, I study log-average wages and employment-to-population ratios. For each outcome, I target the cohort–time average treatment effects on the treated (ATT) as building blocks for summarizing treatment effect heterogeneity across cohorts and over time. For cohort g and calendar year t, I aim to identify the causal contrast,

$$ATT(g,t) \; = \; \mathbb{E}[\, Y_{ict}(g) - Y_{ict}(\infty) \,|\, G_{ic} = g \,] \,. \tag{1}$$

Because the 29-year panel includes 20 distinct treatment cohorts (see Table 1), I summarize the $ATT(g,t)$ parameters with two sets of aggregating measures:

$$ATT^{\text{group}}(g) \;=\; \frac{1}{N_g} \sum_{k:k<T-10} ATT(g, g+k), \qquad N_g = \sum_{k=0}^{10} \mathbf{1}\{g \leq T-k\}, \tag{2}$$

$$ATT^{\text{event}}(k) \;=\; \sum_{g:\, g+k \leq T} \omega_{g,k}\, ATT(g, g+k), \qquad \omega_{g,k} \;=\; \Pr\big(G_{ic} = g \,\big|\, G_{ic} \leq T-k\big). \tag{3}$$

For both summaries, I track effects over the first ten post-entry years to ensure consistent comparison across cohorts and mitigate over-representation of early adopters. The cohort-specific average $ATT^{\text{group}}(g)$ reflects differences in post-entry impacts across adoption cohorts, averaging over all cohort-specific post-treatment periods. In contrast, $ATT^{\text{event}}(k)$ traces the dynamic response to CDFI entry over time, averaging across cohorts at each event time.

Untreated Group Definitions. To identify each cohort–time average treatment effect, I may consider two potential untreated comparison groups to approximate the counterfactual outcomes that treated ZIP codes would have experienced absent CDFI entry. The first group comprises areas that never receive a CDFI loan over the sample period ("never treated"), while the second includes "not-yet-treated" ZIP codes—those in counties that remain untreated in a given year but subsequently gain CDFI access at a later date.

Not-yet-treated areas offer a particularly credible comparison in this context because they are drawn from the same set of potential investment markets that CDFIs ultimately serve. These regions tend to exhibit structural features aligned with CDFI certification criteria and lending missions—including limited banking access, high shares of low- and moderate-income or historically underserved populations, and persistent population decline. As such, they likely fall within the strategic reach of mission-driven financial institutions and reflect credible counterfactual environments for areas that have already gained CDFI access.

Summary Statistics. Table 1 summarizes the composition of treated cohorts by initial adoption year. The diffusion of CDFI market entry follows a staggered, multi-year rollout beginning in 1996 and expanding gradually through the mid-2000s. Early cohorts are relatively large and geographically widespread: the 1996 group alone accounts for roughly 17% of all treated ZIP codes, spanning 41 states and over 500 counties.

Table 2 then reports the socioeconomic characteristics of treated versus untreated ZIP codes. Columns A and B display averages for the earliest adoption cohort (1996) and for all treated areas combined, while Columns C and D summarize outcomes for never-treated and not-yet-treated ZIP codes. For the latter, I construct a stacked panel aligning each cohort's treatment year with untreated areas, weighting means by their observation shares.

Treated ZIP codes differ systematically from both comparison groups: they exhibit higher unemployment rates, minority population shares, and population. to assess overlap with respect to both untreated group definitions, I estimate the propensity scores of treatment adoption in the 1996 cohort. I use a random forest classifier including all demographic measures summarized in Table 2. In Figure 2, I plot the distribution of both sets of propensity scores. I find that many covariate profiles perfectly predict treatment assignment in the 1996 cohort when comparing to the never treated units. In the not-yet-treated comparison, overlap is more established with less bunching at predicted probabilities close to one.

Table 3 also compares treated ZIP codes within CDFI-accessible counties based on whether they record any CDFI lending activity in the Transaction Level Report during the ten-year post-entry window. ZIP codes that experience measurable CDFI lending tend to be more urbanized, demographically diverse, and economically active—displaying higher Hispanic and Black population shares, greater land development, and slightly higher pre-treatment startup rates. These patterns suggest that within treated markets, CDFI lending is concentrated in denser, more dynamic communities that align most closely with the CDFI mission of extending credit to underserved but economically viable markets.

Entrepreneurial Dynamics Before and After CDFI Entry. I next examine how entrepreneurial activity evolves around the time of CDFI entry using unconditional event-time plots from two-way fixed-effects regressions. These event-study analyses provide a descriptive view of how outcomes move before and after CDFI entry, and they demonstrate the treatment groups' degree of comparability with both sets of untreated comparison regions.

I begin by focusing on the first-treated cohort—areas that gained CDFI access in 1996. Examining this group isolates the earliest implementation of the program and offers a window into short-run dynamics among the initial participants, whose selection and local economic composition may differ from later entrants. Moreover, this adoption group is the largest set of treated areas, comprising nearly 20% of all CDFI-accessible ZIP codes.

To complement these results, I construct an all-cohort event-study that aligns each treated area by its own year of CDFI entry and averages the resulting event-time coefficients across cohorts. With 20 distinct adoption groups, individual cohort plots are not easily interpretable; the averaged profile instead provides a concise summary.

For the 1996 cohort, I estimate:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{k \neq -1} \beta_k^{(1996)} \big(\mathbf{1}\{G_i = 1996\} \cdot \mathbf{1}\{t - 1996 = k\}\big) + \varepsilon_{it}, \tag{4}$$

where α_i and λ_t are ZIP and year fixed effects, and $k = -1$ is the omitted reference period. The coefficients $\beta_k^{(1996)}$ trace the average pre- and post-1996 outcome trends of the early exposed regions to the untreated comparison groups. I estimate this using both the never- and not-yet-treated regions as the comparison groups.

To summarize dynamics across full sample of treatment groups, I implement the modified event-study estimator of Sun and Abraham, 2021. This specification interacts relative-time indicators with cohort dummies to obtain cohort-specific estimates, which I then average across cohorts at each event time. For this, I also compare cohort-averaged findings relative to both the never- and not-yet-treated groups by restricting the estimating samples.

For each entrepreneurial outcome, Figures 3 and 4 present the 1996-cohort and full-sample-averaged event-study results, respectively. Each figure reports 95% uniform confidence bands across event times and displays results for both untreated comparisons. For the 1996 cohort, the event-time coefficients do not show statistically significant pre-treatment differences under either comparison. However, joint F-tests of the pre-period coefficients reject equality to zero in across each outcome and comparison group. The cohort-averaged profiles exhibit clear pre-treatment movements: many of the pre-period, lead coefficients are negative and statistically significant, consistent with entrepreneurial activity trending upward as entry approaches. The joint F-tests all reject the null of flat pre-trends relative to both untreated groups. Taken together, these patterns indicates that unconditional DiD comparisons of the treated and untreated outcome trends may be biased for identifying the $ATT(g,t)$.

Main Identification Framework. The descriptive evidence above motivates a semi-parametric difference-in-differences design that leverages, and conditions on, two features of the setting: the institutional rollout of community development financial institutions and rich pre-treatment information capturing selection into entry and baseline entrepreneurial trajectories. Event-time plots indicate nontrivial pre-trend differences across untreated comparison groups, and summary statistics reveal observable imbalance. However, not-yet-treated areas come from the same policy-relevant pool as adopters and display substantially greater covariate overlap. The main analysis therefore restricts comparisons to not-yet-treated areas and conditions on pre-determined characteristics that summarize local conditions prior to CDFI access and influence the post-treatment trends in entrepreneurial activity.

Entry by CDFIs follows a well-defined sequence that generates staggered adoption. Organizations become eligible lenders through certification by the U.S. Treasury's CDFI Fund, which requires delineating and maintaining accountability to an approved target market and committing that the majority of financing activity serves that market. Certification enables applications for financial and technical Assistance awards that provide core lending capital and operating support, typically on annual cycles with matching-fund and deployment requirements (Harger et al., 2019). Timing is further shaped by the build-out of lending capacity—capitalization, staffing, underwriting and servicing systems, and local referral networks. Expansion into a new geographic or borrower market is often coordinated with these milestones and with bank partnerships formed under Community Reinvestment Act incentives (Swack et al., 2015). As a result, the timing of a first observed transaction in a county plausibly reflects institutional and regulatory frictions tied to certification, funding, and capacity ramp-up rather than contemporaneous shocks to local entrepreneurship. At the same time, federal guidelines and mission orientation direct activity toward target communities with limited financial access, including places with weaker credit supply and higher shares of historically underserved populations (Gong et al., 2023).

Given these institutional features, my main empirical strategy imposes conditional parallel trends relative to the not-yet-treated ZIP codes. Assume, for any cohort g and $t \geq g$,

$$\mathbb{E}\big[\, Y_{ict}(\infty) - Y_{ic,g-1}(\infty) \,\big|\, G_{ic} = g, W_{icg} \big] = \mathbb{E}\big[\, Y_{ict}(\infty) - Y_{ic,g-1}(\infty) \,\big|\, G_{ic} > t,\, W_{icg} \big], \quad (5)$$

where $W_{icg} = (X_{ic}, V_{ic,1}, \ldots, V_{ic,g-1})$ collects all pre-treatment characteristics that influence CDFI entry and entrepreneurial activity. Under Equation (5) and overlap with respect to W_{icg}, identification of the $ATT(g,t)$ is follows from

$$ATT(g,t) = \mathbb{E}\big[\big(Y_{ict} - Y_{ic,g-1}\big) - \mathbb{E}\big[\, Y_{ict} - Y_{ic,g-1} \,\big|\, G_{ic} > t,\, W_{icg} \big] \,\big|\, G_{ic} = g \big]\,. \quad (6)$$

Hence, to identify the parameters of interest and summarize them, I construct the predictions of post-treatment outcome trends in the untreated state for treated cohorts by using observationally similar, not-yet-treated units. I condition on a vector X_{ic} that captures structural determinants of entrepreneurial potential and financial access measured strictly prior to treatment. These include fixed state indicators that absorb common macroeconomic, regulatory, and policy shocks; baseline ZIP-level demographics—population size and density, income distribution, and racial and ethnic composition—that shape demand for new business formation; and pre-period banking structure, summarized by bank and credit-union establishments per capita, which proxies for intermediation capacity, competition, and baseline credit supply. I further include a set V_{ict} of slow-moving, pre-trend variables that evolve before first CDFI entry—such as population growth and land development activity—to capture secular local dynamics that would have continued absent access.

Identification concerns arise if CDFIs condition entry on private information about local future outcomes. Such information may include expectations about forthcoming entrepreneurship initiatives, knowledge of pending economic development or investment projects, or assessments of region-specific credit disruptions—for example, the anticipated exit or failure of potential bank partners that would otherwise support CRA-facilitated transactions. If such signals influence where and when CDFIs initiate lending, entry timing can correlate with unobserved determinants of post-entry outcomes, and comparisons to not-yet-treated areas may remain biased even after conditioning on X_{ic} and V_{ict}. A related concern is sorting on the path dependence of business activity: if lenders target locations on rising or declining trajectories, the lagged outcome path both affects CDFI entry and local entrepreneurship.

To address these issues, I maintain a conditional parallel-trends framework and, as a benchmark for selection on dynamic trajectories, I augment the specification with the lagged outcome path. I compare results from the baseline DiD design that conditions on X_{ic} and V_{ict} to a supplemental design that additionally conditions on a vector of pre-entry outcomes. This "selection-on-lagged-observables" approach directly absorbs path dependence that may drive entry decisions and shape untreated potential outcomes, providing a transparent check on whether residual selection on evolving entrepreneurial trends is driving the results.

Alternative Identification: Conditioning on Recent Dynamics. The conditional DiD framework in Equation (5) is credible when CDFI entry timing is primarily driven by administrative and capacity frictions with respect to similarly characterized regions in terms of their demographics, economic conditions, and financial market characteristics. However, if lenders adjust local investment market entry in response to short-run changes in entrepreneurial activity, it may fail even after conditioning on W_{icg}. I adopt an alternative design based on lagged-outcome unconfoundedness (LOU), following Callaway et al., 2023. The approach conditions directly on the recent pre-treatment trajectory of outcomes, allowing lender entry to depend on evolving entrepreneurial or other economic conditions.

To formalize this strategy, let $F_{icg} = \big(Y_{ic,1}, \ldots, Y_{ic,g-1},\, X_{ic},\, V_{ic,1}, \ldots, V_{ic,g-1}\big)$ denote the complete pre-entry state vector for a ZIP code in cohort g, summarizing its entire covariate and outcome histories up to period g. The identifying assumption is that, among areas still untreated at time t, the timing of first entry is independent of future untreated outcomes given the realized pre-entry state:

$$Y_{ict}(\infty) \;\perp\!\!\!\perp\; \{G_{ic} = g\} \;\Big|\; F_{icg}, \quad \forall G_{ic} \in \{g, t+1, \ldots, T\}. \tag{7}$$

Intuitively, after conditioning on both levels and recent paths of the outcomes and covariates, earlier and later entrants with similar F_{icg} should follow comparable counterfactual trajectories absent CDFI access. Under Equation (7), the $ATT(g,t)$ is identified by

$$ATT(g,t) = \mathbb{E}\Big[Y_{ict} - \mathbb{E}\big[Y_{ict} \,\big|\, F_{icg},\, G_{ic} > t\big] \,\Big|\, G_{ic} = g\Big]\,. \tag{8}$$

Both identification strategies exploit variation in the timing of CDFI entry relative to not-yet-treated areas but rest on different assumptions about that timing. The DiD approach equates counterfactual changes across regions after conditioning on slow-moving fundamentals. The LOU framework instead conditions on realized pre-entry trajectories, allowing timing to depend on recent local dynamics. Comparing results across the two designs provides a useful robustness check: similar estimates would suggest limited bias from short-run selection on recent trajectories, while notable differences would indicate that such dynamics influence CDFI entry and untreated entrepreneurship trends.

Estimation and Inference. The two proposed empirical approaches require conditioning on high-dimensional information to flexibly approximate the counterfactual outcomes of not-yet-treated areas. I estimate the conditional means of untreated potential outcomes nonparametrically using random forests. These fitted conditional means enter a doubly robust, Neyman-orthogonal score estimated via a double/debiased machine-learning (DML) procedure (Chernozhukov et al., 2018). The orthogonalization step ensures valid inference even when the first-stage functions are estimated flexibly, by addressing estimation errors and regularization bias from the nuisance function estimation step. For each cohort–time cell (g, t), I estimate the untreated conditional mean and the propensity of entry among the treated and not-yet-treated units, compute $\widehat{ATT}(g, t)$ as the sample mean of the orthogonal score, and aggregate to $\widehat{ATT}^{\text{group}}(g)$ and $\widehat{ATT}^{\text{event}}(k)$ using the sample analogues to (2)–(3). Inference is based on state-clustered standard errors and 95% simultaneous confidence bands.

Both strategies imply that, conditional on the relevant information set, pre-treatment trajectories of treated and comparison units should be statistically indistinguishable. I assess this implication with placebo checks. First, I estimate event-time specifications that include 10 pre-treatment leads of adoption and test whether average effects prior to entry are jointly zero. For the lagged outcome unconfoundedness design, I repeat the same exercise while conditioning year-by-year on the contemporaneous pre-period state. This rolling conditioning ensures that, for each cohort-specific pre-period $t < g$, only information observable at that calendar date enters the adjustment.

4.1 Main Results

Access to a CDFI is followed by a sustained increase in new firm entry. Figure 5 plots event-time estimates with 95% uniform confidence bands. Pre-entry coefficients are small and jointly indistinguishable from zero, while post-entry effects turn positive and remain stable for roughly a decade. The corresponding cohort–time average estimates in Table 4 imply that CDFI entry increases the startup formation rate by about 0.11 new employer firms per 1,000 residents. Relative to a pre-treatment mean of roughly 3.4 startups per 1,000 residents in treated ZIP codes (Table 2), this represents an increase on the order of 3 percent.

Turning to outcomes beyond quantity, Figure 6 shows no detectable change in the average startup quality index following entry. Likewise, Figure 8 presents event-time effects on realized IPOs and major acquisitions, which are statistically null and display no discernible pre-trend. Taken together, these patterns suggest that CDFI access expands the extensive margin of entry without materially shifting the predicted or realized growth outcomes.

The main findings are robust to the alternative identification strategy that conditions on lagged outcome paths. Table 4 also reports the corresponding cohort-averaged effects under the lagged-outcome unconfoundedness design. The magnitude and timing of the estimated startup responses closely mirror those from the conditional DiD framework, and estimated effects on quality and growth events remain near zero.

To assess the survival of these additional startups, I repeat the analysis using the stock of active business establishments from the ZIP Code Business Patterns data, normalized per 1,000 residents. Figure 7 shows that establishment density rises gradually after CDFI entry and remains elevated by 0.17 establishments per 1,000 residents over the following decade. There is no evidence of subsequent decline in the establishment stock, suggesting that the new firms induced by CDFI access are durable and contribute to a persistently larger local business base rather than a short-lived spike in entry followed by rapid exit.

4.2 Heterogeneity Analysis

I examine how the effects of CDFI access vary across communities with different demographic compositions. In particular, I stratify ZIP codes into quartiles based on the baseline share of non-Hispanic white residents and estimate subgroup-specific treatment effects on startup formation. As reported in Table 4, the gains are concentrated in areas with higher minority shares. In the bottom quartile of the white share distribution (corresponding to ZIP codes with the highest minority representation), CDFI entry raises the startup formation rate by about 0.12 startups per 1,000 residents, a statistically significant effect. In the top quartile (lowest minority share), the corresponding estimate is smaller—around 0.06 startups per 1,000 residents—and not statistically distinguishable from zero.

These patterns are consistent with the program's targeting criteria and design. CDFIs are required to direct the majority of their activity toward low-income or historically underserved populations. The heterogeneity in estimated impacts suggests that CDFI access is most effective at expanding entry in places where credit constraints are likely to be most binding and where minority entrepreneurs have historically faced barriers to business ownership.

4.3 Labor Market Effects

To evaluate the broader economic impacts of CDFI access, I analyze labor market outcomes in the 1996 cohort using the ZIP Code Business Patterns data. These results are reported in

Table 5. Average wages appear unaffected. Averaged over the first decade after entry, the employment-to-population ratio increases by roughly 0.47 percentage points.

5 Discussion of Mechanisms

The results point to a mechanism in which CDFIs relax binding credit constraints for would-be entrepreneurs in underserved markets, enabling productive but liquidity-constrained individuals to start and grow firms. This interpretation is consistent with models of entrepreneurial selection under financial frictions, such as Evans and Jovanovic, 1989, in which limited collateral and imperfect capital markets prevent high-ability but low-wealth agents from forming businesses. By expanding the supply of credit and offering flexible underwriting, CDFIs lower borrowing frictions and bring marginal entrepreneurs into the formal sector.

The evidence on heterogeneity supports this view. The largest increases in startup formation occur in ZIP codes with higher minority shares and in areas characterized by lower incomes, higher poverty, and higher unemployment—precisely the communities where wealth gaps and thin credit histories are most likely to restrict entrepreneurial entry. This pattern echoes recent work by Bennett and Robinson, 2024a; Fairlie et al., 2022a, who document that racial disparities in business ownership are driven largely by differences in financial access rather than entrepreneurial ability or idea quality. In that context, CDFIs appear to narrow the entrepreneurship gap by supplying both capital and development services.

The quality and survival evidence further clarifies the mechanism. CDFI entry raises the number of startups without reducing the average predicted quality index or the number of realized growth events. The establishment-stock results show persistent increases in the number of active firms. Together, these findings suggest that CDFIs primarily facilitate productive startup entry and survival, which lead to positive local employment outcomes.

6 Conclusion

This paper provides causal evidence on the role of mission-driven credit access in shaping entrepreneurship and local economic development. Exploiting the staggered expansion of Community Development Financial Institutions across U.S. credit markets, I show that the entry of a CDFI leads to persistent increases in new employer startup formation without degraded firm quality. The additional startups are durable, contributing to a higher long-run stock of establishments, and the largest gains arise in lower-income, high-minority areas that align with the program's targeting criteria. These effects extend beyond firm creation. CDFI entry is associated with modest but meaningful improvements in local labor markets, with employment-to-population ratios increasing. These patterns suggest that CDFIs do not simply reshuffle activity across space or between firms but instead expand the scale of economically viable business activity and labor demand within treated communities.

Taken together, the findings imply that community-oriented financial intermediaries can alleviate long-standing credit constraints that suppress entrepreneurship. By expanding access to capital and providing advisory support, CDFIs help unlock latent entrepreneurial potential and generate employment gains in places that have historically lacked access to mainstream finance. More broadly, the results highlight the importance of nonbank, mission-driven lenders as complements to traditional banking in addressing spatial and racial inequities in entrepreneurial opportunity and in promoting inclusive economic growth.

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Figures and Tables

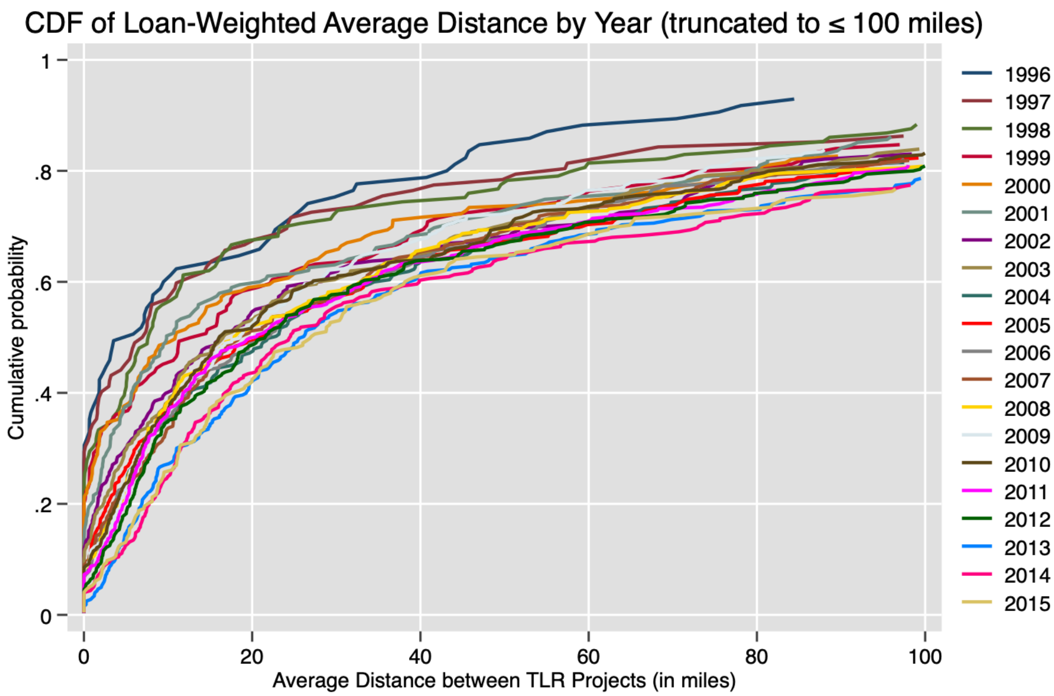


Figure 1: Cumulative distribution of loan-weighted average distance between Census tracts

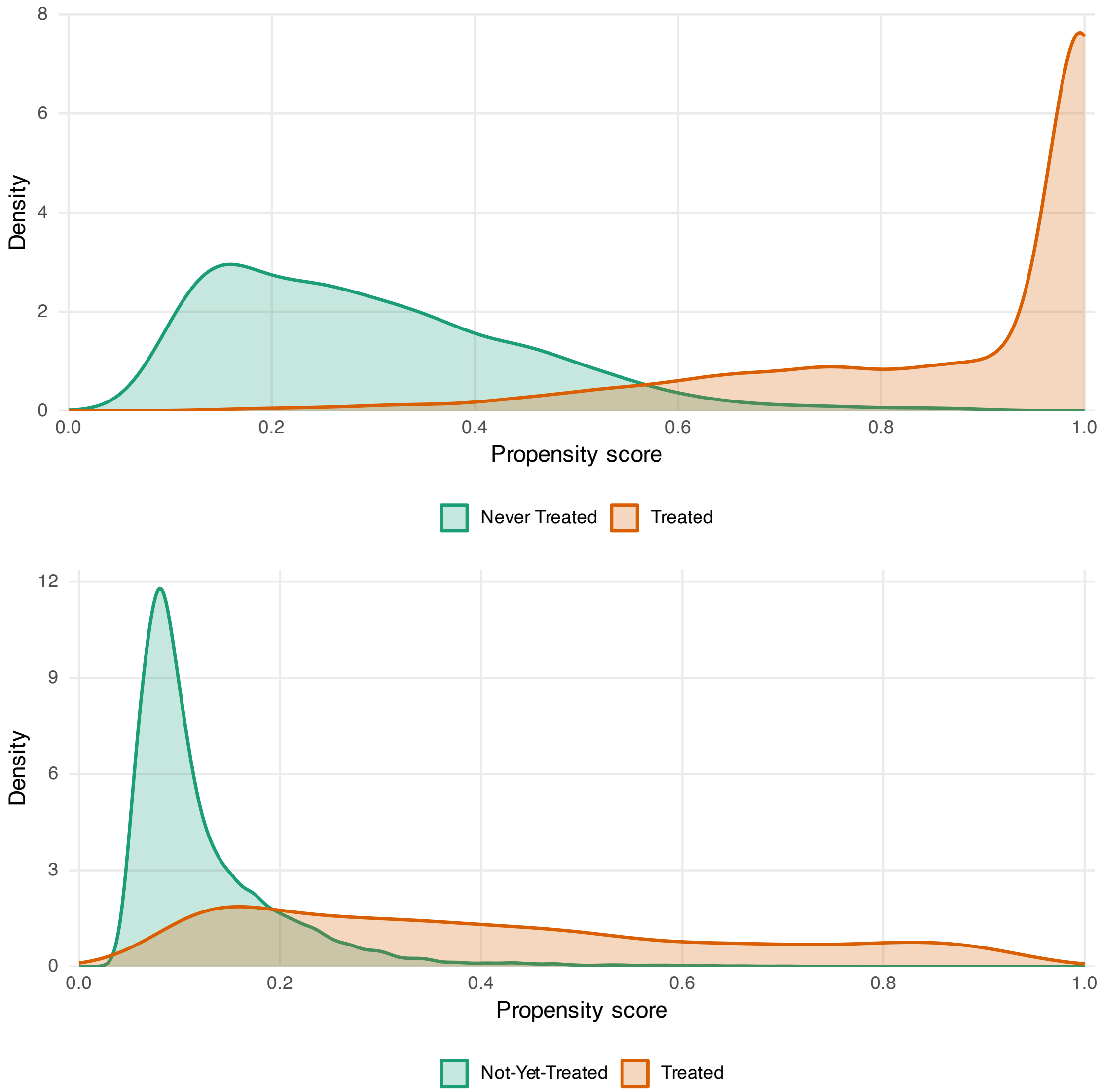
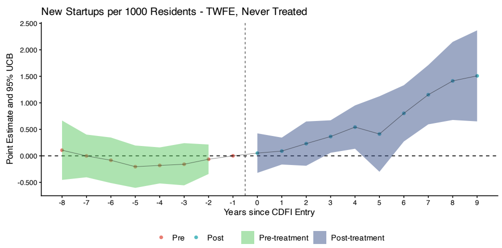
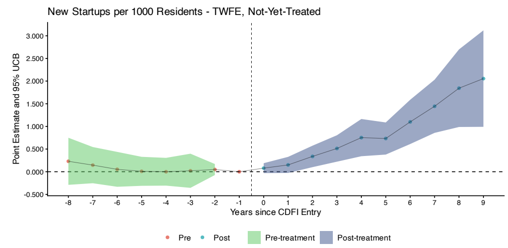


Figure 2: Propensity Scores - 1996 Cohort.

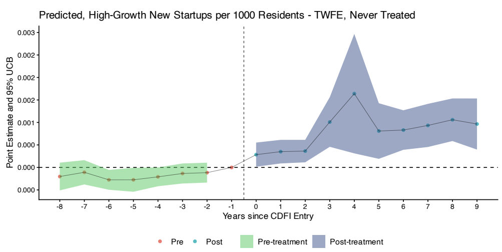
Figure 3: 1996 Cohort TWFE Results



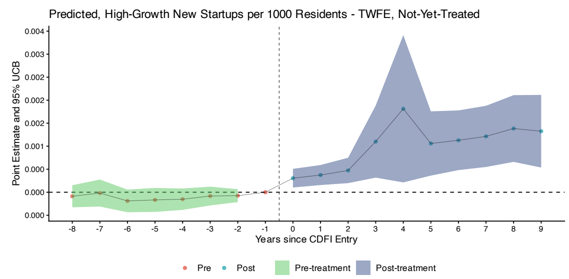
(a) Startup Formation Rate, NT



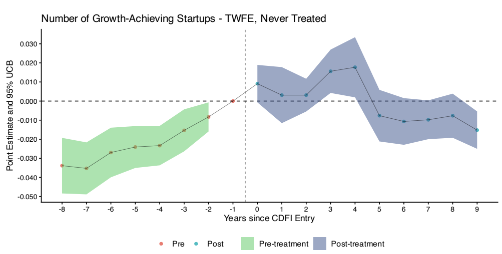
(b) Startup Formation Rate, NYT



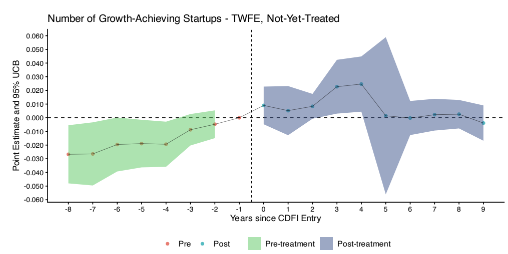
(c) Startup Quality Score, NT



(d) Startup Quality Score, NYT

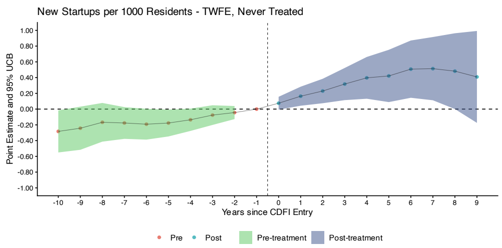


(e) No. Growth Events, NT

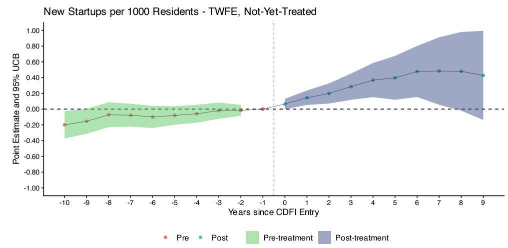


(f) No. Growth Events, NYT

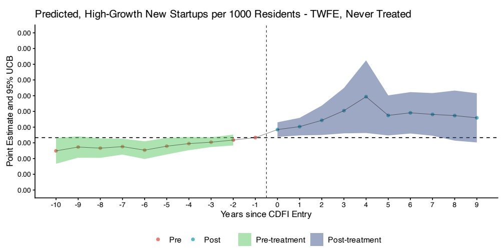
Figure 4: Full Sample, Cross-Cohort TWFE Results



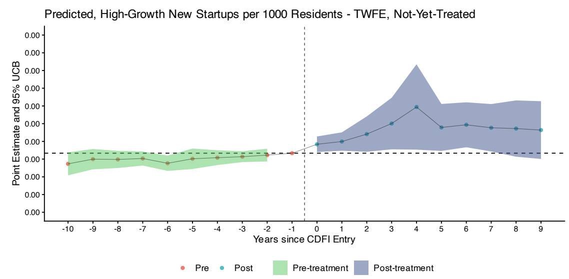
(a) Startup Formation Rate, NT



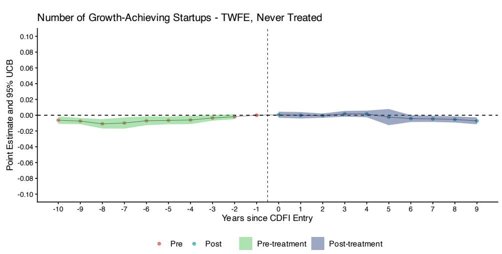
(b) Startup Formation Rate, NYT



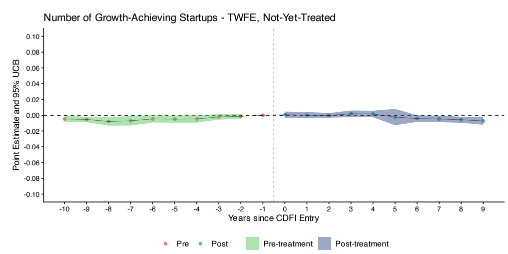
(c) Startup Quality Score, NT



(d) Startup Quality Score, NYT



(e) No. Growth Events, NT



(f) No. Growth Events, NYT

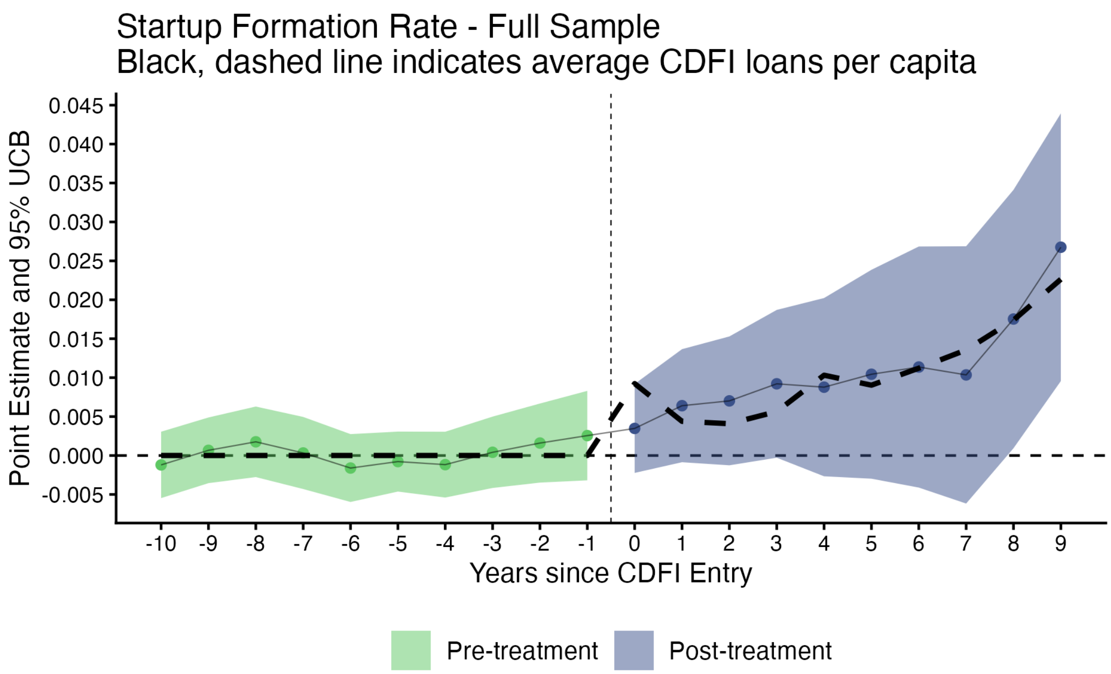


Figure 5: **Event-time effects of CDFI entry on Startup Formation Rate.** Notes: Coefficients relative to $k = -1$; 95% uniform bands; SEs clustered by state.

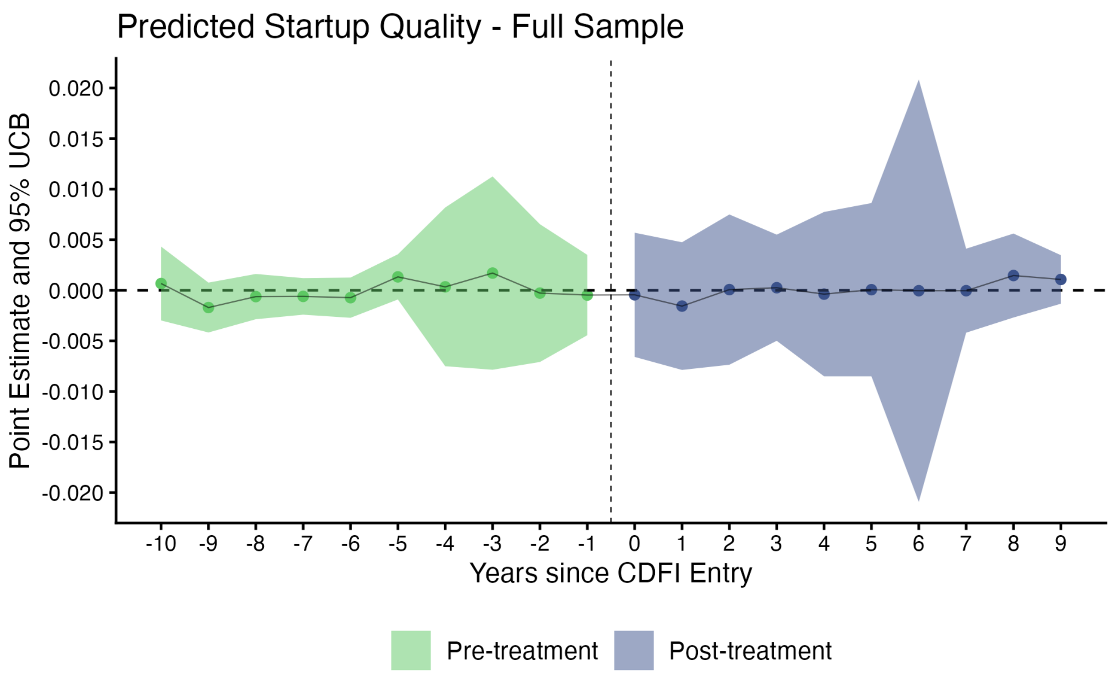


Figure 6: **Event-time effects of CDFI entry on Startup Quality Score.** Notes: Coefficients relative to $k = -1$; 95% uniform bands; SEs clustered by state.

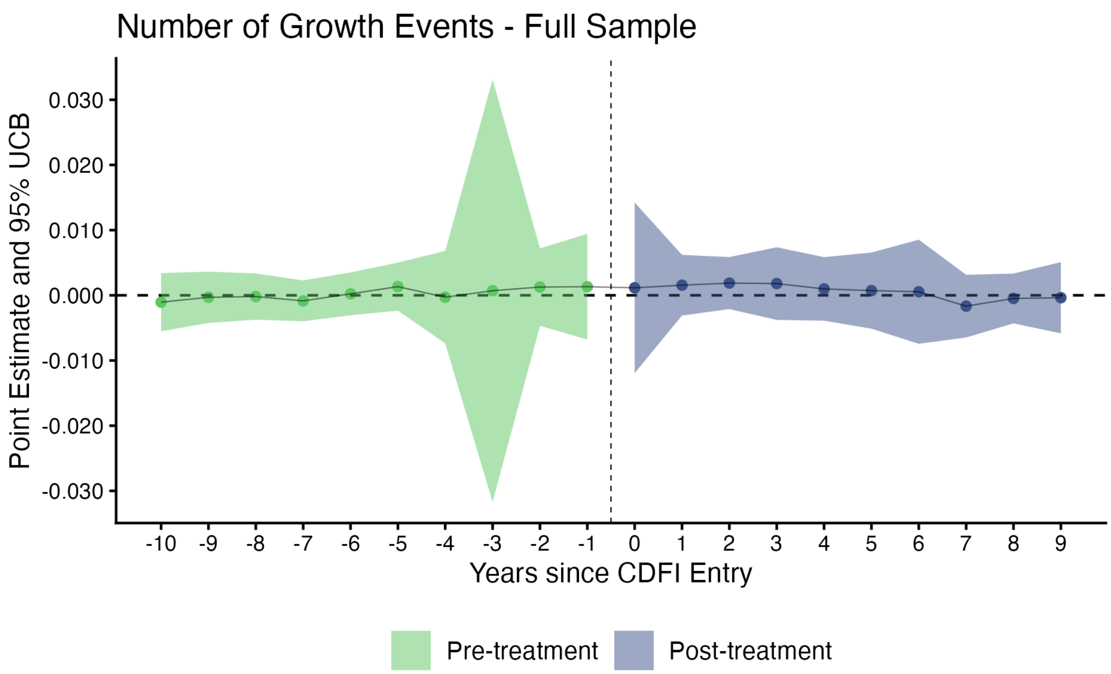


Figure 7: **Event-time effects of CDFI entry on Number of Growth Events.** Notes: Coefficients relative to $k = -1$; 95% uniform bands; SEs clustered by state.

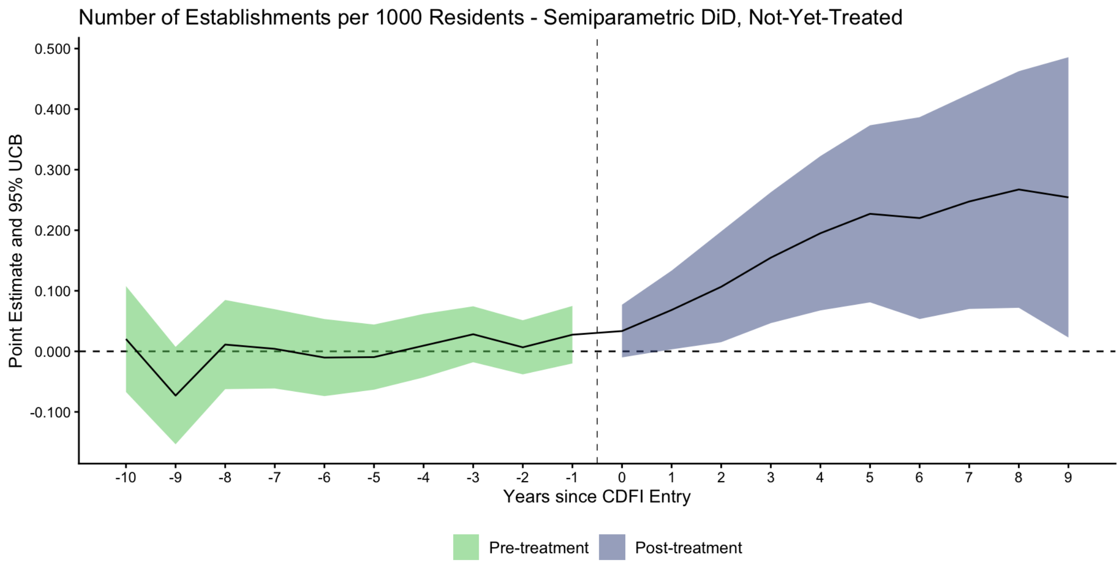


Figure 8: **Event-time effects of CDFI entry on Establishments per 1000 Residents.** Notes: Coefficients relative to $k = -1$; 95% uniform bands; SEs clustered by state.

Table 1: Composition of Treated Cohorts

Cohort	No. ZIP Codes	States Covered	Counties Covered	Share of Treated ZIPs (%)
1996	4,929	41	524	17.4
1997	2,748	41	358	9.7
1998	2,635	39	346	9.3
1999	2,275	44	371	8.0
2000	1,871	39	381	6.6
2001	2,297	40	384	8.1
2002	1,546	36	331	5.5
2003	2,192	42	448	7.8
2004	1,437	42	346	5.1
2005	1,489	39	331	5.3
2006	1,237	33	291	4.4
2007	682	34	189	2.4
2008	580	34	164	2.1
2009	502	29	123	1.8
2010	369	26	108	1.3
2011	246	22	82	0.9
2012	395	25	110	1.4
2013	362	24	98	1.3
2014	330	22	92	1.2
2015	158	18	43	0.6

Table 2: Summary Statistics by Treatment Status

Variable	(A) Earliest Treated (1996)	(B) All Treated	(C) Never Treated	(D) Not-yet Treated
Population Count	17,859	10,727	2,125	6,569
Share Hispanic (%)	15.2	8.8	4.7	5.8
Share Non-Hispanic White (%)	67.9	78.0	88.2	83.9
Share Non-Hispanic Black (%)	8.8	7.9	4.9	6.4
Share Income > $75k (%)	41.3	36.6	29.8	32.8
Unemployment Rate (%)	5.7	5.4	4.1	5.0
Poverty Rate (%)	15.0	14.8	14.0	14.9
Bank and Credit Union Lenders per 1,000 Residents	0.26	0.32	0.51	0.36
Percentage of ZIP Area Developed	40.6	26.3	7.5	16.8
Total New Startups per 1,000 Residents	3.28	3.41	1.93	2.75
IPO-Predicted Startups per 1,000 Residents	0.003	0.001	0.001	0.001

Table 3: Summary Statistics: Treated ZIPs With vs. Without CDFI Lending

Variable	**(A) Treated ZIPs with CDFI Lending**	**(B) Treated ZIPs without CDFI Lending**
Population Count	15,803	5,540
Share Hispanic (%)	11.0	6.6
Share Non-Hispanic White (%)	73.2	83.0
Share Non-Hispanic Black (%)	10.1	5.7
Share Income > $75k (%)	37.4	35.7
Unemployment Rate (%)	5.5	5.2
Poverty Rate (%)	15.4	14.1
Bank and Credit Union Lenders per 1,000 Residents	0.32	0.31
Percentage of ZIP Area Developed	32.1	20.3
CDFI Loans per 1,000 Residents	0.29	0.00
Total New Startups per 1,000 Residents	3.67	3.15
IPO-Predicted Startups per 1,000 Residents	0.002	0.001
Number of ZIP Codes	14,034	14,246

Table 4: CDFI Access (Full Sample)

	Startup Formation Rate		Startup Quality Score		No. Growth Events	
10-Year ATT (DiD Strategy)	0.11*	(0.03)	0.00	(0.02)	0.00	(0.01)
10-Year ATT (LOU Strategy)	0.12*	(0.04)	0.00	(0.02)	0.00	(0.02)
Bottom Quartile, White (DiD Strategy)	0.12*	(0.04)				
Top Quartile, White (DiD Strategy)	0.06	(0.05)				
Cluster			State			

Notes: * $p < 0.05$.

Table 5: Establishment Counts and Labor Market Measures (1996 Cohort)

	Establishments per 1K Residents		(Log) Average Wage		Employment-to-Population Ratio	
Full Sample ATT (DiD Strategy)	0.17*	(0.04)	0.00	(0.00)	0.0047	(0.0011)
Cluster			State			

Notes: * $p < 0.05$.