Taxes, Incentives, and Entrepreneurship: Evidence from the Universe of U.S. Startups

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November 23, 2025

Abstract

Each year, governments forgo billions in revenue in tax cuts and incentives aimed at fostering new businesses, yet their impact remains unclear. We provide the first comprehensive evidence on how local taxes and incentives influence startup activity, using restricted U.S. administrative data and difference-in-differences analyses of major tax and incentive reforms. Tax increases substantially reduce startups and their employment, whereas cuts yield symmetric positive effects. Corporate taxes exert the strongest influence, with large cuts boosting employer startups by nearly 5%. New incentives, especially R&D credits, enhance formation and curb exit. Employment effects operate through changes in the *quantity* of startups rather than *growth* within them. Tax policy changes induce virtually no cross-state firm relocation.

JEL Codes: H24, H25, J23, J24, L26

Keywords: startups, entrepreneurship, taxes, incentives, Comprehensive Startup Panel

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The authors are grateful to the Federal Statistical Research Data Center (FSRDC) in Berkeley, CA, for providing access to the microdata, and Alex Bell, Paige Clayton, Ryan Decker, Chris Esposito, Maryann Feldman, Lee Fleming, Jim Hines, Zachary Kroff, Jennifer Kuan, Francine Lafontaine, Cathy Liu, Jackson Mejia, Carlianne Patrick, Ben Rosa,Jagadeesh Sivadasan, Abraham Song, Ping Wang, as well as participants at the Price Center Conference on Entrepreneurship and Innovation (UCLA), the 6th SOLE/EALE/AASLE World Labor Conference, the 100th WEAI Annual Meeting, the 20th Economics Graduate Student Conference, and seminars at University of Michigan, Georgia State University, and University of Nevada-Reno for excellent comments. Frank Fossen thanks the Ewing Marion Kauffman Foundation for funding the research project "RG-202204-12283". The contents of this paper are solely the responsibility of the authors. Any views expressed are those of the authors and not those of the U.S. Census Bureau. The Census Bureau has reviewed this data product to ensure appropriate access, use, and disclosure avoidance protection of the confidential source data used to produce this product. This research was performed at a Federal Statistical Research Data Center under FSRDC Project Number 2936 (CBDRB-FY25-P2936-R11747 and CBDRB-FY25-P2936-R12020).

The entrepreneur shifts economic resources out of an area of lower and into an area of higher productivity and greater yield.

—Economist Jean-Baptiste Say (1767–1832)

1 Introduction

Entrepreneurs generate substantial positive externalities. Beyond raising consumer welfare and contributing tax revenue for public goods, entrepreneurs drive employment growth and wage competition in labor markets (Moretti, 2012; Haltiwanger et al., 2013; Decker et al., 2014; Christensen, 2015; Glaeser et al., 2015; Burton et al., 2018). Critically, because managerial span-of-control limits the size of individual firms, the *number* of entrepreneurs is a binding constraint on employment (Keren and Levhari, 1979; McAfee and McMillan, 1995; Beaudry et al., 2018). New firms also drive innovation, knowledge spillovers, and broader economic growth (e.g., Audretsch et al., 2006; Baumol and Strom, 2007; Kerr and Robert-Nicoud, 2020). Policies that affect startup formation are therefore particularly consequential.

Recognizing the benefits of entrepreneurship, governments forgo billions each year to encourage startup creation, growth, and survival (Bartik, 2020; Mejia, 2024). Governments pursue these goals primarily through tax policy, using two main approaches. First, they cut broad-based taxes—including those on property, sales, corporate income, and personal income. Second, they offer targeted tax incentives for specific activities—like those for job creation, investment, or research and development (R&D). Do these policies actually increase startup activity, or do they simply provide windfalls to firms that would have started anyway?

In this paper, we investigate which of these approaches increases startup formation and growth. We leverage newly available Census administrative records—the Comprehensive Startup Panel (CSP)—which covers the universe of U.S. startups and tracks them over time. The CSP includes all types of startups: those with and without employees, all legal forms of organization, all industries, and all locations in the United States.[1] This comprehensive coverage allows us to measure effects on both the extensive margin (entry and exit) and the intensive margin (employment growth within surviving firms).

We examine the effects of a comprehensive set of tax policies on startup outcomes: personal income taxes, corporate income taxes, property taxes, sales taxes, investment tax credits, job creation tax credits, R&D credits, and property tax abatements. Critically, we evaluate all policy changes using the same identification strategy and selection criteria. This approach enables direct comparisons across policies while avoiding

[1] Previously available administrative records, including the Business Dynamics Statistics (BDS) and Longitudinal Business Database (LBD), exclude non-employer startups, which represent the vast majority of new firms and create substantial employment in subsequent years. These data define the "startup year" as the first year it has employees.

the publication bias that can arise when studies selectively examine and report results. Our comprehensive framework clarifies which specific policies successfully foster startup activity.

To identify causal effects, we exploit large, discrete changes in tax policies across states and over time using difference-in-differences designs. We employ recent advances in difference-in-differences methodology (Callaway and Sant'Anna, 2021) to address well-documented biases in traditional two-way fixed effects models with staggered policy adoption. Our approach uses only not-yet-treated or never-treated jurisdictions as controls, avoiding contamination from units already affected by treatment. Event study analysis confirms parallel pre-trends between treatment and control groups, validating our identification strategy, and show that effects coincide with the timing of policy changes. We address concerns about correlated policy changes by controlling for other an array of contemporaneous policies, and we find our results are robust.

Tax increases significantly reduce startup formation and employment, while tax cuts tend to produce symmetric positive effects. This symmetry is particularly striking for corporate income taxes: a tax cut increases the number of employer startups by 4.7 percent and their employment by 3.9 percent, while a tax hike reduces employer startups by 3.1 percent.[2] In a typical state, a corporate tax cut translates to approximately [X thousand] additional firms and [Y thousand] new jobs. The symmetry in both magnitude and statistical significance supports a causal interpretation and is consistent with tax-induced changes in returns driving startup responses.

The effects vary substantially across tax types and startup organizational forms. Corporate income taxes show the strongest and most consistent effects on employer startups, while personal income cuts primarily influence *nonemployer* startups (increasing their formation by 2.1 percent following tax cuts). This pattern aligns with the legal structure of these firms: most nonemployers are sole proprietorships or partnerships filing under personal income tax, while incorporated employers face corporate income tax. Property tax cuts also significantly increase employer startups and their employment, potentially because employment usually requires office space. In contrast, sales taxes show weak and inconsistent effects on startup activity, likely because most startups have limited consumer-facing sales in their early years.

Taxes affect employment primarily through the extensive margin—increasing entry and reducing exit—rather than through employment growth within existing firms. Tax cuts significantly reduce exit rates, particularly for corporate and property taxes, suggesting that lower taxes help marginal firms survive. Analysis at the individual firm level using firm fixed-effects confirms no employment response within firms, indicating that the aggregate employment effects arise entirely from having more startups rather than from growth in existing ones.

[2] The corporate tax rate cuts in our analysis are 60 percent larger than the rate increases. When effects are normalized by the size of the tax change, the per-unit employment change is essentially identical.

Critically, we find no evidence that startups systematically relocate across state boundaries in response to tax changes. The estimated effects on interstate move-out rates are economically negligible (0.03-0.07 percentage points), even when statistically significant.[3] This implies that our measured effects on startup numbers represent genuine changes in entrepreneurial activity rather than spatial reallocation. The lack of relocation suggests that tax-rate changes generate real welfare effects through their impact on the total supply of entrepreneurs.

We next turn to examining targeted business incentives, which reveal a strikingly different pattern. Enacting new incentives significantly increases startup activity, but eliminating existing incentives has minimal negative effects. The introduction of new incentives increases employer startups by 4.2 percent and their employment by 5.2 percent, while also reducing exit rates by 0.5 percentage points. However, the removal of incentives shows no significant effects on employer formation or employment. One interpretation is that new incentives induce entry by marginal entrepreneurs who would not have started otherwise. In contrast, when longstanding incentives are removed, the affected firms have already become established and the removal represents a lost windfall rather than a threat to survival.

The effectiveness of incentives varies dramatically by type. R&D tax credits show particularly strong effects, increasing employer startups by 7.1 percent and their employment by 7.5 percent—substantially larger effects than any other policy we examine, including corporate income tax cuts. Notably, when R&D credits are eliminated, employer formation shows no significant decline, and exit rates actually decrease. This pattern suggests R&D credits may foster higher-quality or more committed ventures whose viability does not depend on the credit's permanent continuation. In contrast, property tax abatements show weak or even perverse effects, with new abatements reducing employer startups by 1.5 percent. Job creation and investment tax credits show modest positive effects when introduced but similarly minimal impacts when removed.

The asymmetric effects of incentives appear to operate primarily through the exit margin: new incentives consistently reduce exit rates across nearly all policy types, while removal of incentives does not significantly increase exit. This contrasts with tax effects, which operate more symmetrically through both entry and exit. The pattern suggests that new incentives may be particularly effective at helping marginal firms survive and may select for startups with greater employment. The asymmetry also has important policy implications: temporary incentives could potentially stimulate entrepreneurship without creating long-term fiscal commitments.

A substantial literature, discussed in the next section, examines how taxes affect entrepreneurship and

[3] We also find no evidence of stronger effects in border counties, where relocation costs would be lowest—the coefficients are actually somewhat smaller—further suggesting minimal cross-border reallocation.

how incentives influence firm behavior, with mixed findings across studies and policy types (Bruce et al., 2020; Gentry and Hubbard, 2000; Cullen and Gordon, 2007; Partridge et al., 2020; Fazio et al., 2020; Da Rin et al., 2011; Sapollnik and Swonder, 2025). Our analysis provides three key advantages over prior work. First, we leverage the universe of U.S. startups—including the 89 percent of new firms without employees excluded from standard datasets. Second, we evaluate all major tax and incentive policies using the same identification strategy, enabling direct comparisons of their relative efficacy. Third, we decompose effects across extensive and intensive margins and find no evidence of interstate relocation, indicating our results reflect genuine changes in entrepreneurial activity rather than spatial reallocation.

2 Related Literature

Our analysis builds on and extends three key branches of work examining the effects of tax and incentive policies on entrepreneurship. We advance this literature by providing the first comprehensive causal analysis that combines complete startup data—including the vast majority of new firms excluded from prior studies—with consistent identification across all major policy types.

2.1 2.1 Complete Startup Data

A persistent limitation of prior research has been incomplete data on startup activity. Most existing studies rely on small, non-random surveys or on administrative datasets that exclude nonemployer startups, which represent 89 percent of new firms and create substantial employment in subsequent years (Fairlie et al., 2023).

The exclusion of nonemployers is particularly consequential when examining personal income tax effects. Most nonemployers operate as sole proprietorships or partnerships subject to personal income taxation rather than corporate taxation (Giroud and Rauh, 2019). Prior studies that focus exclusively on employer firms may therefore understate or miss entirely the effects of personal income taxes on entrepreneurship. Our use of the Comprehensive Startup Panel allows us to observe all types of startups, enabling us to identify how different tax policies affect different organizational forms.

Beyond the employer-nonemployer distinction, prior work has faced challenges in properly measuring when a new firm begins. Standard administrative data define the "startup year" as the first year a firm has employees (Fairlie et al., 2023), which can significantly mistime actual founding dates for firms that begin as nonemployers before hiring their first employee. The CSP's comprehensive coverage addresses this measurement issue and allows us to track firms from true founding through their first eight years, capturing both extensive margin effects (entry and exit) and intensive margin responses (employment growth within surviving firms).

2.2 Comprehensive Policy Comparison

Prior research has typically examined individual tax or incentive policies in isolation, making it difficult to assess their relative effectiveness or to account for simultaneous policy changes. Studies of corporate income taxes have established negative effects on various entrepreneurship measures (Da Rin et al., 2011; Djankov et al., 2010; Belitski et al., 2016), with Da Rin et al. (2011) finding that corporate tax cuts increase entrepreneurial entry across 17 European countries. Curtis and Decker (2018) find that corporate tax increases reduce startup employment by 3.7 percent, with startups responding more strongly than overall employment. Most recently, Sapollnik and Swonder (2025) estimate a five-year elasticity of employer business entry with respect to corporate tax cuts of 2.7.

Research on personal income taxes presents more mixed findings, consistent with the theoretical ambiguity surrounding their effect. Gentry and Hubbard (2000) argue that progressive income taxes function as a "success tax" that discourages entrepreneurial entry, while Cullen and Gordon (2007) demonstrate that higher personal tax rates can increase risk-taking by providing implicit insurance. Curtis and Decker (2018) find no detectable effect of personal income taxes on startup activity. The difficulty in reconciling these findings may stem partly from data limitations—prior studies focusing on employer firms or self-employment cannot observe the full range of organizational responses to personal income tax changes.

Less research has examined property and sales taxes, despite their importance for state and local revenues. Bartik (1985) and Bartik (1989) find negative effects of property taxes on small business formation, while Chen et al. (2023) argues that property taxes may be particularly burdensome for liquidity-constrained entrepreneurs. However, prior work has not separately estimated effects for employer versus nonemployer startups, despite their likely differential exposure to property taxes through their use of commercial versus residential space.

Turning to business incentives, research presents an even more fragmented picture. Partridge et al. (2020) find that overall incentives have a statistically significant negative relationship with startup rates, suggesting crowding-out effects that offset any direct stimulus. In contrast, Fazio et al. (2020) find that R&D tax credits positively affect both the quantity and quality of entrepreneurship, while investment tax credits show no positive impact and may even reduce high-growth startup formation. Leonard et al. (2020) finds that property tax abatements facilitate short-term employment growth that often diminishes over time.

The fragmented nature of this literature creates two key challenges. First, when different policies are studied separately using different data, methods, and time periods, direct comparisons of policy effectiveness are difficult. Second, because states often adjust multiple policies simultaneously, studies that omit correlated policy changes risk biased estimates. We address both challenges by evaluating all major tax and incentive policies within a unified framework, using the same data, identification strategy, and sample period. This approach not only enables direct comparisons of policy effectiveness but also reduces publication bias that can arise when studies selectively examine and report results for individual policies (Ioannidis et al., 2017).

2.3 Causal Identification and Policy Mechanisms

Establishing causal effects of tax policies on entrepreneurship has proven difficult due to endogeneity concerns. States may adjust tax rates in response to economic conditions or in anticipation of future trends, creating bias in standard panel regressions (Ljungqvist and Smolyansky, 2014). Early influential studies relied on cross-sectional comparisons that struggle to disentangle causal effects from unobserved factors (Glaeser and Kerr, 2009; Delgado et al., 2010; Chatterji et al., 2014). More recent work has employed panel methods, but these approaches are now known to produce biased estimates in the presence of staggered treatment timing and heterogeneous treatment effects (Goodman-Bacon, 2021).

We employ the Callaway and Sant'Anna (2021) difference-in-differences estimator, which addresses these well-documented biases by using only not-yet-treated or never-treated units as controls. We complement this with event study analyses to verify parallel pre-trends and assess dynamic responses. Sapollnik and Swonder (2025) similarly employ modern difference-in-differences methods, though their analysis is limited to employer startups and does not examine incentives or the full range of tax types. Curtis and Decker (2018) use border-county comparisons to address endogeneity concerns, finding robust corporate tax effects but limited evidence on other tax types. Both Sapollnik and Swonder (2025) and Curtis and Decker (2018) find strong effects for corporate taxes but not personal income taxes.

Beyond establishing causal effects, understanding the mechanisms through which policies operate is critical for policy design. Young firms have been shown to be particularly sensitive to various shocks including credit conditions (Fort et al., 2013), demand shocks (Adelino et al., 2017), and productivity shocks (Decker et al., 2020). Our analysis extends this literature by decomposing policy effects across extensive and intensive margins, revealing whether effects operate primarily through changes in entry and exit rates or through employment growth within surviving firms. This decomposition provides insight into whether policies affect the decision to start a business, the ability of marginal firms to survive, or the employment growth within startups.

A related question concerns geographic mobility: do startup responses reflect genuine changes in entrepreneurial activity, or merely relocation across jurisdictions? Rohlin et al. (2014) find evidence of taxmotivated cross-border establishment location decisions. Sapollnik and Swonder (2025) similarly find little evidence of cross-border spillovers in response to corporate tax changes. We contribute to this debate by directly examining startup relocation rates and find evidence of very limited interstate migration in response to policy changes, indicating that our measured effects represent genuine changes in total startup activity rather than spatial reallocation.

Our comprehensive approach clarifies several outstanding questions in this literature. First, by observing both employer and nonemployer startups, we can assess whether personal income taxes affect entrepreneurship even if effects are not detectable in employer-only samples. Second, by examining multiple policies simultaneously, we can determine whether aggregate tax measures mask heterogeneous effects across tax types. Third, by decomposing effects across margins and examining relocation, we provide insight into the mechanisms through which tax and incentive policies affect startup activity. Finally, by applying consistent methods across all policies, we can directly compare their relative effectiveness—information essential for policymakers seeking to allocate resources efficiently across different entrepreneurship promotion strategies.

By revealing which policies are most effective and through which channels they operate, our findings provide actionable guidance for governments spending billions annually on these programs.

3 Data and Measures

We combine newly available data on the universe of U.S. startups with comprehensive measures of state and local tax policies to examine how these policies affect entrepreneurial activity. Our entrepreneurial outcomes come from the Comprehensive Startup Panel (CSP), which tracks all business startups in their first eight years. Tax and incentive data are sourced from the Panel Database on Incentives and Taxes (PDIT) and from the Tax Foundation. We supplement these with demographic and economic controls from the Bureau of Economic Analysis, the American Community Survey, and U.S. Census Bureau intercensal datasets. Our analysis covers 33 states representing over 90 percent of U.S. economic activity from 2001 to 2015.

3.1 Measurement of Entrepreneurial Outcomes

Our analysis leverages the Comprehensive Startup Panel (CSP), which provides the first administrative panel data on the *universe* of U.S. business startups—including both employer and nonemployer firms—tracked from founding through their first eight years (Fairlie et al., 2023). This comprehensive coverage addresses a critical limitation of prior entrepreneurship research: existing datasets either focus exclusively on employer firms (excluding 89 percent of startups) or define the startup year as the first year with employees, significantly mistiming founding dates for firms that begin as nonemployers.

The CSP integrates two Census Bureau data sources: the Longitudinal Business Database (LBD), covering all non-farm employer establishments starting in 1976, and the Integrated Longitudinal Business Database (ILBD), covering nonemployer establishments with quinquennial data from 1977 to 1992 and annual data thereafter. By linking these universes, the CSP enables us to track nonemployer startups and observe when they transition to employer status—critical for understanding the full spectrum of entrepreneurial responses to policy changes.[4] The CSP includes detailed information on businesses, such as geographic location, establishment year, employment dynamics, payroll distributions, legal form, industry, and closure.

We analyze five startup outcomes at the county-year level. Our primary extensive margin measures are the counts of new nonemployer startups (in their first year) and employer startups (in their first eight years). To assess survival and geographic mobility, we also measure startup exit rates (the share of startups that cease operations) and interstate move-out rates (the share of startups that relocate to a different state). For intensive margin analysis, we examine total employment across all startups in their first eight years. These outcomes allow us to decompose policy effects across different margins of adjustment and distinguish between effects on firm formation, survival, and growth within surviving firms.

3.2 Measurement of Business Taxation and Incentives

We examine two categories of policies: taxes (corporate income, personal income, property, and sales) and targeted incentives (job creation credits, investment credits, R&D credits, and property tax abatements). Our comprehensive approach allows direct comparison of policy effectiveness across all major instruments used by states to foster entrepreneurship.

Tax and incentive data come primarily from the Panel Database on Incentives and Taxes (PDIT) produced by the Upjohn Institute for Employment Research (Bartik, 2017). The PDIT provides state-level measures of taxes and incentives for a hypothetical new firm across 33 states from 1990 to 2015, covering over 90 percent of U.S. output and population.[5] As shown in Figure A1 in Appendix A, the included states provide comprehensive geographic coverage of major U.S. economic regions.

The PDIT simulates tax liabilities and available incentives by modeling the establishment of a new

[4]The CSP identifies startups using an algorithm that prioritizes the oldest establishments, resolving ties by considering employment, payroll, and random selection among equals. The data allow for accurate tracking of business entities as they evolve over time, including when they relocate across state borders or undergo ownership changes. A business enters a startup cohort only if it has not appeared in either the nonemployer or employer universes in the previous seven years. See Fairlie et al. (2023) for further details.

[5]Excluded states are Alaska, Arkansas, Delaware, Hawaii, Idaho, Kansas, Maine, Mississippi, Montana, New Hampshire, North Dakota, Oklahoma, Rhode Island, South Dakota, Utah, Vermont, and West Virginia—predominantly small, mountain, or non-contiguous states with populations generally below 2 million.

business in 47 cities across the 33 states, projecting taxes and incentives over the first 20 years of operation at constant operating scale.[6] By combining simulated firm balance sheet data with state and local business tax rates and incentive eligibility rules, the PDIT calculates standardized measures applicable to business entry across states and industries. This standardization represents a key advantage over alternative data sources, which typically focus on either aggregate budget allocations, individual subsidy deals, or qualitative rankings rather than actual projected tax liabilities.[7]

The PDIT's total tax measure includes state-level property taxes, sales taxes, and corporate income taxes. Local-specific tax types, such as city-level corporate income taxes and public utility taxes, are excluded from the total taxes calculation to ensure comparability across states. The total incentive measure includes property tax abatements, job creation tax credits, investment tax credits, R&D tax credits, and customized job training subsidies.[8] We use both the aggregate measures (total taxes and total incentives) and the individual policy components in our analysis to assess overall effects as well as heterogeneity across policy types.

To supplement the PDIT, we add state personal income tax rates for the highest tax brackets from the Tax Foundation (Tax Foundation, 2021), as shown in Figure A2 in Appendix A. The Tax Foundation provides comprehensive state-level tax policy data for all states. We focus on state-level marginal rates, excluding local personal income taxes—imposed by approximately ten states at the county or city level—to ensure consistency across states.

3.3 Measurement of Control Variables

We include demographic and economic controls shown in prior literature to explain entrepreneurship. County-level population data are obtained from the Bureau of Economic Analysis, part of the U.S. Department of Commerce. Demographic variables—including shares of the working-age population (ages 25-64), educational attainment (high school graduates and college graduates), and racial and ethnic composition (shares of female, African American, Asian, and Hispanic populations)—are sourced from the American Community Survey (ACS) for the years 2010 to 2021.

The ACS does not provide detailed demographic data consistently for years prior to 2010. To ensure complete coverage of our 2001-2015 analysis period, we supplement the demographic data for 2001-2009

[6]Present values are calculated using a 12 percent discount rate. Industry-specific measures are aggregated to the state level using value-added weights.

[7]The PDIT's extensive coverage sets it apart from other datasets such as the Community and Economic Research (C2ER) database, the Good Jobs First Subsidy Tracker, and the Tax Foundation's State Business Tax Climate Index. C2ER primarily focuses on state budget allocations for economic development and lacks detailed tax and incentive information. Good Jobs First emphasizes individual subsidy deals without providing long-term projections. The Tax Foundation's index ranks tax environments but does not simulate actual business tax liabilities over time.

[8]We do not analyze job training subsidies separately due to limited time variation in this policy instrument.

using the County Intercensal Datasets provided by the U.S. Census Bureau for demographic shares and the USDA Economic Research Service for educational attainment variables. We also control for the average age of startups in the county and its square when estimating outcomes for existing startups (all outcomes except the number of new nonemployers).

4 Empirical Strategy

4.1 Identification Approach

To identify causal effects of tax and incentive policies on startup outcomes, we face two key empirical challenges. First, states may adjust policies endogenously in response to economic conditions or in anticipation of future trends, creating bias in standard panel regressions. Second, with staggered policy adoption across states and over time, and potentially heterogeneous treatment effects, standard two-way fixed effects estimators are known to produce biased estimates (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021).

We address these challenges using the Callaway and Sant'Anna (2021) difference-in-differences estimator, which uses only not-yet-treated or never-treated units as controls, avoiding the contamination and negative weighting problems that arise when already-treated units serve as comparisons. We focus our analysis on large, discrete policy changes—defined as year-over-year changes exceeding the 95th percentile (for increases) or falling below the 5th percentile (for decreases) of each policy's distribution.[9] We estimate the effects of increases and decreases separately to allow for potential asymmetries. This approach isolates the largest available policy shocks, which maximizes statistical power to detect startup responses and helps separate policy effects from slow-moving trends or gradual changes in economic conditions. As robustness checks, we also estimate models using the 90th (10th) and 99th (1st) percentiles as alternative thresholds; results are similar and reported in the Appendix.

4.2 Difference-in-Differences Specification

Our primary specification examines outcomes at the county-year level, which captures both extensive margin effects (firm entry and exit) and intensive margin effects (employment growth within firms). To isolate intensive margin responses, we also estimate specifications at the firm-year level with firm fixed effects.

Our baseline county-level specification examines the average treatment effect of a policy regime change

[9] When the 95th (5th) percentile equals the median—which can occur when many states maintain constant policy levels—we use the 99th (1st) percentile instead. If the 99th (1st) percentile also equals the median, we do not analyze increases (decreases) for that policy variable.

on startup outcomes in the post-treatment period:

$$Y_{ct} = \beta_0 + \beta_1 \, Treat_{ct} + \beta_2 \, X_{ct} + \gamma_c + \tau_t + \epsilon_{ct} \tag{1}$$

The dependent variable Y_{ct} represents one of five measures of startup outcomes in county c at time t. The first outcome is the log count of new nonemployer startups in their first year, which captures market entry. The remaining four outcomes are measured for startups within their first eight years of existence: the log count of employer startups, the log of total startup employment, the exit rate (the share of startups that cease operations), and the interstate move-out rate (the share of startups that relocate to a different state).

The key explanatory variable $Treat_{ct}$ is a dummy variable equal to 1 for observations in the treatment group after counties experienced either a large increase or a large decrease in a tax or incentive, and 0 otherwise. The coefficient of interest, β_1, captures the causal effect of the policy change on the startup outcome, under the identifying assumption of parallel trends between treatment and control groups in the absence of treatment.

The control vector X_{ct} includes the logarithm of total county population, the share of the working-age population, shares of high school and college graduates, and demographic shares of the female, African American, Asian, and Hispanic populations. When estimating outcomes for existing startups (all outcomes except the number of new nonemployers), we also control for the average age of startups in the county and its square to account for compositional changes in the startup population. County fixed effects (γ_c) capture time-invariant county characteristics, while year fixed effects (τ_t) control for common shocks affecting all counties.

We find that different policy events tend to be uncorrelated with one another across states and time—for instance, increases in incentives typically do not coincide with tax cuts. More importantly, when we explicitly control for other contemporaneous policy changes in robustness checks, the estimates remain essentially unchanged, suggesting that our single-policy specifications successfully isolate the effects of each policy type.

4.3 Event Study Specification

To assess pre-treatment parallel trends and examine dynamic treatment effects, we estimate event study models that quantify how policy effects evolve over time relative to the year of policy change:

$$Y_{ct} = \beta_0 + \sum_{k=-5}^{10} \beta_k \, D_{ct}^k + \beta \, X_{ct} + \gamma_c + \tau_t + \epsilon_{ct} \tag{2}$$

In this specification, D_{ct}^k is a set of dummy variables for each year k relative to the year of substantial policy change, with $k = 0$ representing the event year itself. The coefficients β_k capture the differential time-specific effects between treatment and control groups for each year relative to the event, extending from 5 years before to 10 years after the policy change. Event time $k = -6$ is included in the sample and serves as the omitted reference category, so all coefficients are interpreted relative to this baseline period. The fixed effects γ_c and τ_t continue to control for unobserved heterogeneity across counties and calendar time, while X_{ct} includes the same control variables as in equation (1).

The event study specification serves two purposes. First, it provides a test of the parallel trends assumption by examining whether $\beta_k \approx 0$ for $k < 0$. Significant pre-treatment coefficients would suggest that treatment and control groups followed different trends prior to policy changes, calling into question the validity of the identifying assumptions. Second, the post-treatment coefficients (β_k for $k \geq 0$) reveal the dynamic pattern of policy effects, showing whether effects emerge immediately or build over time, and whether they persist or fade as startups age.

4.4 Implementation Details

We implement the Callaway and Sant'Anna (2021) estimator using the doubly-robust approach based on stabilized inverse probability weighting and outcome regression adjustment. The inverse probability weighting reweights control observations to mirror the covariate distribution of the treatment group, improving comparability by assigning higher weights to control units with characteristics similar to treated units. The doubly-robust property ensures that our estimates remain consistent if either the propensity score model (used for weighting) or the outcome regression model is correctly specified, providing two independent paths to consistent estimation and making our results more robust to potential model misspecification.

The sample for each treatment-control comparison is trimmed to include up to 6 pre-treatment years and 10 post-treatment years, balancing the desire for long panels against the need to maintain sufficient sample sizes as cohorts age. Standard errors are calculated using the multiplicative Wild Bootstrap procedure with 1,000 repetitions and are clustered at the state level to account for potential correlation in outcomes within states over time.

To verify that our treatment definition captures meaningful policy variation, we examine the relationship between treatment indicators (large policy increases or decreases) and actual policy levels. Event study analyses confirm that treated units experience sharp, statistically significant changes in policy levels at the time of treatment, with stable pre-treatment trends and no evidence of anticipatory changes. These patterns, shown in Figures 1 (for taxes) and 3 (for incentives), confirm that our treatment classification successfully identifies economically meaningful policy shifts that coincide with the designated treatment timing.

5 Empirical Results

5.1 Descriptive Statistics

Covering the universe of startups in the United States, the CSP includes over 100 million startups and more than 850 million startup-year observations (Fairlie et al., 2023). On average, it records approximately 4.1 million new startups annually, of which 3.7 million are nonemployer startups, representing 89% of all new firms. Table 1 (Panel A) presents summary statistics for the startups in our analysis sample covering 2001–2015, which includes 201.1 million startup-year observations. Average employment is 1.4 workers per startup, consistent with the small scale and predominance of nonemployer startups in the United States. Changes in employment and transitions from nonemployer to employer status are low. Exit rates are high, with 30 percent of startups exiting each year, indicating substantial turnover. The share of startups moving out of a county into a different state averages 1.2 percent per year, while the share of startups in a county that moved in from a different state averages 2.3 percent per year.[10]

The tax and incentive variables are reported in Panel B. The average total state-level tax burden is 4.96% of value-added, with property tax averaging 2.35%, sales tax at 1.60%, and corporate income tax at 1.01%, based on the PDIT data from 2001 to 2015.[11] Additionally, state top marginal PIT rates obtained from the Tax Foundation average 5.23% of the income tax base. Overall incentives average 0.75% of value-added, with property tax abatements at 0.26%, job creation tax credits at 0.24%, investment tax credits at 0.16%, and R&D credits at 0.04%.

Panel C reports startup outcome and control variables at the county level across the 33 states included in the PDIT from 2001 to 2015. The averages across counties are 5,600 for total startups and 8,000 for total employment at these startups (numbers are rounded for Census disclosure). Nonemployer startups, including sole proprietorships and microenterprises, average 4,700 per county, while employer startups average 900 per county. The annual transition rate from nonemployer to employer is 0.9% and the exit rate is 29.9%. These numbers differ slightly from Panel A because Panel C shows unweighted county-level averages.

Panel C also reports control variables capturing demographic and economic characteristics at the county level. The average age of startups in the CSP is 2.35 years, with a large proportion being newly established: 29.06% of firms are in their first year, 16.59% are one year old, and only 5.84% are eight years old, the final year firms are defined as startups in the CSP.

[10] These rates differ because they are calculated as shares of the total startup population in each county, and counties vary substantially in size. Additionally, our sample includes only 33 states, so startups moving from our sample states to excluded states (or vice versa) create an imbalance between observed move-outs and move-ins.

[11] The PDIT data for taxes and incentives are expressed as a proportion of value-added for a typical new firm in a particular state.

Table 1: Descriptive Statistics

Panel A: Startup-Level Variables	**Mean**	**Standard Deviation**
Employment	1.37	80.44
ΔEmployment	0.04	62.6
Transition from Nonemployer to Employer (%)	0.83	0.09
Exit Rate (%)	30.15	0.46
Move-out Rate (%)	1.21	0.15
Move-in Rate (%)	2.32	0.11
Firm Age	2.06	1.97
Panel B: Tax and Incentive Policy Variables	**Mean**	**Standard Deviation**
Total Taxes (%)	4.96	0.96
Total Incentives (%)	0.75	0.57
Property Tax (%)	2.35	0.75
Sales Tax (%)	1.60	0.67
Corporate Income Tax (%)	1.01	0.36
State Top Personal Income Tax Rate (%)	5.23	2.82
Job Creation Tax Credits (%)	0.24	0.27
Investment Tax Credits (%)	0.16	0.28
Research and Development Credits (%)	0.04	0.05
Property Tax Abatement (%)	0.26	0.36
Panel C: County-Level Variables	**Mean**	**Standard Deviation**
Total Employment	8,000	30,500
Nonemployer Startups	4,700	17,500
Employer Startups	900	3,300
Total Startups	5,600	21,000
Transition from Nonemployer to Employer (%)	0.86	0.62
Exit Rate (%)	29.85	4.68
Move-out Rate (%)	0.97	0.71
Startup Age	2.35	0.46
Startup Age = 0	29.06	4.70
Startup Age = 1	16.59	2.98
Startup Age = 2	13.13	2.62
Startup Age = 3	11.43	2.58
Startup Age = 4	9.36	3.10
Startup Age = 5	7.87	3.36
Startup Age = 6	6.71	3.50
Startup Age = 7	5.84	3.60
Ln(Population)	10.48	1.44
High-school Graduate (%)	81.20	8.17
College Graduate (%)	18.34	8.77
Working-age (%)	51.72	3.28
Female (%)	50.17	2.23
African American (%)	9.45	14.11
Asian (%)	1.15	2.19
Hispanic (%)	8.68	14.15

Panel A reports startup-level variables from the CSP. Panel B reports county-level tax and incentive policy variables from the PDIT and the Tax Foundation. Panel C reports entrepreneurial outcomes and control variables at the county level. Some numbers are rounded for disclosure.

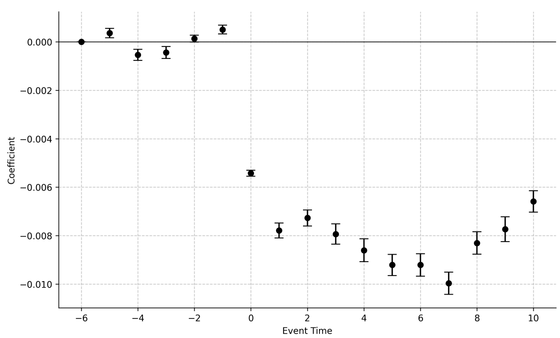
Main data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

5.2 The Effects of Tax Rate Reforms

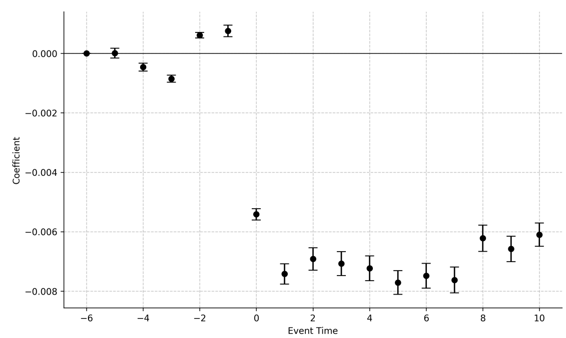
We find that tax policy significantly affects startup formation and growth, with substantial heterogeneity across tax types and firm types. Tax increases consistently reduce entrepreneurial activity, while tax cuts typically show symmetric effects—stimulating employment growth and increasing firm entry. We begin by examining aggregate tax effects before decomposing results into specific tax instruments.

Figure 1 confirms that our treatment definition captures meaningful policy variation: treated counties experience sharp declines in tax levels at event time zero, with stable pre-treatment trends and no anticipatory effects. Notably, the magnitude and persistence of the first-stage effects are more pronounced for tax cuts than for tax increases.

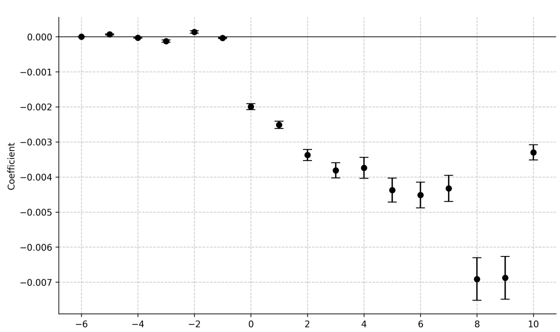
Figure 1: First Stage: Effects of Tax Cuts on Tax Levels



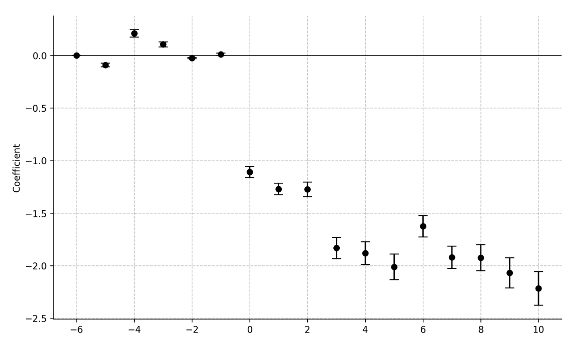
(a) Total Tax Cuts



(b) Property Tax Cuts



(c) Corporate Income Tax Cuts



(d) Personal Income Tax Cuts

Notes: Event-study estimates showing the effects of tax cuts on corresponding tax levels. Event time 0 denotes the year of the tax cut. *Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

Total Taxes. Panel A of Table 2 reveals that substantial increases in total taxes significantly reduce entrepreneurial activity. A large tax increase—corresponding to a 0.054 percentage point rise in taxes as a share of firm value added (approximately 11 percent of the mean)—reduces nonemployer startups by 2.91

percent and employer startups by 5.46 percent. The effect on total startup employment is somewhat larger at 6.40 percent, implying total tax elasticities of –0.27 for nonemployer startups, –0.50 for employer startups, and –0.59 for employment. These elasticities indicate that employer startups and startup employment are roughly twice as responsive to tax changes as nonemployer startups.

Importantly, tax increases operate on employment primarily through the extensive margin of entry: we find no significant effects on exit rates or interstate relocation, suggesting that higher taxes suppress new venture formation rather than triggering closures or geographic reallocation. Event study plots in Figure A3 confirm parallel pre-trends and show that effects emerge immediately following the tax increase at event time zero.

Table 2: Effect of Large Tax Policy Increases on Startup Outcomes

	(1) Policy	(2) ln(Nonemployers)	(3) ln(Employers)	(4) ln(Employment)	(5) Exit Rate	(6) Move-out Rate
Panel A: Total Taxes						
$Treat_{ct}$	0.0054***	-0.0291***	-0.0546***	-0.0640***	0.0019	-0.0003
	(0.000198)	(0.0073)	(0.0095)	(0.0162)	(0.0057)	(0.0002)
No. of Obs.	34,500	34,500	34,500	34,500	34,500	34,500
Panel B: Property Tax						
$Treat_{ct}$	0.0067***	0.0155	-0.0057	0.0027	-0.0017	-0.0002
	(0.0002)	(0.0150)	(0.0062)	(0.0177)	(0.0014)	(0.0002)
No. of Obs.	34,500	34,500	34,500	34,500	34,500	34,500
Panel C: Corporate Income Tax						
$Treat_{ct}$	0.0022***	-0.0248***	-0.0313***	-0.0106	-0.0013	0.0003
	(0.0001)	(0.0049)	(0.0062)	(0.0159)	(0.0009)	(0.0002)
No. of Obs.	36,000	36,000	36,000	36,000	36,000	36,000
Panel D: Personal Income Tax State						
$Treat_{ct}$	1.9180***	-0.0301	-0.0034	-0.0130	-0.0009	-0.0003
	(0.1220)	(0.0284)	(0.0461)	(0.0275)	(0.0020)	(0.0008)
No. of Obs.	65,000	65,000	65,000	65,000	65,000	65,000
Panel E: Sales Tax						
$Treat_{ct}$	0.0020***	0.0134	-0.0278**	-0.0169	0.0024	0.0003
	(0.0001)	(0.0095)	(0.0125)	(0.0185)	(0.0017)	(0.0003)
No. of Obs.	35,000	35,000	35,000	35,000	35,000	35,000

This table shows the effects of large tax policy increases on startup outcomes at the county level. The estimates are obtained using the DID method by Callaway and Sant'Anna (2021).

A large increase generally refers to the top 5% of the distribution of tax policy changes. If the 95th percentile was equal to the median (usually zero), the top 1% were used instead. Sales tax, property tax, and corporate income tax are sourced from the Upjohn dataset. Personal income tax state is the top marginal rate in the state, sourced from the Tax Foundation.

The dependent variables are listed at the top. When ln(Nonemployers) is the dependent variable, only startups in their first year (year 0) in the CSP are included in the sample before aggregating to the county level. This represents the number of newly founded nonemployers. For all other dependent variables, the sample includes all CSP startups (years 0-7) before aggregating to the county level. For example, ln(Employment) is the total employment of startups within their first 8 years in a county.

Pre-treatment control variables include firm age shares 0-7 (except when ln(Nonemployers) is the dependent variable), ln(Population), percentage of individuals aged 25 to 64, high school graduation percentage, college graduation percentage, female percentage, African American percentage, Asian percentage, and Hispanic percentage.

Standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

Effects of Tax Cuts. We test whether tax cuts generate symmetric (opposite-signed) effects of similar magnitude. Large total tax cuts increase employment by 4.55 percent (Figure A4b) and reduce exit rates, consistent with symmetric responses. However, we find no significant positive effect on employer startup formation, and—unexpectedly—tax cuts are associated with somewhat *lower* nonemployer startups (Figure A4a). This pattern becomes clearer when we decompose total taxes by type: the negative effect on nonemployers is driven primarily by property tax cuts (discussed below), which appear to shift the composition of entry toward employer firms. Tax cuts may enable more entrepreneurs to start directly as employers rather than beginning as solo operations, reducing measured nonemployer entry while increasing employment at the same time.

Table 3: Effect of Large Tax Policy Decreases on Startup Outcomes

	(1) Policy	(2) ln(Nonemployers)	(3) ln(Employers)	(4) ln(Employment)	(5) Exit Rate	(6) Move-out Rate
Panel A: Total Taxes						
$Treat_{ct}$	-0.0081***	-0.0149**	0.0117	0.0455**	-0.0036**	0.0003*
	(0.000164)	(0.0066)	(0.0083)	(0.0199)	(0.0015)	(0.0002)
No. of Obs.	36,500	36,500	36,500	36,500	36,500	36,500
Panel B: Property Tax						
$Treat_{ct}$	-0.0069***	-0.0136**	0.0200**	0.0402**	-0.0053**	0.0006*
	(0.0002)	(0.0059)	(0.0078)	(0.0179)	(0.0018)	(0.0003)
No. of Obs.	36,500	36,500	36,500	36,500	36,500	36,500
Panel C: Corporate Income Tax						
$Treat_{ct}$	-0.0036***	0.0329***	0.0471***	0.0391**	-0.0059**	-0.0007*
	(0.0001)	(0.0065)	(0.0082)	(0.0233)	(0.0024)	(0.0003)
No. of Obs.	33,000	33,000	33,000	33,000	33,000	33,000
Panel D: Personal Income Tax State						
$Treat_{ct}$	-1.6410***	0.0210***	0.0196**	0.0497**	-0.0002	-0.0001
	(0.0431)	(0.0051)	(0.0076)	(0.0210)	(0.0010)	(0.0002)
No. of Obs.	55,000	55,000	55,000	55,000	55,000	55,000
Panel E: Sales Tax						
$Treat_{ct}$	-0.0002**	0.0177	0.0172	-0.0023	0.0048	-0.0002
	(0.0001)	(0.0061)	(0.0081)	(0.0162)	(0.0010)	(0.0002)
No. of Obs.	35,500	35,500	35,500	35,500	35,500	35,500

This table shows the effects of large tax policy decreases on startup outcomes at the county level. The estimates are obtained using the DID method by Callaway and Sant'Anna (2021).

A large decrease generally refers to the bottom 5% of the distribution of tax policy changes. If the 5th percentile was equal to the median (usually zero), the bottom 1% was used instead. Sales tax, property tax and corporate income tax are sourced from the Upjohn dataset. Personal income tax state is the top marginal rate in the state, sourced from the Tax Foundation.

The dependent variables are listed at the top. When ln(Nonemployers) is the dependent variable, only startups in their first year (year 0) in the CSP are included in the sample before aggregating to the county level. This represents the number of newly founded nonemployers. For all other dependent variables, the sample includes all CSP startups (years 0-7) before aggregating to the county level. For example, ln(Employment) is the total employment of startups within their first 8 years in a county.

Pre-treatment control variables include firm age shares 0-7 (except when ln(Nonemployers) is the dependent variable), ln(Population), percentage of individuals aged 25 to 64, high school graduation percentage, college graduation percentage, female percentage, African American percentage, Asian percentage, and Hispanic percentage.

Standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

Specific Tax Types. Having found that total taxes affect startups, we next examine which specific taxes drive these effects. Tables 2 and 3 (Panels B-E) report results for property, corporate income, personal income, and sales taxes. The results are heterogeneous but the effects of certain instruments align with the total-tax patterns. Property taxes, PIT, and CIT exhibit significant effects, whereas sales taxes show minimal effect.

Property Taxes. Property taxes, which constitute 47 percent of total state taxes in our sample, exhibit particularly strong effects on startup composition.[12] Panel B of Table 3 shows that large property tax cuts reduce nonemployer startups but increase both employer startups and total employment (Figures A5b and A5c). This compositional shift reflects property taxes' role as a fixed cost barrier to scaling: lower property taxes enable ventures to start directly as employers rather than beginning as nonemployers, since hiring employees typically requires commercial office space that incurs property tax costs. Consistent with this interpretation, exit rates decline by 0.53 percentage points when property tax is cut (Figure A5d), indicating that property tax cuts stabilize existing businesses.

Corporate Income Tax. Corporate income taxes show strong, symmetric effects. Large CIT increases reduce nonemployer startups by 2.48 percent and employer startups by 3.13 percent (Panel C, Table 2 and Figure A6b). Mirroring these results, CIT cuts increase nonemployer startups by 3.29 percent, employer startups by 4.71 percent (Figure A6c), and total employment by 3.91 percent (Panel C, Table 3). The symmetric responses suggest that CIT operates as a marginal tax on entrepreneurial profits, affecting entry decisions both for tax increases and decreases.

Personal Income Tax. PIT cuts boost *nonemployer* startups by 2.1 percent, employer startups by 1.96 percent, and total employment by 4.97 percent (Panel D, Table 3). However, these effects emerge gradually: Figures A7a and A7b show significant effects appearing three years after the tax cut, presumably reflecting adjustment costs in hiring and organizational form transitions. PIT increases yield negative but statistically insignificant effects across all outcomes.

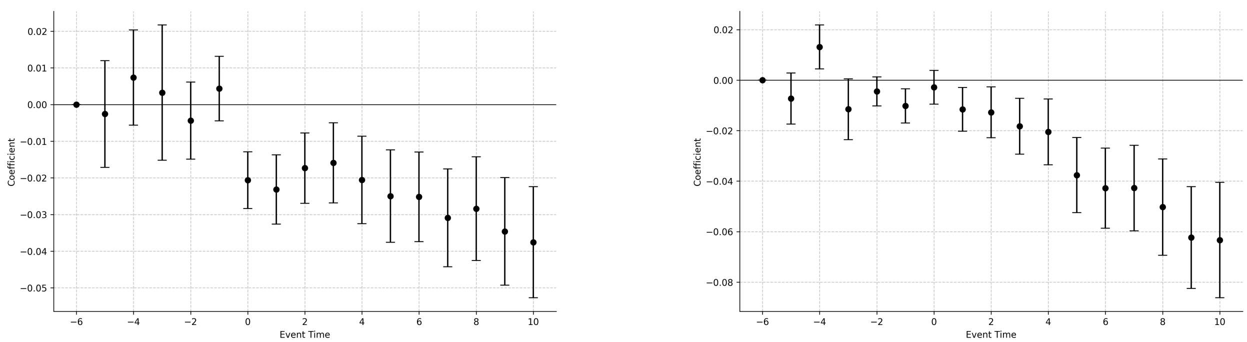
Sales Tax. Sales taxes show minimal effects on entrepreneurial outcomes in aggregate specifications, though we document industry-specific impacts in our heterogeneity analysis below.

Extensive versus Intensive Margin. The results above suggest that tax policies affect startup activity primarily through the extensive margin (firm entry and exit) rather than the intensive margin (employment growth within surviving firms): Namely, the employment elasticities are similar in magnitude to the elasticities for the number of employer startups, suggesting that employment effects operate through changes in the number of firms rather than growth within existing firms.

[12] This estimate comes from our summary statistics: property taxes represent 2.35 percent of value added and total taxes represent 4.96 percent of value added, yielding $2.35/4.96 = 0.47$.

We test this interpretation more formally using firm-level specifications that include firm fixed effects, allowing us to isolate how tax changes affect employment within continuing firms. Table **??** reports these results for increases and decreases of both total taxes and total incentives. The estimates are negligible in magnitude and statistically insignificant, confirming that tax policies do not significantly alter employment growth within surviving startups. This evidence indicates that tax policy affects entrepreneurial employment almost entirely through the extensive margin: tax increases suppress new firm entry, while tax cuts reduce exit rates and stimulate entry, but neither significantly affects employment growth among continuing firms.

Figure 2: Effects of Corporate Income Tax Increases on Startup Outcomes



(a) Nonemployer Startups (log)

(b) Employer Startups (log)

Notes: Event-study estimates of the effects of corporate income tax increases on startup outcomes. Event time 0 denotes the year of the tax increase. *Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

5.3 Effects of Tax Incentive Reforms

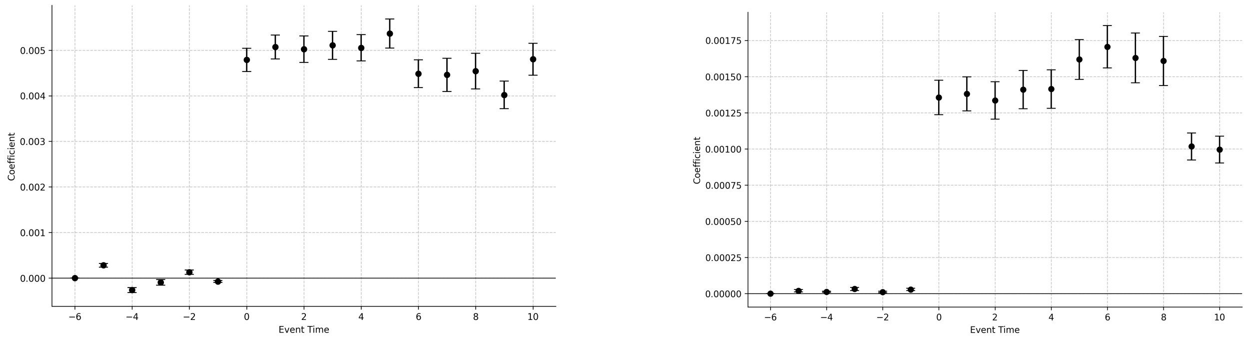
We find that incentive increases stimulate entrepreneurial activity, particularly among employer startups, but exhibit asymmetric effects: removing incentives does not consistently reverse these gains. This asymmetry contrasts with the more symmetric tax effects documented above and suggests that incentives operate through different mechanisms than tax rates.

Figure 3 confirms that large incentive increases correspond to sharp rises in incentive levels at event time zero. Tables 4 and 5 report the effects of incentive increases and decreases, respectively.

Total Incentives. Large increases in total incentives raise employer startups by 4.21 percent and total employment by 5.24 percent, with exit rates declining by 0.53 percentage points (Panel A, Table 4; Figures A9a and A9b). These effects indicate that incentives primarily benefit employer startups rather than lone entrepreneurs.

In contrast, incentive decreases show muted effects (Panel A, Table 5). We observe a decline in

Figure 3: First Stage: Effects of Incentive Increases on Incentive Levels



(a) Total Incentive Increases (b) Investment Tax Credit Increases

Notes: Event-study estimates showing the effects of incentive increases on corresponding incentive levels. Event time 0 denotes the year of the incentive increase. *Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

nonemployer startups but null effects on employer startups, employment, and exit rates. Startups are not more likely to exit when incentives are removed, suggesting that removed incentives tend to be windfall rather than necessary for marginal firms.

Specific Incentive Types. Incentives differ substantially in the behaviors they reward, so they may also differ in how successfully each affects entry and employment growth. Tables 4 and 5 report results for four major incentive programs. Among these, R&D and investment tax credits show the strongest and most consistent positive effects, while job creation tax credits show more modest impacts and property tax abatements prove largely ineffective in fostering startups.

R&D Tax Credits. R&D tax credits exhibit the strongest effects of any incentive program. Large increases drive a 7.05 percent rise in employer startups and a 7.50 percent increase in total startup employment (Panel C, Table 4)—nearly four times the magnitude of job creation tax credit effects. Interestingly, R&D credit decreases show asymmetric effects: rather than reversing the gains from increases, decreases significantly increase *nonemployer* startups (by 2.12 percent) and reduce exit rates (by 1.21 percentage points). This pattern may indicate that when R&D credits disappear, entrepreneurs who would have started employer firms instead launch smaller-scale nonemployer ventures, in turn reducing exit rates because nonemployers face lower fixed costs and closure risks.

Investment Tax Credits. Large increases in investment tax credits raise both nonemployer startups (by 2.19 percent) and employer startups (by 1.92 percent), making investment credits the only incentive program to significantly boost nonemployer formation (Panel B, Table 4 and Figure A10a). Increases also reduce exit rates by 0.24 percentage points, helping to sustain existing ventures. Credit reductions show a

Table 4: Effect of Large Incentive Policy Increases on Startup Outcomes

	(1) Policy	(2) ln(Nonemployers)	(3) ln(Employers)	(4) ln(Employment)	(5) Exit Rate	(6) Move-out Rate
Panel A: Total Incentives						
$Treat_{ct}$	0.0048***	0.0040	0.0421***	0.0524*	-0.0053**	-0.0006*
	(0.000145)	(0.0160)	(0.0103)	(0.0283)	(0.0026)	(0.0003)
No. of Obs.	34,500	34,500	34,500	34,500	34,500	34,500
Panel B: Job Creation Tax Credit						
$Treat_{ct}$	0.00305***	-0.0171	0.0282**	0.0269	-0.0053	-0.0006
	(0.0000786)	(0.0185)	(0.0118)	(0.0294)	(0.0029)	(0.0004)
No. of Obs.	34,000	34,000	34,000	34,000	34,000	34,000
Panel C: Investment Tax Credit						
$Treat_{ct}$	0.00141***	0.0219**	0.0192*	-0.0086	-0.0024*	0.0001
	(0.0000614)	(0.0067)	(0.0073)	(0.0195)	(0.0012)	(0.0003)
No. of Obs.	35,500	35,500	35,000	35,000	35,000	35,000
Panel D: Research and Development Credit						
$Treat_{ct}$	0.000205***	0.0177	0.0705**	0.0750*	0.0009	-0.0019*
	(0.00000905)	(0.0216)	(0.0268)	(0.0390)	(0.0040)	(0.0010)
No. of Obs.	33,500	33,500	33,500	33,500	33,500	33,500
Panel E: Property Tax Abatement						
$Treat_{ct}$	0.00257***	-0.0037	-0.0145*	0.0173	-0.0019	-0.0001
	(0.00011)	(0.0054)	(0.0060)	(0.0160)	(0.0010)	(0.0002)
No. of Obs.	35,500	35,500	35,500	35,500	35,500	35,500

This table shows the effects of large incentive policy increases on startup outcomes at the county level. The estimates are obtained using the DID method by Callaway and Sant'Anna (2021).

A large increase generally refers to the top 5% of the distribution of incentive policy changes. If the 95th percentile was equal to the median (usually zero), the top 1% was used instead. Job creation tax credit, investment tax credit, research and development credit, and property tax abatement are sourced from the Upjohn dataset.

The dependent variables are listed at the top. When ln(Nonemployers) is the dependent variable, only startups in their first year (year 0) in the CSP are included in the sample before aggregating to the county level. This represents the number of newly founded nonemployers. For all other dependent variables, the sample includes all CSP startups (years 0-7) before aggregating to the county level. For example, ln(Employment) is the total employment of startups within their first 8 years in a county.

Pre-treatment control variables include firm age shares 0-7 (except when ln(Nonemployers) is the dependent variable), ln(Population), percentage of individuals aged 25 to 64, high school graduation percentage, college graduation percentage, female percentage, African American percentage, Asian percentage, and Hispanic percentage.

Standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

Table 5: Effect of Large Incentive Policy Decreases on Startup Outcomes

	(1) Policy	(2) ln(Nonemployers)	(3) ln(Employers)	(4) ln(Employment)	(5) Exit Rate	(6) Move-out Rate
Panel A: Total Incentives						
$Treat_{ct}$	-0.0034***	-0.0131**	0.0076	-0.0114	-0.0037**	0.0003
	(0.000129)	(0.0063)	(0.0137)	(0.0206)	(0.0015)	(0.0003)
No. of Obs.	35,500	35,500	35,500	35,500	35,500	35,500
Panel B: Job Creation Tax Credit						
$Treat_{ct}$	-0.001***	0.0308	-0.0043	-0.0469	-0.0004	0.0019
	(0.0000497)	(0.0200)	(0.0245)	(0.0505)	(0.0021)	(0.0009)
No. of Obs.	34,000	34,000	34,000	34,000	34,000	34,000
Panel C: Investment Tax Credit						
$Treat_{ct}$	-0.000954***	0.0015	-0.0128	-0.0334*	-0.0015	-0.0001
	(0.0000397)	(0.0055)	(0.0067)	(0.0162)	(0.0010)	(0.0002)
No. of Obs.	35,500	35,500	35,500	35,500	35,500	35,500
Panel D: Research and Development Credit						
$Treat_{ct}$	-0.000333***	0.0212**	-0.0099	0.0116	-0.0121**	-0.0013*
	(0.00000962)	(0.0059)	(0.0190)	(0.0437)	(0.0042)	(0.0008)
No. of Obs.	35,000	35,000	35,000	35,000	35,000	35,000
Panel E: Property Tax Abatement						
$Treat_{ct}$	-0.0016***	-0.0139**	-0.0105	-0.0011	-0.0022	-0.0001
	(0.000073)	(0.0060)	(0.0116)	(0.0222)	(0.0016)	(0.0002)
No. of Obs.	37,000	37,000	37,000	37,000	37,000	37,000

This table shows the effects of large incentive policy decreases on startup outcomes at the county level. The estimates are obtained using the DID method by Callaway and Sant'Anna (2021).

A large decrease generally refers to the bottom 5% of the distribution of incentive policy changes. If the 5th percentile was equal to the median (usually zero), the bottom 1% was used instead. Job creation tax credit, investment tax credit, research and development credit, and property tax abatement are sourced from the Upjohn dataset.

The dependent variables are listed at the top. When ln(Nonemployers) is the dependent variable, only startups in their first year (year 0) in the CSP are included in the sample before aggregating to the county level. This represents the number of newly founded nonemployers. For all other dependent variables, the sample includes all CSP startups (years 0-7) before aggregating to the county level. For example, ln(Employment) is the total employment of startups within their first 8 years in a county.

Pre-treatment control variables include firm age shares 0-7 (except when ln(Nonemployers) is the dependent variable), ln(Population), percentage of individuals aged 25 to 64, high school graduation percentage, college graduation percentage, female percentage, African American percentage, Asian percentage, and Hispanic percentage.

Standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

different pattern: they significantly reduce total employment by 3.34 percent without affecting firm entry or exit (Panel B, Table 5 and Figure A10b). This asymmetry indicates that increases and decreases operate through different margins—increases stimulate entry and reduce exits, while decreases primarily constrain employment within continuing firms.

Job Creation Tax Credits. Notably, job creation tax credits produce smaller employment effects than R&D or investment credits despite explicitly targeting job creation. Large increases raise employer startups by 2.82 percent (Panel A, Table 4), but the employment effect (2.69 percent) is similar in magnitude and not statistically significant. This pattern indicates that job creation credits stimulate startup entry but do not make firms substantially larger, operating purely through the extensive margin. Credit decreases show no significant effects on any outcome, though this null result may reflect that the job creation tax credit cuts observed in our sample are relatively small in magnitude.

Property Tax Abatements. Property tax abatements show generally weak or negative results, making them the least effective incentive for startups. Increased abatements negatively affect employer startups (by 1.45 percent), while decreases adversely affect nonemployer startups (by 1.39 percent), with neither increase nor decrease significantly affecting employment or exit rates (Panel D, Tables 4 and 5). This pattern suggests that property tax abatements disproportionately benefit existing firms rather than new startups.[13] The contrast between the strong effects of direct property tax rate cuts (documented in Section 5.2) and the weak effects of property tax abatements suggests that the targeting and incidence of tax relief matters substantially for entrepreneurial responses.

In summary, R&D and investment tax credits prove most effective at stimulating startup activity, with R&D credits particularly powerful for employer startups and investment credits unique in benefiting nonemployers. Job creation tax credits show modest positive effects, while property tax abatements demonstrate limited efficacy for spurring entrepreneurship.

5.4 Heterogeneity Analysis

We explore effect heterogeneity across four dimensions: distance to state borders, industry exposure, policy introductions versus expansions, and entrepreneurial growth orientation. These analyses shed light on the mechanisms through which policies affect startups and help assess whether our estimates reflect genuine changes in entrepreneurial activity or merely geographic reallocation of firms.

[13] For example, Texas Tax Code Chapter 313 offers significant property tax limitations primarily benefiting large-scale projects rather than small startups. The negative effect of decreases on nonemployers may reflect composition effects: when abatements are reduced alongside property tax cuts (which we document are correlated in Table B3), entrepreneurs may shift toward employer form rather than nonemployer form.

Distance to State Borders. A key question is whether our estimated policy effects represent genuine changes in total entrepreneurial activity or simply geographic reallocation across state lines. Our results above show limited relocation of *existing* startups, but policies could influence where entrepreneurs initially form firms rather than where they relocate afterward. If entrepreneurs respond to policy changes by choosing to start businesses in neighboring states with more favorable conditions, our estimates would overstate the aggregate effects of policy since reallocation would be measured as an improvement. To examine this possibility, we compare policy effects in border counties (those located along state borders) versus interior counties (away from borders). Entrepreneurs near state borders face lower costs of operating across state lines—including commuting costs, moving costs, and network disruption—than those in interior counties. If cross-border mobility drives our results, we should observe larger policy effects in border counties.

Figures A14 and A15 in Appendix A compare the effects of large changes in total taxes and total incentives on startup outcomes across border and interior counties. We find no systematically differential responses: the 95 percent confidence intervals largely overlap across both county types.[14] Combined with the null effects on move-out rates documented in our main results (Tables 2–5), this evidence indicates that cross-state reallocation is not the primary channel through which policies affect startups. Instead, policy changes generate real adjustments in the total number of startups rather than redistributing activity across state lines. Our estimated effects do not simply reflect a zero-sum contest among states for a limited pool of entrepreneurs.

Industry Heterogeneity. Certain industries face greater direct exposure to specific taxes. Retail activity may be immediately affected by sales taxes through consumer demand, while capital-intensive manufacturing may be particularly sensitive to property taxes. We estimate policy effects separately by industry to capture such heterogeneity.

Figure A8b shows that sales tax increases significantly reduce employment in the retail industry, with consistently negative point estimates across post-treatment periods. This effect is substantially larger than the small, statistically insignificant aggregate effect found in baseline regressions (Table 2 and Figure A8a). Similarly, Figure A5e demonstrates that property tax cuts boost employer startups in manufacturing by amounts exceeding the 2.00 percent baseline effect (Table 3 and Figure A5b). These results confirm that policy impacts are not uniform across sectors but concentrate in industries most directly affected by the relevant tax base.

Policy Introductions versus Expansions. Policy introductions—cases where a policy emerges from zero for the first time—may have different effects than incremental expansions of existing programs. For

[14]Standard errors are larger for both groups than in the main estimations due to smaller sample sizes. The negative effect of total tax increases on exit rates is only significant for border counties, but this outcome measures closures rather than relocations.

most taxes, introductions are rare because these policies have existed for decades. However, introductions account for 44–48 percent of large increases in R&D tax credits and job creation tax credits, allowing us to compare introduction effects with expansion effects.

Table B1 in Appendix B restricts the analysis to policy introductions, while Table 4 reports baseline estimates pooling introductions and expansions. The results reveal that policy introductions generate substantially stronger effects. Introducing an R&D tax credit increases nonemployer startups by 2.35 percent and employer startups by 7.51 percent (Figure A11)—magnitudes exceeding the pooled estimates in Table 4. Likewise, introducing a job creation tax credit raises employer startups by 4.86 percent and employment by 7.32 percent. These findings suggest that new policy introductions—particularly for R&D and job creation tax credits—generate stronger impacts on startup activity than subsequent modifications of existing incentives. Incremental expansions of established programs appear less effective at stimulating entrepreneurship.

Growth Ambition Among Nonemployers. The universe of nonemployers includes diverse business types, from lifestyle businesses with no growth intention to ambitious ventures planning to hire. To identify high-growth-potential nonemployers, we focus on those registering for an Employer Identification Number (EIN).[15] EIN registration typically signals intent to hire employees or incorporate in the future, distinguishing growth-oriented nonemployers from lifestyle businesses.

Table B2 reports results restricting the nonemployer sample to EIN holders. Comparing these estimates to the full-sample results (Tables 2–5) reveals that effects on EIN-holding nonemployers generally have the same signs as effects on all nonemployers, but point estimates are closer to zero, rendering effects of tax and incentive decreases statistically insignificant. The exception is increases in total incentives, which significantly boost EIN-holding nonemployers—mirroring the effect on employer startups. Indeed, except for total tax increases, EIN-holding nonemployers respond to policy changes more similarly to employer startups than to nonemployers overall. This pattern is consistent with EIN-holding nonemployers representing a distinct subpopulation planning future transitions to employer status.

5.5 Robustness Checks

We conduct two sets of robustness checks to assess the sensitivity of our results: tests for concurrent policy changes and alternative treatment definitions.

Concurrent Policy Changes. A potential concern is that states may simultaneously adjust multiple policies, either to maintain revenue neutrality (offsetting one tax change with another) or for political reasons.[16] If our focal policy changes coincide with other policy shifts, our estimates could reflect confounded

[15] This screening approach follows Fairlie et al. (2023). Guzman and Stern (2020) use initial conditions such as business registration, organizational form, naming conventions, and intellectual property protection to measure entrepreneurial quality.

[16] For example, high-tax states like New Mexico, Iowa, New York, and Pennsylvania tend to offer higher incentives, potentially

effects.

We first assess whether policy changes are systematically correlated. Table B3 reports results from DID regressions using other policies as dependent variables, estimating whether large changes in one policy predict changes in other policies. Policy events (increases and decreases) appear in columns, with treatment effects on other policies in rows. We exclude mechanically correlated pairs (e.g., specific taxes and total taxes). The results indicate that policy changes are largely uncorrelated: in most cases, tax policy events are not significantly associated with changes in other policies. The main exception is property tax abatements, which correlate positively with property taxes—a plausible relationship suggesting that states raising property taxes may compensate through corresponding abatements.

Although policy changes are largely uncorrelated, we conduct direct robustness checks controlling for concurrent policy changes. Because the Callaway and Sant'Anna (2021) method uses only pre-treatment control values and does not accommodate time-varying controls, we employ the stacked DID approach of Cengiz et al. (2019) for this test. This method allows us to incorporate contemporaneous changes in other policies, addressing potential omitted-variable bias from simultaneous reforms.

Table B4 reports results with and without controls for other policies, including other taxes, other incentives, and minimum wages (indicated in the "Policy Controls" row). The key finding is that results remain quite similar regardless of whether other policy controls are included. This consistency demonstrates that our main results are not driven by concurrent policy changes. If anything, some estimated effects are larger in absolute magnitude when including other policy controls, indicating that our main specifications without policy controls provide conservative estimates of policy effects.[17]

Alternative Treatment Thresholds. Our baseline analysis defines "large" changes as those exceeding the 95th percentile (increases) or falling below the 5th percentile (decreases) of the policy change distribution. To assess sensitivity to this definition, we re-estimate regressions using more extreme thresholds (99th/1st percentiles) and broader thresholds (90th/10th percentiles), following Cavallo et al. (2013).

Table B5 reports results for total tax changes using the 99th/1st and 90th/10th percentile thresholds as alternative definitions of large policy changes. The findings are largely consistent with our main results (95th/5th percentiles): total tax increases reduce employer startups and employment, while total tax decreases reduce exit rates. One exception is the estimated effect of tax increases on nonemployers, which becomes statistically insignificant when using the 90th percentile cutoff, potentially because the threshold

offsetting tax burdens. In contrast, low-tax states like Nevada and Wyoming typically provide fewer incentives, reflecting a more hands-off approach.

[17] This robustness check also reveals that results differ somewhat between the Callaway and Sant'Anna (2021) method and stacked DID (Cengiz et al., 2019). We present the Callaway and Sant'Anna (2021) estimates as our main results because stacked DID uses weights determined by the number of treated units and treatment variance within each stacked event, which lacks economic justification (Roth et al., 2023).

includes more modest tax changes. Overall, these tests indicate that our results are not sensitive to the specific definition of large policy reforms.

6 Conclusion

Fiscal policy significantly shapes entrepreneurial activity, but policy design matters: the effects of taxes and incentives vary substantially across both policy instruments and firm types. Using administrative data covering the universe of U.S. startups, we provide the first comprehensive analysis of this heterogeneity—patterns that prior research could not detect due to data limitations and narrow policy focus.

We find that different fiscal policies affect startups through distinct mechanisms. Among broad-based taxes, those on corporate income show large, symmetric effects on nonemployer and employer startups (elasticities of –0.27 and –0.50), operating as a straightforward reduction in the returns to entrepreneurship. Property taxes primarily affect the composition of new businesses: cuts reduce nonemployer startups while increasing employer startups and total employment, functioning as a fixed cost barrier to scaling. Personal income tax cuts boost both startup types. Among incentives, R&D and investment tax credits prove most effective at increasing startups and their employment, raising employer startup formation by 7.05 and 1.92 percent respectively. However, the effects of these incentives are asymmetric: expansions stimulate activity while cuts do not cause contractions of similar magnitude. Policy introductions generate substantially stronger effects than incremental expansions, with first-time R&D credit adoptions increasing employer startups by 7.51 percent.

Critically, we find that tax policies affect total entrepreneurial activity rather than merely reallocating startups across state borders. We document null effects on interstate relocation rates and no differential impacts between border and interior counties, indicating that our estimates reflect genuine changes in aggregate startup activity rather than zero-sum competition among states. Combined with our firm-level evidence that tax policies operate through the extensive margin of entry and exit rather than employment growth within surviving firms, these findings suggest that state tax competition over startups is limited in scope.

Our empirical approach addresses key identification challenges in this literature. We use the Callaway and Sant'Anna (2021) difference-in-differences estimator to avoid contamination from already-treated units, focus on large discrete policy changes (exceeding the 95th percentile) to maximize statistical power and separate policy effects from trends, and verify that different policy changes are largely uncorrelated across states and time. Event study estimates confirm parallel pre-trends and show that effects emerge immediately following policy implementation. Results are robust to controlling for concurrent policy changes using stacked DID and to alternative definitions of large policy changes.

These findings have important implications. For policymakers, our results demonstrate that fiscal policy can be precisely targeted: corporate income tax cuts primarily benefit employer startups, personal income tax cuts disproportionately affect nonemployers, and R&D credits stimulate high-growth ventures. The magnitudes are economically meaningful—total tax elasticities of –0.27 to –0.59 imply that a 10 percent tax increase reduces startup activity by 2.7 to 5.9 percent. Critically, these effects reflect genuine changes in total entrepreneurial activity rather than reallocation across state borders, suggesting that state-level tax policy can generate aggregate welfare gains without merely redistributing activity. For researchers, incorporating these startup responses into optimal taxation models could yield important welfare insights, particularly given our finding that policies operate primarily through the extensive margin of firm entry and exit rather than growth within surviving firms. Standard models of optimal taxation typically focus on labor supply and savings decisions; our evidence suggests that entrepreneurial entry responses represent an additional margin of behavioral adjustment with potentially large aggregate consequences.

Several directions for future research emerge. First, our findings raise questions about general equilibrium effects: if corporate tax cuts increase employer startups while property tax cuts shift entry from nonemployer to employer form, what are the net employment and productivity effects of revenue-neutral tax reforms? Second, linking administrative records to individual-level data could illuminate why entrepreneurs respond differently to different taxes—for instance, whether property tax effects operate through wealth constraints, organizational form costs, or hiring frictions. Third, long-run impacts on firm quality, survival trajectories, and aggregate productivity beyond the first eight years warrant investigation, particularly given our finding that R&D credits have especially large effects. Finally, understanding the incidence of business taxes and incentives—how much is borne by entrepreneurs versus workers, landlords, or consumers—would clarify welfare implications.

Overall, this study demonstrates that fiscal policy significantly shapes entrepreneurial activity, but effects vary substantially across policy instruments and firm types. Tax increases suppress startup formation, while tax cuts and targeted incentives stimulate entry and reduce exits. Recognizing and exploiting this heterogeneity is essential for designing effective entrepreneurial policy and for incorporating startup dynamics into models of optimal taxation and economic growth.

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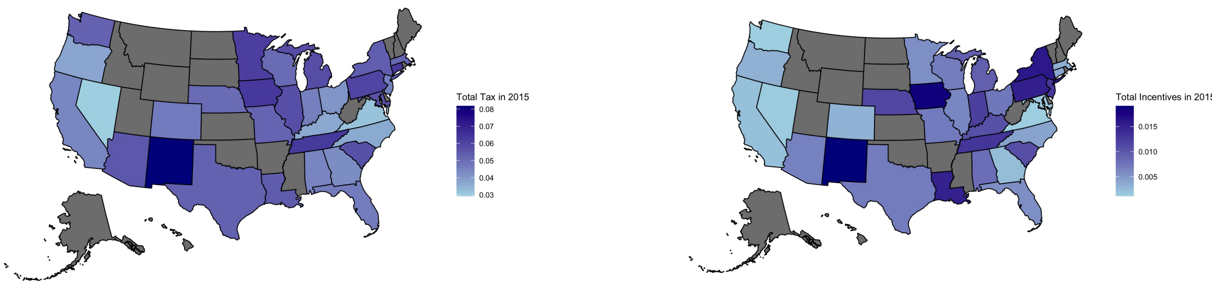
Appendices

A Supplemental Figures



(a) Total Taxes in 2015

(b) Total Incentives in 2015

Figure A1: State-level comparisons: Total Taxes and Total Incentives

Note: Maps for 2015 show total taxes (Figure A1a) and total incentives (Figure A1b) for the 33 PDIT-covered jurisdictions: Alabama, Arizona, California, Colorado, Connecticut, the District of Columbia, Florida, Georgia, Illinois, Indiana, Iowa, Kentucky, Louisiana, Maryland, Massachusetts, Michigan, Minnesota, Missouri, Nebraska, Nevada, New Jersey, New Mexico, New York, North Carolina, Ohio, Oregon, Pennsylvania, South Carolina, Tennessee, Texas, Virginia, Washington, and Wisconsin. Jurisdictions not covered by PDIT are shaded gray.

Data Source: Upjohn Institute's PDIT.

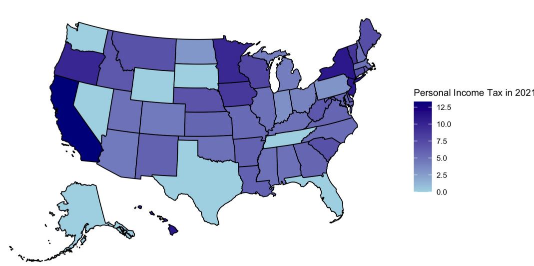
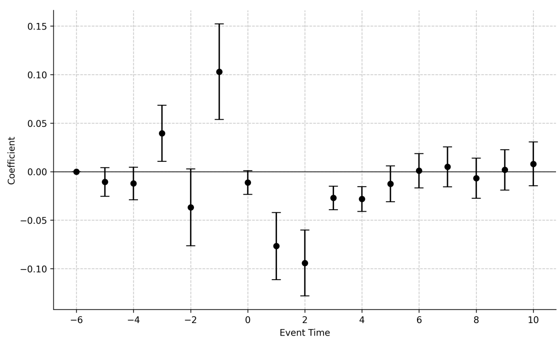


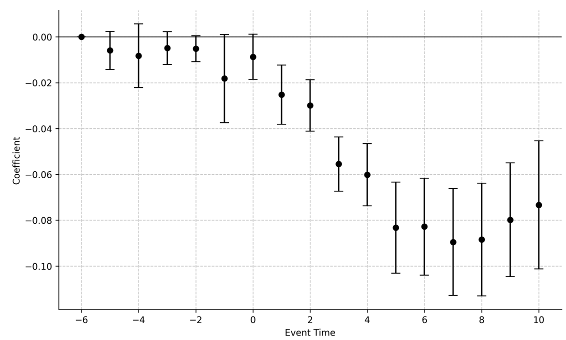
Figure A2: State-level Comparisons: Personal Income Tax Rates in 2021

Note: The map shows the top marginal state PIT rates for tax year 2021.

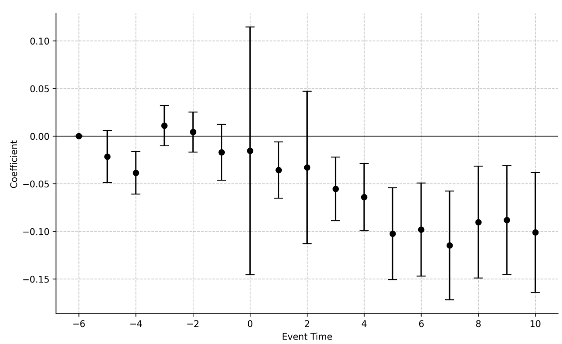
Data source: Tax Foundation.



(a) Effect of total tax increases on ln(Nonemployers)

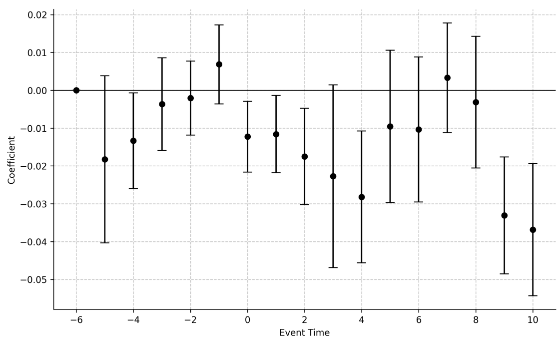


(b) Effect of total tax increases on ln(Employers)

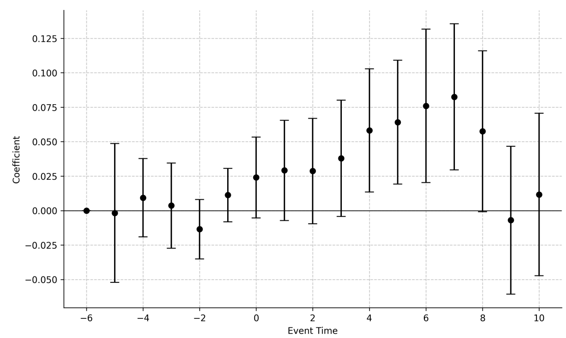


(c) Effect of total tax increases on ln(Employment)

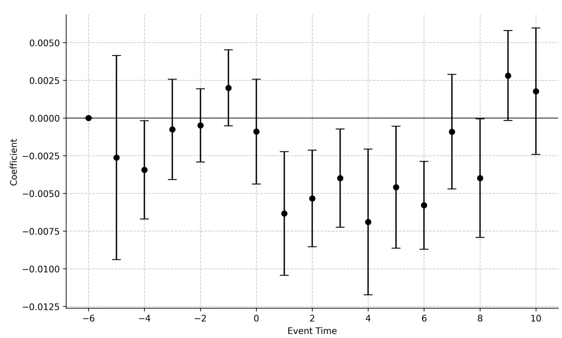
Figure A3: Event-study estimates of the effects of total tax increases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



(a) Effect of total tax decreases on ln(Nonemployers)

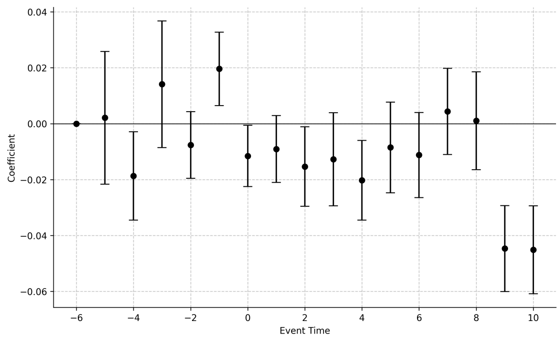


(b) Effect of total tax decreases on ln(Employment)

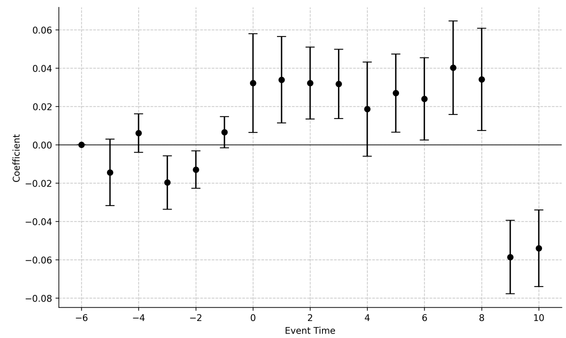


(c) Effect of total tax decreases on exit rate

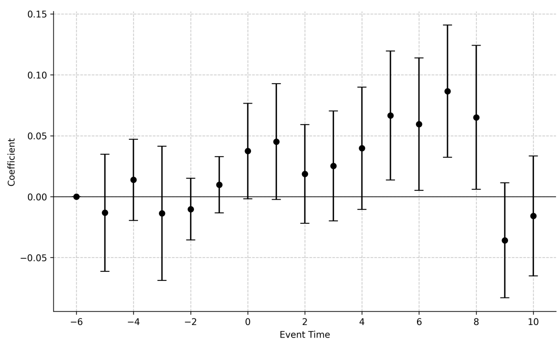
Figure A4: Event-study estimates of the effects of total tax decreases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



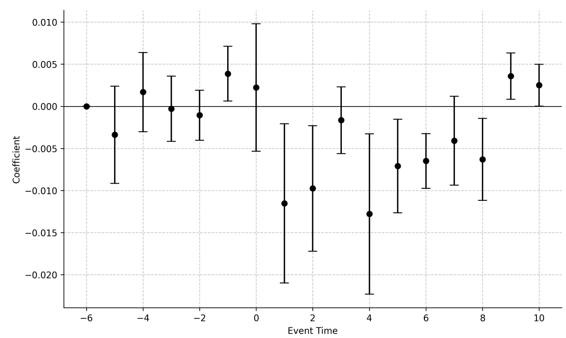
(a) Effect of property tax decreases on ln(Nonemployers)



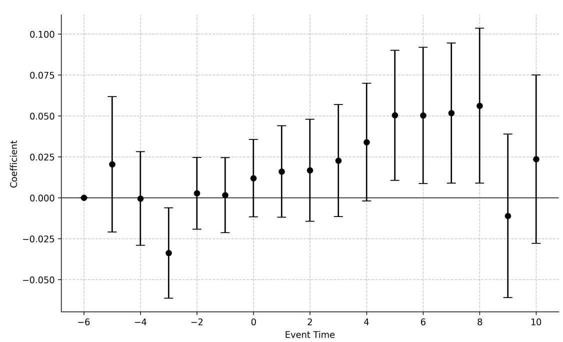
(b) Effect of property tax decreases on ln(Employers)



(c) Effect of property tax decreases on ln(Employment)

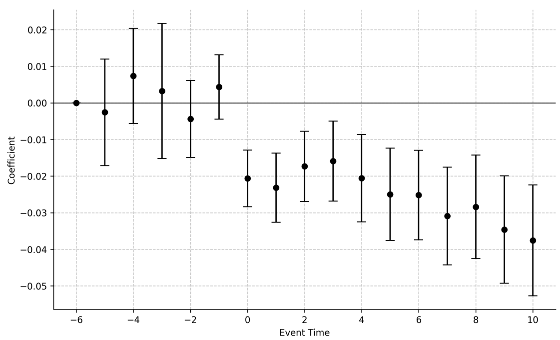


(d) Effect of property tax decreases on exit rate

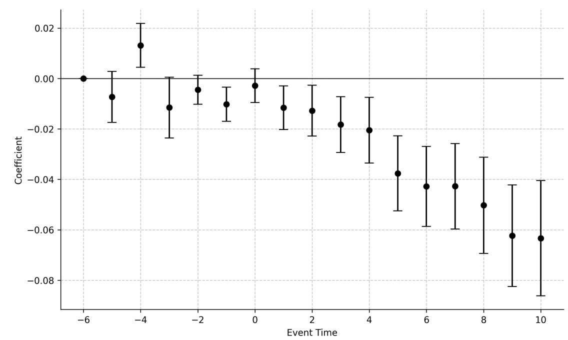


(e) Effect of property tax decrease on ln(Employers) in the manufacturing industry

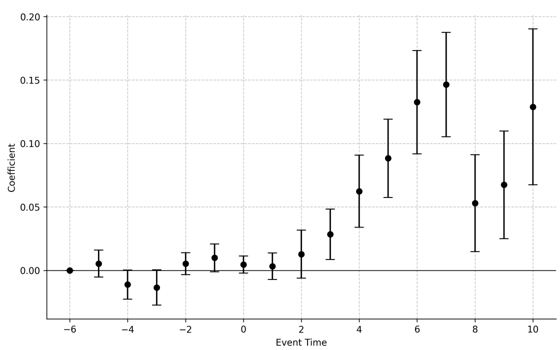
Figure A5: Event-study estimates of the effects of property tax decreases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



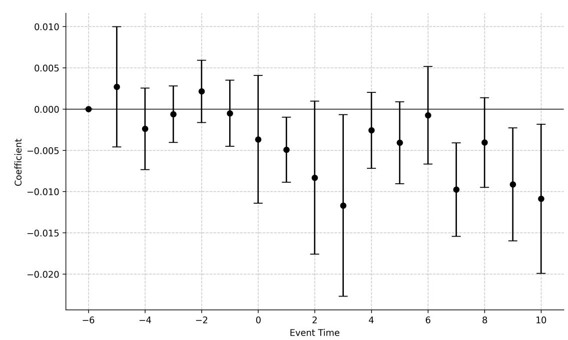
(a) Effect of CIT increases on ln(Nonemployers)



(b) Effect of CIT increases on ln(Employers)



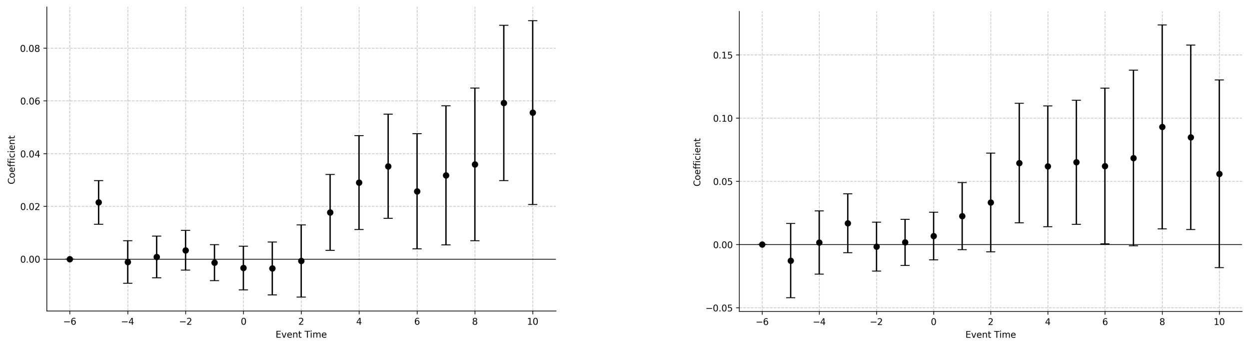
(c) Effect of CIT decreases on ln(Employers)



(d) Effect of CIT decreases on exit rate

Figure A6: Event-study estimates of the effects of CIT increases and decreases on startup outcomes

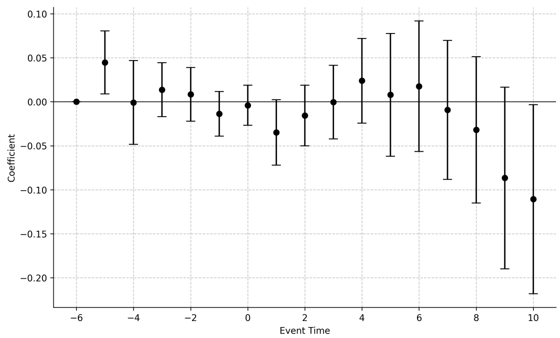
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



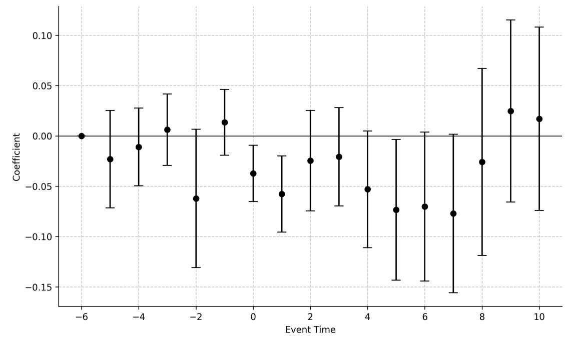
(a) Effect of PIT decreases on ln(Employers)

(b) Effect of PIT decreases on ln(Employment)

Figure A7: Event-study estimates of the effects of PIT decreases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

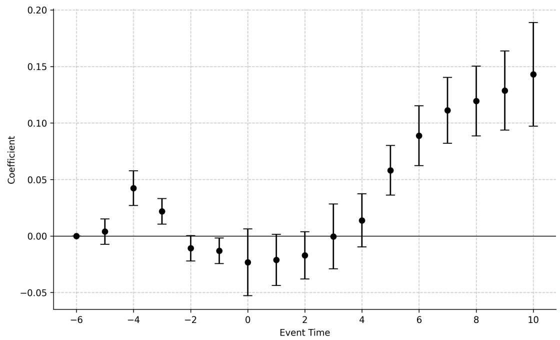


(a) Effect of sales tax increases on ln(Employment)

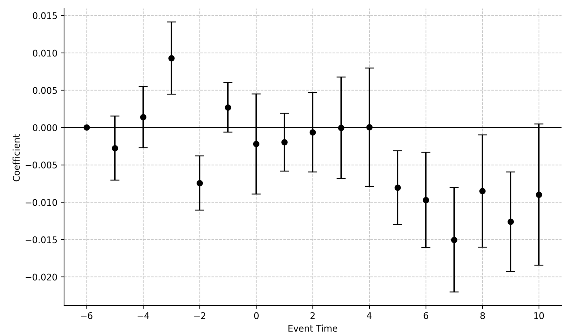


(b) Effect of sales tax increase on ln(Employment) in the retail industry

Figure A8: Event-study estimates of the effects of sales tax increases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

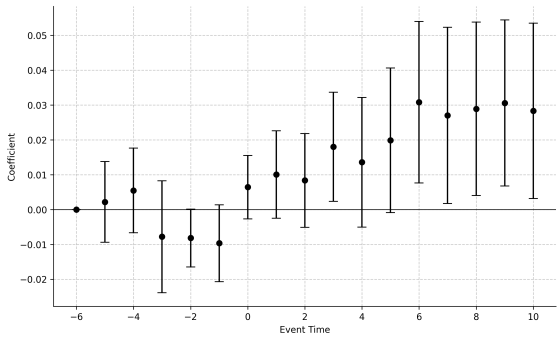


(a) Effect of total incentive increases on ln(Employers)

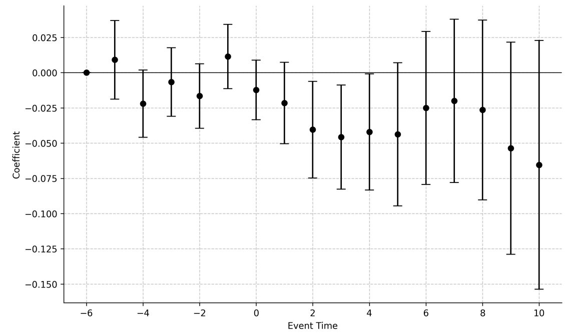


(b) Effect of total incentive increases on exit rate

Figure A9: Event-study estimates of the effects of total incentive increases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

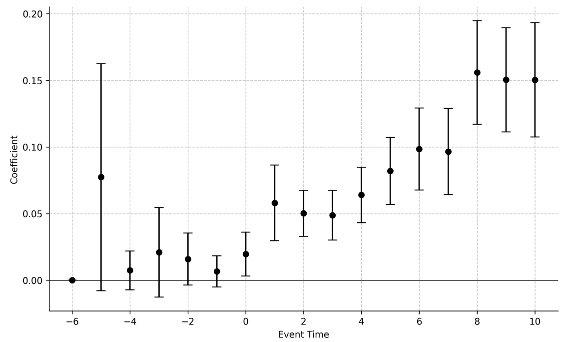


(a) Effect of investment tax credit increases on ln(Employers)



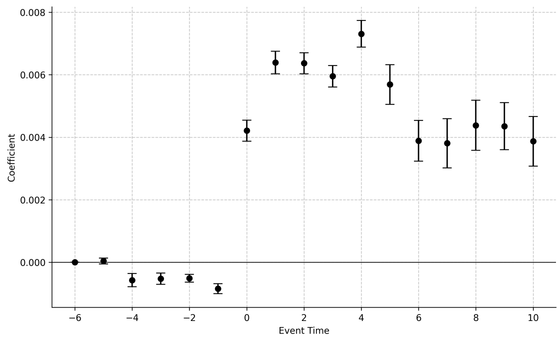
(b) Effect of investment tax credit decreases on ln(Employment)

Figure A10: Event-study estimates of the effects of investment tax credit increases and decreases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

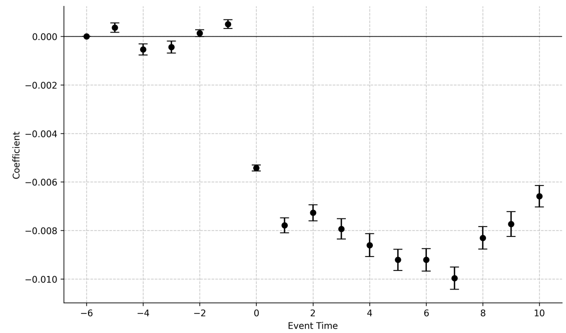


(a) Effect of research and development credit introduction on ln(Employers)

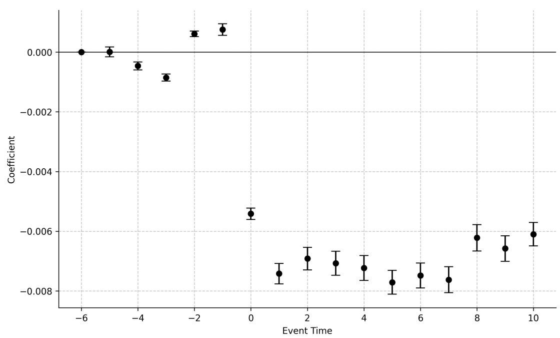
Figure A11: Event-study estimates of the effects of research and development credit introduction on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



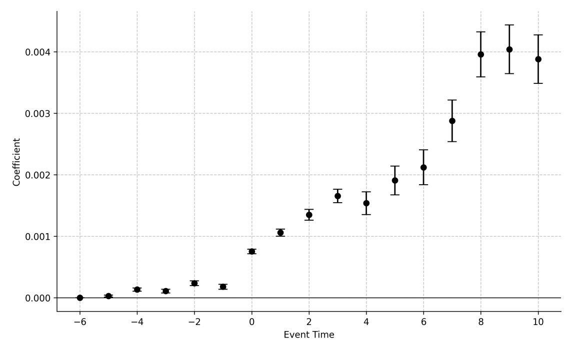
(a) Effect of total tax increases on total tax level



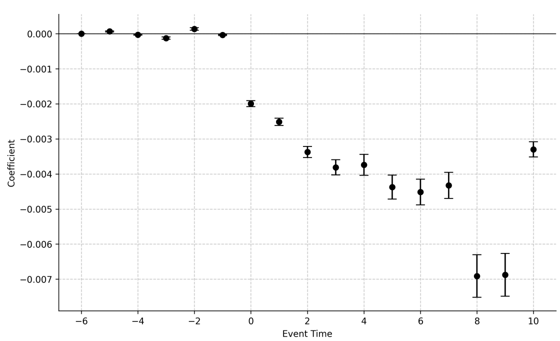
(b) Effect of total tax decreases on total tax level



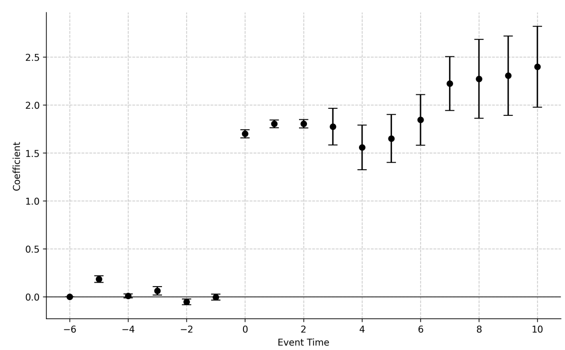
(c) Effect of property tax decreases on property tax level



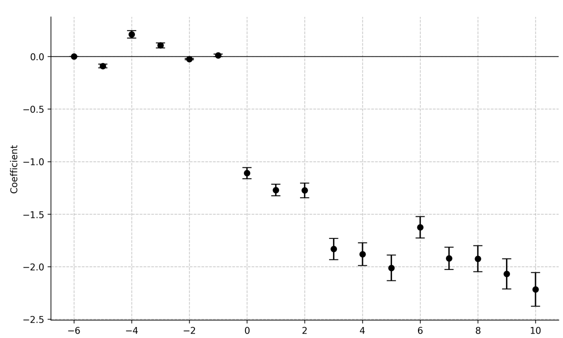
(d) Effect of CIT increases on CIT level



(e) Effect of CIT decreases on CIT level

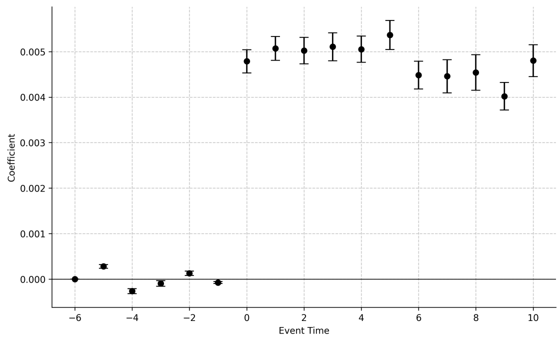


(f) Effect of PIT increases on PIT level

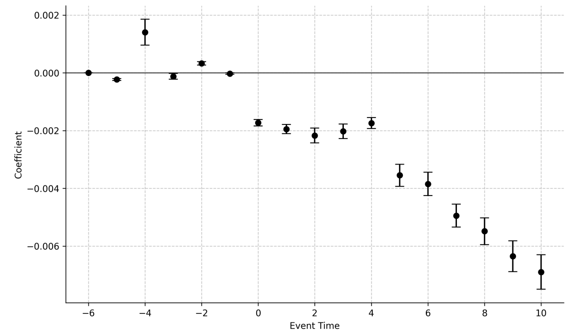


(g) Effect of PIT decreases on PIT level

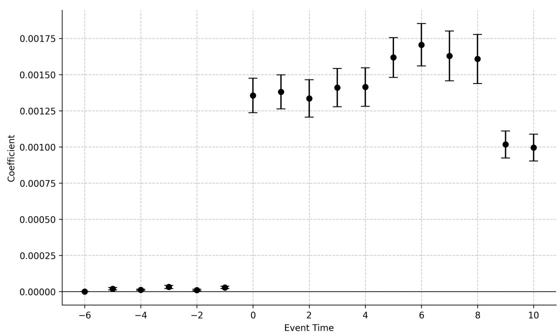
Figure A12: Event-study estimates of the effects of tax increases and decreases on tax level
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



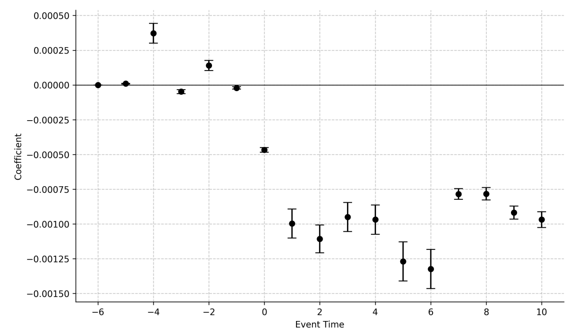
(a) Effect of total incentive increases on total incentive level



(b) Effect of total incentive decreases on total incentive level



(c) Effect of investment tax credit increases on investment tax credit level



(d) Effect of investment tax credit decreases on investment tax credit level

Figure A13: Event-study estimates of the effects of incentive increases and decreases on incentive level
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

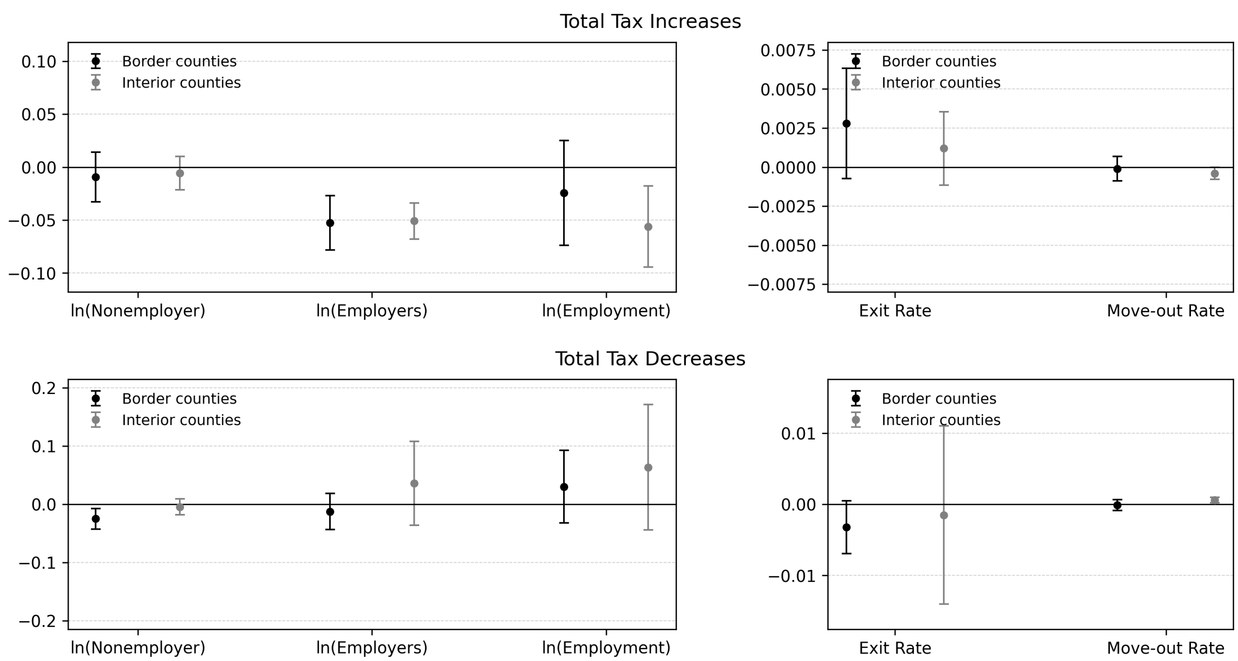


Figure A14: Effects of Large Total Tax Changes on Startup Outcomes (Border vs. Interior Counties)
Notes: This figure reports the effects of large changes in total taxes on startup outcomes at the county level, comparing counties that border other states with interior counties. Estimates are obtained using the DID method of Callaway and Sant'Anna (2021).
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

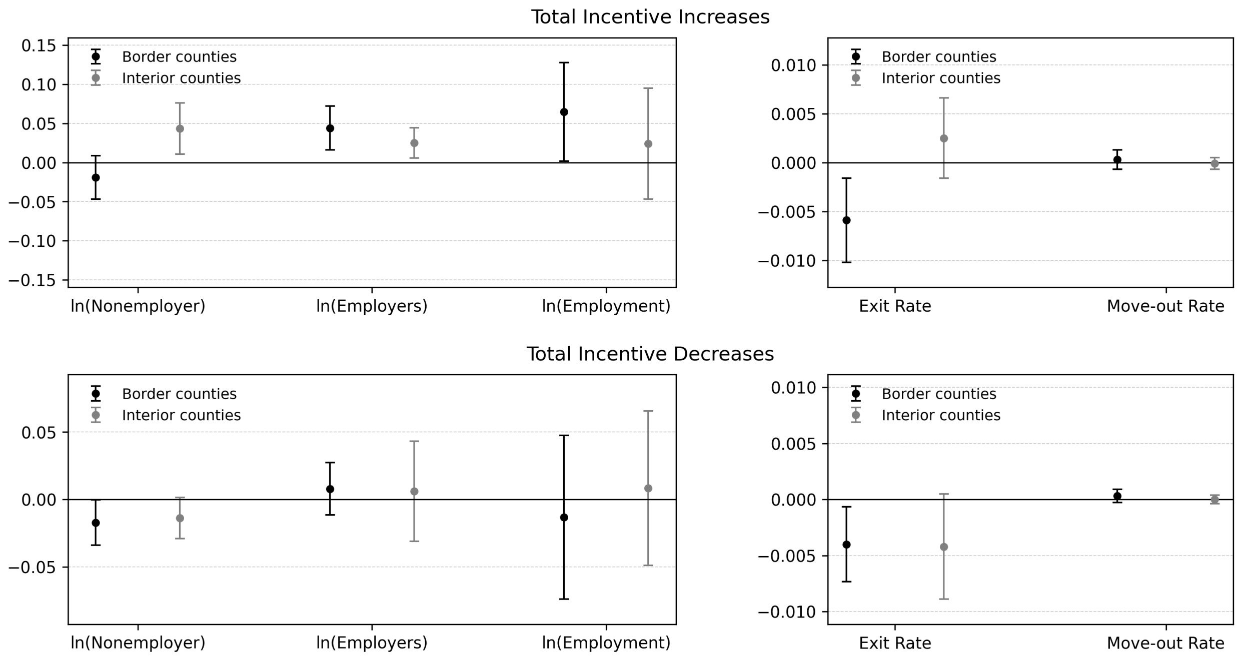


Figure A15: Effects of Large Total Incentive Changes on Startup Outcomes (Border vs. Interior Counties)
Notes: This figure reports the effects of large changes in total incentives on startup outcomes at the county level, comparing counties that border other states with interior counties. Estimates are obtained using the DID method of Callaway and Sant'Anna (2021).
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

B Supplemental Tables

Table B1: Effects of New Policy Introductions on Startup Outcomes

	(1) Policy	(2) ln(Nonemployers)	(3) ln(Employers)	(4) ln(Employment)	(5) Exit Rate	(6) Move-out Rate
Panel A: Introduction of Job Creation Tax Credit						
$Treat_{ct}$	0.0041***	-0.0278	0.0486**	0.0732*	-0.0090	-0.0005
	(0.0010)	(0.0263)	(0.0227)	(0.0394)	(0.0055)	(0.0004)
No. of Obs.	36,000	36,000	36,000	36,000	36,000	36,000
Panel B: Introduction of Research and Development Credit						
$Treat_{ct}$	0.0003***	0.0235***	0.0751***	0.0433**	-0.0030**	-0.0009***
	(0.0001)	(0.0072)	(0.0103)	(0.0197)	(0.0014)	(0.0003)
No. of Obs.	34,000	34,000	34,000	34,000	34,000	34,000

Notes: This table reports the effects of policy introductions on startup outcomes using the DID method of Callaway and Sant'Anna (2021). Unlike the baseline specifications that define "large" policy changes based on percentiles of the policy change distribution, here only large increases that represent new introductions of a policy (changes from zero) are coded as events.

The dependent variables are listed in the column headers. When ln(Nonemployers) is the dependent variable, the sample includes only first-year (year 0) CSP startups before county-level aggregation. For other outcomes, the sample includes all CSP startups (years 0–7). For example, ln(Employment) measures total employment of startups within their first eight years in a county.

Pre-treatment controls include firm age shares 0–7 (except when ln(Nonemployers) is the dependent variable), ln(Population), percentage aged 25–64, high school graduation rate, college graduation rate, female percentage, African American percentage, Asian percentage, and Hispanic percentage. Standard errors are in parentheses, clustered at the state level.

Standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

Table B2: Effects of Large Policy Changes on Nonemployer Startups with EIN

	(1) ln(Nonemployers with EIN)	(2) ln(Nonemployers with EIN)
	Total Tax Increases	**Total Tax Decreases**
$Treat_{ct}$	-0.0224**	-0.0072
	(0.0108)	(0.0128)
No. of Obs.	34,500	36,500
	Total Incentive Increases	**Total Incentive Decreases**
$Treat_{ct}$	0.0803***	-0.0083
	(0.0146)	(0.0101)
No. of Obs.	34,500	35,500

Notes: This table shows the effects of large policy increases or decreases on nonemployer startups with EIN at the county level. The estimates are obtained using the DID method by Callaway and Sant'Anna (2021).

A large increase generally refers to the top 5% of the distribution of policy changes. If the 95th percentile was equal to the median (usually zero), the top 1% were used instead.

The dependent variables are listed at the top. When ln(Nonemployers) is the dependent variable, only startups in their first year (year 0) in the CSP are included in the sample before aggregating to the county level. This represents the number of newly founded nonemployers. For all other dependent variables, the sample includes all CSP startups (years 0-7) before aggregating to the county level. For example, ln(Employment) is the total employment of startups within their first 8 years in a county.

Pre-treatment control variables include firm age shares 0-7 (except when ln(Nonemployers) is the dependent variable), ln(Population), percentage of individuals aged 25 to 64, high school graduation percentage, college graduation percentage, female percentage, African American percentage, Asian percentage, and Hispanic percentage.

Standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

Table B3: Correlation Between Policy Changes

Policy/Events	Total Tax ↑	Total Tax ↓	Total Incentives ↑	Total Incentives ↓	Property Tax ↑	Property Tax ↓	CIT ↑	CIT ↓	PIT ↑ (State)	PIT ↓ (State)
Total taxes			0.2970	-0.3842***						
Total incentives	0.2029	-0.1398			0.2986***	-0.0713	0.1551	-0.4107***	0.0402	-0.3278
Property tax			0.3292**	-0.1908			-0.6958***	0.0445	0.2974	-0.2438
Corporate Income Tax			0.2095	-0.5743	0.1494	0.2123			0.3103**	-0.8638
State personal income tax rate			0.0285	-0.0527	0.0545	0.1062	0.0961	-0.0977		
Sales tax			-0.0412	-0.0552	-0.0446	-0.0683	-0.0984**	-0.0382	-0.0119	0.1450
Job creation tax credit	-0.1374	-0.1528			-0.1459	-0.1212	-0.1336	-0.3116**	0.4302***	0.3636
Investment tax credit	0.2446	0.1257			0.3245	0.1673	0.3495	-0.1510	-0.1648	-0.4700
R&D credit	0.0463	-0.1531			0.0192	-0.1136	0.1746	-0.5382**	0.3904	-0.0564
Property tax abatement	0.2048	-0.2081			0.2610	-0.1774	-0.0318	-0.1482	-0.2812	-0.3973*
Policy/Events	**Sales Tax ↑**	**Sales Tax ↓**	**Job Creation Credit ↑**	**Job Creation Credit ↓**	**Investment Credit ↑**	**Investment Credit ↓**	**R&D Credit ↑**	**R&D Credit ↓**	**Property Tax Abatement ↑**	**Property Tax Abatement ↓**
Total taxes			0.0263	-0.3123**	-0.0857	-0.4095**	-0.1611	-0.1934	0.3987***	-0.5230**
Total incentives	0.2013	0.1675								
Property tax	0.1880	-0.1117	0.0320	-0.0330	-0.2780	-0.1854	0.0046	0.0174	0.4799***	-0.4874***
Corporate income tax	-0.1122	-0.0440	0.0987	-0.5315**	0.4071***	-0.6561**	-0.3754	-0.5822**	0.0032	-0.5653
State personal income tax rate	-0.1134	-0.0417	0.0051	-0.0091	0.1461**	-0.0193	0.0624	-0.0766	0.0281	-0.1426
Sales tax			-0.0456	-0.1332**	-0.0263	-0.0424	-0.0401	0.0024	0.0307	0.0628
Job creation tax credit	-0.0508	-0.1126			-0.0346	-0.1592*	0.0218	0.0955	-0.2700***	-0.2474**
Investment tax credit	0.0526	0.0858	-0.0595	-0.1745			-0.2735	0.0784	-0.1629	-0.2623
R&D credit	-0.0034	0.1238	-0.0137	-0.1372	0.1936	0.2104			0.0155	-0.0172
Property tax abatement	0.1749	0.2075	0.0092	-0.2729**	-0.2005	-0.2893	-0.3061*	-0.1366		

This table reports estimates from the DID method of Callaway and Sant'Anna (2021), showing how large policy events (↑ increases, ↓ decreases) affect other policy levels. Each cell presents the coefficient from a separate regression, where the outcome is the policy level indicated by the row and the treatment is the event indicated by the column. Insignificant coefficients indicate no systematic change in the policy outcome following the event.

Events are defined as large increases or decreases in the corresponding policy measures. A large increase generally refers to the top 5% of the distribution of policy changes. If the 95th percentile was equal to the median (usually zero), the top 1% were used instead. Similarly, a large decrease is defined using the bottom 5% (or bottom 1% when applicable).

Standard errors in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B4: Effects of Large Tax Policy Increases on Startups (Stacked DID With and Without Policy Controls)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	ln(Nonemployers)		ln(Employers)		ln(Employment)		Exit Rate		Move-out Rate	
Panel A: Total Taxes										
Post Treat	-0.0153	-0.0139	-0.0395**	-0.0401**	-0.0385*	-0.0397*	-0.0003	-0.0001	-0.0001	-0.0001
	(0.0173)	(0.0175)	(0.0159)	(0.0160)	(0.0197)	(0.0204)	(0.0013)	(0.0013)	(0.0002)	(0.0002)
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel B: Total Incentives										
Post Treat	-0.0037	-0.0102	0.0311**	0.0320**	0.0343*	0.0346*	-0.0060***	-0.0069***	-0.0005**	-0.0005***
	(0.0129)	(0.0144)	(0.0140)	(0.0141)	(0.0197)	(0.0205)	(0.0021)	(0.0021)	(0.0002)	(0.0002)
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel D: Property Tax										
Post Treat	0.0084	0.0086	-0.0301*	-0.0356**	-0.0226	-0.0296	-0.0006	0.0002	0.0001	0.0001
	(0.0276)	(0.0269)	(0.0180)	(0.0175)	(0.0231)	(0.0226)	(0.0015)	(0.0016)	(0.0002)	(0.0002)
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel E: Corporate Income Tax										
Post Treat	-0.0239*	-0.0247*	-0.0217	-0.0487***	-0.0338**	-0.0778***	-0.0016	0.0007	0.0002	0.0002
	(0.0137)	(0.0142)	(0.0135)	(0.0128)	(0.0157)	(0.0185)	(0.0013)	(0.0016)	(0.0002)	(0.0002)
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Panel C: Sales Tax										
Post Treat	0.0307*	0.0278	-0.0276**	-0.0277**	-0.0181	-0.0175	0.0033*	0.0033*	0.0001	0.0001
	(0.0185)	(0.0189)	(0.0117)	(0.0115)	(0.0139)	(0.0148)	(0.0019)	(0.0018)	(0.0002)	(0.0002)
Policy Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

This table shows the effects of large policy increases on startup outcomes at the county level. The estimates are obtained using the DID method by Cengiz et al. (2019).

A large increase refers to the top 5% of the distribution of policy changes. If the 95th percentile was equal to the median (usually zero), the top 1% were used instead. Sales tax, property tax and corporate income tax are sourced from the Upjohn dataset. Personal income tax state is the top marginal rate in the state, sourced from the Tax Foundation.

The dependent variables are listed at the top. When ln(Nonemployers) is the dependent variable, only startups in their first year (year 0) in the CSP are included in the sample before aggregating to the county level. This represents the number of newly founded nonemployers. For all other dependent variables, the sample includes all CSP startups (years 0-7) before aggregating to the county level. For example, ln(Employment) is the total employment of startups within their first 8 years in a county.

Policy control variables include total taxes, total incentives, and minimum wages, excluding total taxes (incentives) when focusing on tax (incentive) events. When we focus on events on single taxes (incentives), we additionally control for the other single taxes (incentives). Pre-treatment control variables include firm age shares 0-7 (except when ln(Nonemployers) is the dependent variable), ln(Population), percentage of individuals aged 25 to 64, high school graduation percentage, college graduation percentage, female percentage, African American percentage, Asian percentage, and Hispanic percentage.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

Table B5: Effects of Large Policy Changes on Startup Outcomes (Alternative Policy Thresholds)

	(1) Policy	(2) ln(Nonemployers)	(3) ln(Employers)	(4) ln(Employment)	(5) Exit Rate	(6) Move-out Rate
Panel A: Total Tax Increases (99% Threshold)						
$Treat_{ct}$	0.0070***	-0.0160***	-0.0354***	-0.0258	-0.0040***	-0.0000
	(0.0004)	(0.0058)	(0.0069)	(0.0202)	(0.0011)	(0.0002)
No. of Obs.	37,500	37,500	37,500	37,500	37,500	37,500
Panel B: Total Tax Increases (90% Threshold)						
$Treat_{ct}$	0.0059***	0.0175*	-0.0393***	-0.0801***	0.0006	-0.0008***
	(0.0001)	(0.0097)	(0.0073)	(0.0173)	(0.0013)	(0.0002)
No. of Obs.	32,000	32,000	32,000	32,000	32,000	32,000
Panel C: Total Tax Decreases (1% Threshold)						
$Treat_{ct}$	-0.0095***	-0.0180**	-0.0026	0.0259	-0.0045***	0.0006**
	(0.0002)	(0.0080)	(0.0082)	(0.0232)	(0.0013)	(0.0003)
No. of Obs.	37,000	37,000	37,000	37,000	37,000	37,500
Panel D: Total Tax Decreases (10% Threshold)						
$Treat_{ct}$	-0.0076***	-0.0236***	0.0091	0.0336*	-0.0060***	0.0002
	(0.0002)	(0.0067)	(0.0060)	(0.0180)	(0.0011)	(0.0008)
No. of Obs.	35,000	35,000	35,000	35,000	35,000	35,000

Notes: This table reports robustness checks of county-level effects of large policy changes on startup outcomes using the DID method of Callaway and Sant'Anna (2021). In the baseline specification, a "large" change is defined as being above the 95th percentile (or below the 5th percentile) of the distribution of policy changes. Here, we alternatively use more extreme thresholds at the 99th/1st percentiles, as well as broader thresholds at the 90th/10th percentiles.

The dependent variables are listed in the column headers. When ln(Nonemployers) is the dependent variable, the sample includes only first-year (year 0) CSP startups before county-level aggregation. For other outcomes, the sample includes all CSP startups (years 0–7). For example, ln(Employment) measures total employment of startups within their first eight years in a county.

Pre-treatment controls include firm age shares 0–7 (except when ln(Nonemployers) is the dependent variable), ln(Population), percentage aged 25–64, high school graduation rate, college graduation rate, female percentage, African American percentage, Asian percentage, and Hispanic percentage. Standard errors are in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

Table B6: Effects of Large Policy Changes on Startup Outcomes (Firm Level)

	(1) Transition	(2) ΔEmployment	(3) Exit Rate	(4) Move-Out Rate
Panel A: Total Tax Increases				
Total Tax Increase$_{ct}$	0.00013	0.00636	0.00048	0.00034
	(0.00034)	(0.03630)	(0.00325)	(0.00163)
No. of Obs.	111,700,000	176,300,000	201,100,000	200,900,000
Panel B: Total Tax Decreases				
Total Tax Decrease$_{ct}$	0.00033	0.05300	-0.00164	0.00113
	(0.00060)	(0.04490)	(0.00772)	(0.00395)
No. of Obs.	111,700,000	176,300,000	201,100,000	200,900,000
Panel C: Total Incentive Increases				
Total Incentive Increase$_{ct}$	-0.00021	-0.01140	0.00168	0.00130
	(0.00039)	(0.03790)	(0.00812)	(0.00492)
No. of Obs.	111,700,000	176,300,000	201,100,000	200,900,000
Panel D: Total Incentive Decreases				
Total Incentive Decrease$_{ct}$	0.00034	0.01520	-0.00205	0.00017
	(0.00028)	(0.02560)	(0.00471)	(0.00257)
No. of Obs.	111,700,000	176,300,000	201,100,000	200,900,000

This table shows the effects of large policy increases or decreases on startup outcomes at the firm level. Estimates are two-way fixed effects with firm and year fixed effects.

The dependent variables are listed at the top. *Transition* is a dummy indicating transition from nonemployer to employer. ΔEmployment is the change in a firm's employment from one year to the next.

Standard errors in parentheses, clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).