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

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Abstract

Governments spend billions of dollars each year on tax cuts and incentives targeted at businesses, but are these tax policies effective? This study provides the first causal evidence on how a comprehensive set of local taxes and incentives influences startup activity and outcomes. Leveraging new restricted-access administrative panel data on the universe of U.S. startups, we employ difference-in-differences designs exploiting large policy changes. Key findings include: (1) Tax increases significantly reduce startup numbers and employment; tax cuts have nearly symmetric positive effects. (2) Corporate taxes have the most robust impact on startup activity. (3) Enacted incentives stimulate startup formation and reduce exits (notably R&D tax credits); this stimulus is asymmetric, not equally reversed by incentive cuts. We do not find evidence that startups relocate to different states as state policies change, implying responses in total numbers of startups and corresponding broader welfare effects.

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

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

–Jean-Baptiste Say, economist who popularized the term "entrepreneur"

1 Introduction

Entrepreneurs generate a cascade of positive externalities. In addition to raising welfare for consumers and contributing revenue for public goods, entrepreneurs drive employment growth and wage competition in the market for workers (Moretti, 2012; Haltiwanger et al., 2013; Decker et al., 2014, 2016; Christensen, 2015; Sorenson et al., 2015). Indeed, the scarcity of entrepreneurs appears to be a limiting factor in job creation, due to the fact that firms can effectively manage only so many workers (Keren and Levhari, 1979; McAfee and McMillan, 1995; Beaudry et al., 2018). More broadly, entrepreneurs and the startups they create contribute to economic growth, knowledge spillovers and innovation (e.g., Audretsch et al., 2006; Baumol and Strom, 2007; Kerr and Robert-Nicoud, 2020). To foster entrepreneurs and their externalities, therefore, governments spend billions each year to encourage their growth, success and job creation.

In practice, government efforts to stimulate entrepreneurship are wide-ranging. These efforts usually emphasize cutting tax rates (including on property and sales, as well as on corporate and personal income), and offering business incentives usually through the tax code (tax credits for job creation, investment, R&D, and property tax abatements). Governments also worry about potential deleterious effects on entrepreneurship from raising taxes when facing continuous budget shortfalls, and consider the political feasibility, cost and efficiency of various interventions. Finally, there is concern that incentives might not increase startup activity as intended but instead simply act as windfall for firms who would have engaged in the targeted behavior anyway.

In this paper, we investigate which if any of these approaches actually increases the supply of entrepreneurs and success of the business entities they create. To answer this question we use the most comprehensive data possible on startups combined with detailed data on tax policies. We bring to bear newly available Census administrative records, the Comprehensive Startup Panel (CSP), which covers the universe of U.S. startups and follows these startups over time. The novel CSP includes all types of startups (i.e. both with and without employees, all legal forms of organization, all industries, and all locations in the country), and properly defines the startup year, where previously available data do not (Fairlie et al., 2023).[1] With our rich administrative data, we are able to capture effects on both the extensive margin

[1]The administrative records available up to now, including those from the published tabulations of the Business Dynamics Statistics (BDS) and underlying microdata in the Longitudinal Business Database (LBD), exclude non-employer startups, which represent the vast majority of startups and create a large number of jobs in future years.

(e.g., entry and exit) and intensive margin (e.g., employment growth), allowing us to decompose the overall effect into entry-exit and within-firm components. Using various sources of data, we explore the effects of a comprehensive set of taxes and incentives on these outcomes. We distinguish between the effects of personal income taxes, corporate income taxes, property taxes, sales taxes, investment tax credits, job creation tax credits, R&D credits, and property tax abatements on different types of startups, which allows us to clarify the mechanisms through which various tax structures influence startup decisions. Higher taxes on income, for instance, may reduce the after-tax profits of entrepreneurial ventures, whereas property and sales taxes raise the cost of doing business. While taxes generally impose additional costs that can negatively affect business formation, the specific impact varies depending on the legal structure and scale of the business—such as nonemployer versus employer startups. Furthermore, to address the piecemeal problem with estimating many different tax and incentive effects, we evaluate all tax and incentive changes using consistent selection criteria, avoiding potential publication bias and enabling direct comparison across different policy approaches.

To address selection, we estimate causal effects using new difference-in-differences methods that account for unobserved differences across jurisdictions and national shocks over time. We employ Callaway and Sant'Anna's (2021) difference-in-differences approach which addresses heterogeneity in treatment timing and dynamic treatment effects. This methodology specifically accounts for the well-documented biases in two-way fixed effects models with staggered adoption designs (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). Our approach uses only never-treated or not-yet-treated units as controls, thus avoiding the contamination that occurs when already-treated units implicitly serve as comparisons. Additionally, we implement inverse probability weighting to balance covariates across treatment and control groups, which improves precision and the comparability of treated and untreated units.[2] We also use event studies to provide a means of testing whether treatment and control groups exhibit similar trends prior to intervention and whether effects align temporally with treatment. Our analysis confirms parallel pre-trends, validating the underlying identifying assumptions, and finds that the main effects coincide with the timing of the policy change. The primary vulnerability of the design is that events may be correlated with other policy changes that take place in the same jurisdictions at the same time. To address this, we explore tax policy timing correlations and explicitly control for changes in other policies and find that the results are robust to these controls.

The analysis of taxes and tax incentives reveals several novel findings. We first examine the effects of large tax changes on startup outcomes.[3] The average large tax-hike event increased taxes by 54 basis points

[2]The approach also has the advantage of being "doubly robust," which means that the estimate is consistent if either the propensity score model or the outcome model is correctly specified.

[3]To harness the greatest available statistical power, we focus on the most substantial changes for each policy. Specifically, we

of firm value added, which is 11 percent of the mean tax burden. These events accompany a 5.5 percent reduction in employer startups (the number of startups with employees) and a 6.4 percent reduction in overall startup employment (total employment in startups). Tax-cut events, for their part, reduce taxes by 81 basis points (or 16 percent of the mean). They increase startup employment by 4.6 percent and reduce the annual exit rate of startups by 0.4 percentage point. Taxes, however, may have different effects on startup activity depending on *what* is taxed. We examine the largest events for each specific tax category: taxes on sales, property, personal income, and corporate income. Corporate income taxes (CIT) show clear symmetric effects on startup activity, with tax hikes accompanying a 3.1 percent decline in employer startups and tax cuts accompanying a 4.7 percent increase. Property tax cuts increase employer startups by 2.0 percent and reduce the exit rate by 0.5 percentage points. Personal-income tax (PIT) cuts increase startup activity, but tax increases do not reduce it.

Next, we examine the effect of large changes in tax incentives. The average large incentive increase event we examine raises incentives for firms by 48 basis points of firm value add (a 64 percent increase of the mean incentive rate). These incentive-increase events accompany a 4.2 percent increase in employer startups and a 0.5 percentage point reduction in the yearly exit rate. Since incentives differ substantially in their targets, they likely also differ in their effects. We find research and development (R&D) credits have the most substantial effect on startup activity, increasing employer startups by 7.1 percent. Job creation incentives increase employer startups by 2.8 percent. Investment incentives increase non-employer startups and may slightly reduce exit rates. New incentives appear to have a larger effect than their phase-out.

These findings contribute to the literature on the effects of taxes and incentives on business formation, startup growth and employment. The theoretical net effects of taxation on entrepreneurship are ambiguous (Bruce et al., 2020). Businesses such as sole proprietorships, partnerships, S-corporations, and limited liability companies (LLC) are typically taxed under the PIT system due to their "pass-through" nature, which means that business income is reported on the owner's personal tax return. In contrast, profits of C-corporations are first taxed under the CIT, and distributions or realized capital gains are then subject to the PIT. As wages and salaries are subject to the PIT as well, the choice to be an entrepreneur or a paid employee depends on how the two income taxes change the distributions of after-tax income in entrepreneurship in comparison to paid employment and preferences about these distributions (Bruce, 2000, 2002; Wen and Gordon, 2014; Arulampalam and Papini, 2023). Income taxes reduce the upside of the returns distribution for entrepreneurs and can therefore act as a "success tax" and discourage entrepreneurial activity (Gentry

calculate year-over-year policy changes in each jurisdiction and identify the 5 percent largest increases for each tax or incentive; where data permit, we also identify the 5 percent most substantial *decreases* in each policy. This approach allows us to test for symmetric effects while also ensuring we focus on meaningful policy variation since examining the effect of minor changes is more likely to be swamped with noise.

and Hubbard, 2000). However, higher tax rates could, under certain conditions, increase business formation by shifting some financial risk from entrepreneurs to the government, thus providing a form of insurance and encouraging new business ventures (Domar and Musgrave, 1944; Cullen and Gordon, 2007). Moreover, entrepreneurship provides more opportunities for tax avoidance in comparison to wage employment, so higher PIT rates might induce more business activity (Kleven et al., 2011; Fossen et al., 2020). Concerning the CIT, research shows more consistently that higher CIT rates have a negative impact on entrepreneurial entry (Belitski et al., 2016; Sapollnik and Swonder, 2025) and discourage business expansion and reinvestment (Da Rin et al., 2011; Henrekson and Sanandaji, 2011; Block, 2021). In the most recent work, Sapollnik and Swonder (2025) use tabulations from the Business Dynamics Statistics (BDS)[4] and find that corporate tax cuts increase employer business entry but reduce the predicted growth potential of entrants. They also find no evidence that PIT reforms affect employer business entry. The previous literature, however, has not examined whether the CIT affects the decisions of businesses that are not currently subject to the CIT because they might plan to incorporate in the future, or because the CIT affects investors in their businesses.

Less previous work focuses on the effects of sales and property taxes on entrepreneurs. Sales taxes can affect business costs and consumer pricing strategies, potentially reducing customer sales and affecting overall business viability as firms must adjust prices to account for the tax burden while maintaining competitiveness in the market (Chen et al., 2023). Property taxes are generally found to have a negative effect on small business formation (Bartik, 1985, 1989). Higher property taxes increase the cost of owning or leasing property, which might discourage new businesses from entering the market and constrain the growth of existing startups. Property taxes may be particularly relevant for liquidity-constrained entrepreneurs as they still have to pay this tax in years with low sales and even losses, in contrast to the income taxes (Chen et al., 2023). The previous literature, however, does not separately estimate the effects for employer startups, which often depend on commercial or industrial properties, and nonemployer startups, which are more likely to operate from home and likely affected differently by property tax increases.

While incentives such as tax credits and grants are designed to reduce the tax burden, their potential impact may not be uniformly positive across all types of firms. A concern is that these incentives can be used by governments to support selected projects that benefit very large firms, which may come at the cost of crowding out smaller startups and nonemployer businesses by increasing competition for customers and critical resources such as land, labor, and capital.[5] Indeed, empirical studies on the effectiveness of business incentives have yielded mixed results on employment and business establishments, with estimates ranging

[4]The Census Bureau sources the BDS from the Longitudinal Business Database (LBD).

[5]For example, Amazon received $573 million in 2018 in grants from Virginia for HQ2, Tesla secured over $1 billion in 2020 in incentives from Texas for its Austin Gigafactory, and Wisconsin promised Foxconn over $4 billion in 2017 in incentives for a manufacturing plant.

from large positive effects to small negative effects.[6] Turning to specific types of incentives, job creation tax credits and job training subsidies are designed to help businesses hire and upskill their workforce, possibly encouraging employment, but the empirical evidence is limited (Faulk, 2002; Chirinko and Wilson, 2023). R&D tax credits promote innovation by making research activities more affordable, which has been found to enhance the quality and quantity of entrepreneurship, particularly in high-tech sectors (Cappelen et al., 2012; Fazio et al., 2020), and increase the demand for R&D workers, leading to higher employment opportunities in research-intensive sectors (Chen and Hshieh, 2024).[7] Property tax abatements reduce long-term operating costs and are found to facilitate short-term employment growth, though these effects often diminish over time (Leonard et al., 2020). Meanwhile, investment tax credits provide direct financial incentives for capital investments in machinery, equipment, or technology; however, they may disproportionately benefit larger firms, which could lead to negative spillover effects that crowd out startup activity (Fazio et al., 2020). While business incentives are intended to promote firm growth and job creation and are widespread and expensive, both the theoretical predictions and empirical evidence on their effectiveness for startups are limited and mixed.

Our analysis provides several advantages over the previous literature. First, we leverage newly available administrative records covering the universe of U.S. startups (Fairlie et al., 2023). In contrast, many of the previous studies use small surveys or selected samples of startups and cannot distinguish between employer and nonemployer responses — a critical omission given that 89% of U.S. startups lack employees (Fairlie et al., 2023). The CSP provides a massive and inclusive frame for analysis that comprises all types of businesses, and that correctly times the start of businesses. This comprehensive panel dataset allows us to observe the full spectrum of entrepreneurial activity, from formation to employment growth and survival over the first eight years of firm life. By capturing the complete spectrum of entrepreneurial activity and tax policies, we provide more accurate and generalizable estimates of policy effects than previously possible.

Second, we employ methodologically robust causal identification strategies that address limitations of earlier work and the broader literature on entrepreneurship. For example, past influential studies on regional entrepreneurship relied on cross-sectional comparisons across jurisdictions that make it hard to disentangle causal effects from unobserved factors (e.g., Glaeser and Kerr, 2009; Delgado et al., 2010; Chatterji et al.,

[6] For example, Wang et al. (2020) found that substantial increases in incentives (i.e. 95^{th} percentile of the distribution of incentive increases) resulted in small positive effects on overall employment. Freedman (2013) reported more positive employment effects from hiring incentives in Texas. In contrast, Meurers and Moenius (2020) found no significant impact of incentives on the number of manufacturing firms, and Gabe and Kraybill (2002) found that while economic development incentives in Ohio had positive effects on announced employment growth, the actual growth was marginal or even negative. Partridge et al. (2020) identified negative effects of incentives on overall startup activity.

[7] Government R&D funding is highly concentrated, with a few firms receiving disproportionate support. Although these programs are controversial, targeted programs such as the Small Business Innovation Research (SBIR) program have been found to foster innovation and talent development, especially for small and young firms (Lanahan and Feldman, 2018; Feldman et al., 2022)

2014). Other studies have used panel methods that are now known to produce biased estimates in the presence of staggered treatment timing and heterogeneous effects (Goodman-Bacon, 2021). Our application of the Callaway and Sant'Anna (2021) difference-in-differences approach, including inverse probability weighting to balance covariates across treatment and control groups, provides more credible causal estimates than previous research. We build on this model and also estimate event study regressions to examine pre-trends, timing, and dynamics effects — ensuring that observed changes in outcomes occur only after policy implementation and not before. To account for time-varying concurrent policy changes, we employ the stacked DID approach proposed by Cengiz et al. (2019) as a robustness check adding to the methodological improvements.

Third, we provide a unified framework to compare policy effectiveness. Rather than examining isolated policy instruments in different contexts—as is typical in the literature—we evaluate various approaches (taxes, incentives) using the same country, methodology, and data. We also examine a comprehensive set of local taxes and incentives, enabling a rich analysis of how policies affect business formation and performance. This unified approach allows direct comparisons across policy levers, reducing the risk of publication bias that arises when different policies are studied separately and significant or widely held results are selectively published. Policymakers can thus assess the relative "bang for the buck" of different entrepreneurship promotion strategies.

Fourth, by analyzing various startup outcomes at the intensive and extensive margins, we disentangle the margins through which policies operate. This approach reveals whether observed changes stem primarily from firm creation/destruction or from the growth/contraction of existing young businesses, providing deeper insight into policy mechanisms than studies focused solely on business formation. Additionally, we explore effect heterogeneity with respect to the distance to a state border, industries, growth ambition of the startup, and the introduction of new policies. Analyzing relocation rates of startups and comparing effects near state borders to effects far from state borders, we do not find evidence for startup activity relocating between states in response to state policies, suggesting that our estimated effects reflect changes in total startup activity rather than just reallocation from one state to another.

Finally, our analysis yields several novel empirical findings that challenge or refine existing understanding. We find that corporate taxes have the strongest effect among tax instruments, while R&D credits show the largest impact among incentives. Tax increases or reducing incentives do not always act as reverse stimulus policies. New businesses with employees react differently than new businesses without employees to various taxes and incentives. These nuanced results provide actionable insights for policymakers seeking to foster business formation and growth and have broader implications for economic policies and optimal taxation.

2 Data and Measures

We draw entrepreneurial outcomes from the firm-level Comprehensive Startup Panel (CSP), while the key explanatory variables are sourced from the Panel Database on Incentives and Taxes (PDIT) and the Tax Foundation. Additionally, control variables are collected from the Bureau of Economic Analysis (BEA), the American Community Survey (ACS), and the County Intercensal Datasets provided by the U.S. Census Bureau. We analyze the effects of several policies that potentially affect startups, including total taxes, total incentives, CIT, PIT, property tax, sales tax, job creation tax credits, investment tax credits, R&D credits, and property tax abatement. The goal is to examine a comprehensive set of tax policies commonly used by states to attract startups and create jobs.

2.1 Measurement of Entrepreneurial Outcomes

Our analysis is based on the CSP, a new compilation of administrative panel data on the universe of business startups in the United States within their first eight years of existence (Fairlie et al., 2023). We use data covering 2001 to 2015. The CSP merges two critical Census Bureau data sources: the Longitudinal Business Database (LBD), including all non-farm employer establishments from 1976, and the Integrated Longitudinal Business Database (ILBD), covering nonemployer establishments with quinquennial data from 1977 to 1992 and annual data thereafter. This integration uses revenue activity to identify business births and provides measures of job creation and survival among all startups. The CSP documents every business entity that files taxes or a tax report.[8]

A novelty of the CSP is that it provides information to track nonemployer startups and the pre-employment years of employer startups. An examination of early-stage job creation among startups requires identifying nonemployer startups and their transitions to employer status. Since the CSP links the employer and nonemployer universes, it provides an accurate identification of the startup time for business units, allowing each annual cohort of startups to be followed longitudinally. The LBD and published tabulations of the BDS instead define startups as the first year of having employees.[9] Furthermore, the CSP includes detailed information on businesses, such as geographic location, establishment year, employment dynamics, payroll distributions, legal form, industry, and exit status.

[8]The CSP identifies startups through a tailored algorithm that prioritizes the oldest establishments, resolving ties by considering employment, payroll, and random selection among equals. This precision allows for the accurate tracing of business entities across the economic landscape, excluding mergers and acquisitions which are treated as continuations of existing businesses (Fairlie et al., 2023). A business is only included in a startup cohort if it has not appeared in either the nonemployer or employer universes in the previous seven years.

[9]Compared to the employer establishments data in the County Business Patterns (CBP) dataset and the nonemployer establishments data in the Nonemployer Statistics (NES), both provided by the U.S. Census Bureau, the CSP has its unique advantages. While CBP and NES primarily focus on stock data of existing businesses, the CSP provides insights into new startups.

Our analysis primarily focuses on five startup outcomes sourced from the CSP: the numbers of new nonemployer startups and employer startups and total employment by startups in a county and year, the annual exit rate of startups, as well as the rate at which startups relocate out of a county into other states (move-out rate). The count of new nonemployer startups refers to their first year of existence and can be interpreted as market entries. All other measures refer to the first eight years of a startup's existence. This allows us to track employment growth and survival of startups over their early life. The primary unit of analysis is at the county level, we supplemented the analysis with firm-level data for additional insights.

2.2 Measurement of Business Taxation and Incentives

Policymakers have adopted a range of targeted policies, including tax cuts and incentives, to encourage startup creation and job growth (Bartik, 1985). There are two major types of policies thought to affect startup formation and their employment: (i) taxes, including the total tax burden, sales taxes, property taxes, CIT, and PIT; and (ii) incentives, such as investment tax credits, job creation tax credits, R&D credits, and property tax abatements. While the existing literature has concentrated on the effects of individual policy choices on startup formation, this study examines the effect of all of these tax and incentive policies on several startup outcomes, allowing for an "apples-to-apples" comparison.

Data on taxes and incentives are mostly sourced from the PDIT, a new dataset produced by the Upjohn Institute for Employment Research (Bartik, 2017). The PDIT provides state-level information on a broad range of incentives and taxes for a typical new firm across 33 states (as shown in Figure A1 in Appendix A), covering the period from 1990 to 2015. These 33 states account for over 90% of both U.S. output and U.S. population. The states that are excluded are small (most have less than 2 million inhabitants), and many are mountain or non-contiguous states.[10] The total taxes calculation includes: (a) property tax, (b) sales tax, and (c) CIT. Local-specific tax types, such as local CIT and public utility taxes, are excluded from the total taxes calculation. The total incentive calculation includes five types: (a) property tax abatements, (b) job creation tax credits, (c) investment tax credits, (d) R&D tax credits, and (e) customized job training subsidies. We use the total as well as the specific taxes and incentives in our analysis.[11]

The PDIT simulates taxes and incentives by assuming the establishment of a new business in a specific city and state (47 cities within the 33 states are considered in the simulations), industry, and year. Taxes and incentives for the new business are projected over the first 20 years of operation, assuming the business maintains a constant operating scale.[12] By combining the firm's balance sheet data with state and local

[10] The states excluded are Alaska, Arkansas, Delaware, Hawaii, Idaho, Kansas, Maine, Mississippi, Montana, New Hampshire, North Dakota, Oklahoma, Rhode Island, South Dakota, Utah, Vermont, and West Virginia.

[11] We do not analyze job training subsidies separately due to limited variability over time.

[12] The present value of the 20-year stream of taxes and incentives is computed using a 12% discount rate.

business tax rates and applying rules for how incentives are determined based on firm characteristics, state taxes and incentives are calculated for each year. Industry-specific taxes and incentives are aggregated to the state level using value-added as the weight. The novelty of the PDIT lies in its provision of a comprehensive and standardized set of taxes and incentives applicable to business entry across specific states and industries.[13]

As the PDIT does not include information on PIT, we add nominal marginal state PIT rates for the highest tax brackets, sourced from the Tax Foundation (Tax Foundation, 2021). The Tax Foundation provides data on state-level tax policies for all states, as shown in Figure A2. Local PIT—imposed by approximately ten states at the county or city level—are excluded to ensure consistency across states.

2.3 Measurement of Control Variables

In addition to the policy measures, control variables are included in our models to account for the influence of other factors that the literature shows to be related to entrepreneurship. A set of socioeconomic controls are sourced from various databases. Population data at the county level is obtained from the BEA, part of the U.S. Department of Commerce. Demographic variables, including the shares of the working-age population, high-school graduates, college graduates, female, African American, Asian, and Hispanic populations, are sourced from the ACS for the years 2010 to 2021. In the ACS, detailed demographic data are not consistently available for years prior to 2010. To cover this gap, we supplement the demographic data for the period 2001 to 2009 with the County Intercensal Datasets provided by the U.S. Census Bureau, including variables such as the share of the working-age population and shares by gender, race, and ethnicity. To obtain educational attainment variables for the period before 2010, we draw on data from the Economic Research Service (ERS) from 2001 to 2009.

3 Methods

We employ difference-in-differences (DID) designs including event studies to estimate the causal average and dynamic impacts of large changes in local taxes and incentives. We use the method suggested by Callaway and Sant'Anna (2021), which addresses issues related to staggered treatment timing in the presence of heterogeneous treatment effects. The approach does not rely on already-treated units to serve as controls. We use both never-treated and not-yet-treated counties as the control group.

[13]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.

The DID model is specified to examine the average treatment effect of a policy regime change on startup outcomes in the post-period, as follows:

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

Y_{ct} represents 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. The other 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, and the rate of startups moving out of the county into a different state. $Treat_{ct}$ is a dummy variable that equals 1 for observations in the treatment group after counties experienced either a large increase or a large decrease in a tax or incentive (estimated separately). A large increase (decrease) is defined as a policy year-to-year change larger than the 95^{th} percentile (smaller than the 5^{th} percentile, respectively) of the distribution of changes, and 0 otherwise, following Wang et al. (2020) and Duan et al. (2024).[14] The coefficient of interest, β_1, captures the effect of the policy change on the startup outcome.

Controls, X_{ct}, include the logarithm of total county population, shares of the working-age population, high school and college graduates, and demographic shares of the female, African American, Asian, and Hispanic populations. We also control for the average age of the startups in the county and its square when estimating outcomes of existing startups (thus, for all outcomes except the number of new nonemployers). County and year fixed effects, γ_c and τ_t, capture county-specific time-invariant effects and common shocks. We find that the different policy events tend to be uncorrelated with one another. More importantly, when we control for other policy changes explicitly in a robustness check, it makes essentially no difference in the estimates.

We use the doubly-robust DID estimator based on stabilized inverse probability weighting and OLS outcome regression adjustment (Callaway and Sant'Anna, 2021). The approach uses only never-treated or not-yet-treated units as controls, avoiding the contamination that occurs when already-treated units implicitly serve as comparisons. This methodology directly counters the negative weighting and treatment effect contamination problems that arise in standard difference-in-differences models when treatment timing varies and effects are heterogeneous across units or time periods. Additionally, the inverse probability weighting balances covariates across treatment and control groups, improving comparability by weighting observations according to their likelihood of receiving treatment. The doubly-robust approach ensures that our estimates remain consistent if either the propensity score model or the outcome model is correctly

[14] If the 95^{th} (5^{th}) percentile of a policy change equals the median, we use the 99^{th} (1^{st}) percentile instead to define large increases (decreases, respectively). If the 99^{th} (1^{st}) percentile also equals the median, we do not analyze increases (decreases) in this policy variable.

specified, providing two opportunities to get the right specification and making our estimates more robust to model misspecification. The sample is trimmed to include up to 6 pre-treatment and 10 post-treatment years in each 2x2 DID experiment. Standard errors are based on the multiplicative Wild Bootstrap procedure with 1,000 repetitions and clustered at the state level.

The event study model quantifies the dynamic effects of policy changes over time, centered around a pivotal year in which a significant policy change occurs. Additionally, this model serves to evaluate pre-event trends: the event study enables an examination of parallel trends prior to the policy implementation. This allows to assess whether apparent effects are just continuations of pre-existing trends and whether effects are consistent with the timing of the policy change. The model is constructed as follows:

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

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 captures the differential time-specific effects between the treatment and control groups for each year relative to the event, extending from 5 years before the event to 10 years after, thereby encompassing the periods both prior to and following the policy implementation. Event time -6 is included in the sample and serves as the omitted category. The fixed effects γ_c and τ_t control for unobserved heterogeneity across counties and calendar time.

To assess the validity of the Callaway and Sant'Anna (2021)'s method, we examined the first-stage relationship between the treated variable—defined as large increases or decreases in policy—and the corresponding policy level itself. The results demonstrate that the changes in policy variables following large increases or decreases are highly significant, as expected. The corresponding event study graphs, such as those presented in Appendix A in Figures A12 for taxes and A13 for incentives, show consistent patterns with sharp changes at the time of the treatment and stable pre-treatment trends, supporting the validity of the DID approach in capturing the effects of large policy shifts.

4 Empirical Results

4.1 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

startups. Table 1 (Panel A) presents summary statistics for the startups that we pull from the CSP. Our included startups cover the years 2001–2015 and include 201.1 million annual observations. Average employment per startup is 1.4 workers, which is consistent with the small scale of the universe of startups and predominance of nonemployer startups relative to employer startups in the U.S. Changes in employment and transitions from nonemployer to employer status are low, which at least in some cases underscores the challenges small businesses face in scaling up. On the other hand, exit rates are high with 30 percent of startups exiting on average each year, indicating a high degree of turnover. The dynamics of business relocation is captured by move-in and move-out rates. The share of startups moving out of a county into a different state is on average 1.2 percent per year, and the share among startups in a county that moved in from a different state in a year is 2.3 percent.

The findings indicate that startup activity in the U.S. exhibits both volatility and potential contribution to job creation. While exit rates are high and the transition rate from nonemployer to employer status remains relatively low, startups nonetheless serve as an important driver of employment and economic dynamism. Nonemployer businesses underscore the accessibility of entrepreneurship, yet their limited scalability suggests barriers to firm growth. The documented regional business mobility may reflect both competitive reallocation and the role of local economic conditions in shaping entrepreneurial activity. These patterns highlight the inherent trade-offs in the startup ecosystem: while many new ventures struggle to persist, those that successfully scale contribute meaningfully to job creation.

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.[15] Additionally, state top marginal PIT rates obtained from the Tax Foundation average 5.23% of the income tax base. The overall incentives average 0.75% of value-added, with the property tax abatement being 0.26%, job creation tax credits being 0.24%, investment tax credit being 0.16%, and R&D credits being 0.04%. For total incentives, there is a general pattern where states with higher total tax burdens, such as New Mexico, Iowa, New York, and Pennsylvania, tend to have higher incentives to offset the burden of taxes and encourage startups. In contrast, states with low total taxes, like Nevada and Wyoming, typically also provide fewer incentives, reflecting a more hands-off approach in both tax and incentive policies.

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 reported startup outcomes capture various aspects of startup activity and business dynamics. The averages across counties are 5,600 for total startups and 8,000 for total employment

[15]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.

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

at these startups. Numbers are rounded for Census disclosure. The number of nonemployer startups, including sole proprietorships and microenterprises, average 4,700 per county, while the number of employer startups average 900 per county. The annual transition rate from nonemployer to employer is 0.9% per year on average and the exit rate is 29.9% per year. These numbers are slightly different than in Panel A because Panel C shows averages across counties with equal weight for all counties.

Panel C also reports means for several control variables that capture demographic and economic characteristics at the county level. In our regresssions, business characteristics are controlled for using county-level age distributions of the startups. The average age of the startups in the CSP is 2.35 years, with a large proportion of firms 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 last year firms are defined as startups and included in the CSP.

4.2 Estimation Results

We estimate the effects of large changes in each tax or incentive policy on startup activity and dynamics. We report aggregate average treatment effects on the treated (ATT) of a large increase or decrease event in a policy on five startup outcome variables (Equation 1). The size of the policy increases and decreases, i.e., the size of the treatments analyzed, are also reported in each table, showing the effect of a policy change event on the policy.

Table 2 reports empirical results for the effects of large increases in taxes. By increasing the fixed costs or reducing the returns to entrepreneurship, tax increases might reduce startup activity. Focusing initially on total tax increases, Panel A of Table 2 reveals that substantial increases in total taxes negatively affect entrepreneurial activity. The treatment, a large increase in total taxes, corresponds to 0.054 percentage points of total value added of a firm (see Column 1 and Appendix Figure A12a), which is about 11% of the average (which is reported in Table 1). Such an increase in total taxes reduces the numbers of nonemployer startups by 2.91% and of employer startups by 5.46%. The adverse effect on total employment in startups is even more pronounced, with a decline of 6.40%, highlighting the disproportionate sensitivity of larger-scale startups to extra tax burdens. These estimates imply total tax elasticities of -0.27 for nonemployer startups, -0.50 for employer startups, and -0.59 for employment in startups. In contrast, we find estimates of null effects on exit rates and relocation rates out of the state, suggesting that tax increases primarily suppress the formation of new ventures rather than trigger widespread closures or relocation.

Event study plots (Equation 2) for total tax increases are shown in Figure A3 for nonemployer startups, employer startups and total startup employment. The dynamic patterns do not reveal pre-trends in startup outcomes. Additionally, the event study estimates indicate that the changes in startup outcomes correspond

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).

closely with the timing of tax increases, as the effects are significantly negative in the first year after the event, which takes time in year 0.

The finding of negative effects of increases in taxes on startup outcomes does not necessarily imply that decreases in taxes will have opposite effects of a roughly similar magnitude. We examine if there is evidence of symmetry in tax effects by turning to estimates of the effects of large decreases in taxes. The results for total taxes are reported in Panel A of Table 3. Consistent with symmetry, large decreases in total taxes generate positive effects on total employment. A large cut in total taxes increases total employment by 4.55% (Figure A4b). However, the effect on new nonemployer startups is negative (Figure A4a). This might suggest that solo-run businesses do not benefit directly from tax reductions, or, alternatively, that startups are more likely to begin immediately as employers after a large tax reduction. By decomposing total taxes into the specific taxes next, we will see that this effect on nonemployers is driven by property taxes. Also, inconsistent with symmetry we do not find evidence of a positive effect on the number of employer startups, and we find evidence that large tax decreases reduce startup exit rates.

Having found that total taxes affect startups, the next question is which specific taxes contribute to this effect. To clarify the mechanisms through which various tax structures influence startup decisions, we distinguish between the effects of personal income, corporate income, property and sales taxes on different types of startups. Tables 2 and 3 (Panels B-E) examine the effects of increases and decreases in four specific state taxes on the startup outcomes. The results are somewhat heterogeneous, but generally confirm the total-tax pattern for certain taxes. Property taxes, PIT, and CIT exhibit significant effects, whereas sales taxes have only marginal impacts on entrepreneurial outcomes. Property taxes, which constitute approximately 47% of total state taxes, significantly affect startup outcomes. Panel B of Table 3 indicates that a large reduction in property taxes negatively impacts new nonemployer startups but positively affects employer startups and total employment (which is confirmed by the event study Figures A5b and A5c). A large property tax reduction lowers the fixed costs associated with scaling up. Consequently, some ventures that might have started as nonemployer startups start as employers instead, thereby reducing the immediate number of new nonemployer startups but enhancing long-term growth and job creation. This reallocation aligns with the significant positive impact of property tax reductions on total employment. Additionally, exit rates decline significantly by 0.53 %-points, reflecting the stabilizing influence of property tax cuts on existing businesses (see also Figure A5d).

Panel C of Table 2 shows that a large increase in CIT reduces nonemployer startups by 2.48% and employer startups by 3.13% (Figure A6b). Mirroring these effects, Panel C of Table 3 demonstrates that a large decrease in CIT increases nonemployer startups by 3.29%, employer startups by 4.71% (Figure A6c), and total employment in startups by 3.91%. Similarly, a decrease in top PIT rates boosts nonemployer

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).

startups by 2.1%, employer startups by 1.96%, and total employment by 4.97% (Panel D of Table 3), although Figures A7a and A7b show that these effects unfold with a delay of three years after the tax cut at event time 0, presumably because it takes time to adjust and hire workers. Increases in top PIT rates yield negative but insignificant effects across all outcomes.

Turning to total incentives offered by states, Table 4 highlight positive effects on entrepreneurial activity. Panel A of Table 4 demonstrates that a large increase in total incentives leads to a 4.21% increase in employer startups and a 5.24% increase in total employment, with a slight reduction in exit rates by 0.53 %-points. The dynamics are illustrated in event study Figures A9a and A9b. These results indicate that incentives are particularly effective in fostering employer startups.

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).

When tax incentives are removed the evidence on effects is not as clear. Decreases in total incentives offered by states are reported in Panel A of Table 5. We find a decrease in nonemployer startups but null effects for most other outcomes. Startups do not appear to exit when tax incentives are reduced.

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).

Since incentives differ substantially in their targets, we turn to testing whether they differ in their effects. For example, job creation tax credits may increase employment growth, while R&D subsidies support research activities—potentially increasing the productive capacity of new firms. And, tax credits for investment lower equipment costs, and property tax abatements may reduce long-term operating costs if they decrease after-tax prices in equilibrium. A closer examination of specific types of incentives reveals varying levels of

effectiveness. Among all incentives, investment and R&D tax credits have the clearest positive effects on startups. A large increase in investment tax credits results in a 1.92% rise in employer startups, as shown in Panel C of Table 4 and Figure A10a. Conversely, reductions in investment tax credits, detailed in Panel C of Table 5 and Figure A10b, negatively affect total employment, emphasizing their role in supporting labor-intensive growth. Similarly, R&D tax credits exhibit strong effects on employer startups. Panel D of Table 4 shows that a large increase in R&D tax credits drives a 7.05% increase in employer startups and a 7.50% rise in total startup employment. This suggests that R&D support helps new tech or innovation-oriented firms to scale up their hiring.

Other types of incentives, such as property tax abatements and job creation tax credits, show more moderate and less consistent effects. For instance, large increases in job creation tax credits increase employer startups by 2.82%, as shown in Panel B of Table 4. This suggests that lowering the cost of labor through credits encourages entrepreneurs to launch firms that create jobs. However, property tax abatements show mixed results, with increases in these incentives negatively affecting employer startups, while decreases have adverse effects on nonemployer startups. One interpretation of the former effect is that property tax abatements may often benefit existing firms or relocating firms more than smaller startups.[16] Thus, ramping up abatement does not necessarily translate into more startups, and could even divert resources or attention away from entrepreneurship. Still, the other outcomes (employment and exit rates) are not significantly harmed by abatements, so the main takeaway is that abatements show limited efficacy for spurring startups.

In summary, the response to policy changes differs notably between nonemployer and employer startups. Our results consistently show that employer startups are more sensitive to taxes (except for PIT) and incentives, while nonemployer startups benefit more from PIT reductions.

4.3 Heterogeneity Analysis

We explore potential effect heterogeneity with respect to the distance to a state border, industries, new introduction of policies, and an indicator of growth ambition.

Distance to state borders: We found no evidence that large changes in taxes or incentives result in relocation of existing startups, as there are no significant effects on the move-out rate at the 5% level (Tables 2, 3, 4, and 5). However, there are still two possible explanations for the significant estimated effects of local policies on the number of local startups: Policies could influence at the margin whether businesses are created or not at all, or they could merely influence whether they are created in one state versus another offering different policies. This distinction is important because the former mechanism would imply broader

[16] For example, Texas Tax Code Chapter 313 offers significant property tax limitations primarily benefiting large-scale projects rather than small startups.

welfare effects than the latter. To approach this question, we explore geographical heterogeneity by running separate estimations for border counties (counties located along a state border) and interior counties (away from state borders). The idea is that nascent entrepreneurs living near a state border face lower costs of starting a business in the neighboring state than entrepreneurs living further away from the nearest state border. These costs include commuting or moving costs as well as the difficulty of maintaining local networks after moving. Therefore, if entrepreneurs respond to policy changes by starting new ventures in a neighboring state or by relocating startups across state lines, we expect to observe larger effects in border counties than in interior counties.

Figures A14 and A15 in Appendix A compare the effects of large changes in total taxes and total incentives on startup outcomes in border and interior counties. We find no systematically differential responses consistent with cross-border mobility or competition, as the 95% confidence intervals of the estimates largely overlap.[17] Taken together with the insignificant effects on the move-out rate, the evidence indicates that cross-state reallocation is unlikely to be the primary channel through which policy changes affect startups. Instead, the results suggest that policy changes generate real adjustments in the total numbers of startups rather than redistributing activity across state lines—i.e., the effects are not simply the outcome of a zero-sum contest among states.

Industries: Certain industries are more directly exposed to specific taxes. For example, retail activity may be immediately affected by sales taxes through consumer demand, while manufacturing is capital-intensive and may therefore be particularly sensitive to property taxes. To capture such heterogeneity, we estimate the effects by industry. As shown in Figure A8b, sales tax increases reduce employment in the retail industry, with consistently negative point estimates across post-treatment periods. Thus, the retail industry stands out, as the baseline estimation including all industries had yielded a negative effect on employment of smaller magnitude that had not been statistically significant (Table 2 and Figure A8a). Figure A5e shows that property tax decreases significantly boost employer startups in manufacturing, with effects exceeding the overall 2.00% increase found in the baseline regressions (Table 3 and Figure A5b). These results confirm that policy impacts are not uniform across sectors but are stronger in industries most directly tied to the relevant tax base.

New introduction of policies: Another dimension of heterogeneity comes from new introductions of policies, that is, cases where a policy emerges from zero for the first time. For most tax and incentive policies, such introductions are rare because these policies have long universally existed. However, introductions account for a substantial share of the large changes in the R&D tax credit and the job creation tax credit

[17] The standard errors are generally larger for both groups of counties than in the main estimations using all counties due to the smaller sample sizes. The negative effect of total tax increases on the exit rate is only confirmed for border counties, but this outcome variable measures closures not including relocations.

(44–48% of the large increases). Table 4 reports the baseline estimates for all large policy increases, while Table B1 in Appendix B restricts the events considered to introductions only. The results suggest that the positive effects of these incentives on startups are stronger for policy introductions, implying that incremental expansions of existing programs are not as effective. For instance, the introduction of an R&D tax credit increases nonemployer startups by 2.35% and employer startups by 7.51% (see also Figure A11)—effect magnitudes that exceed those of the pooled large changes in Table 4. Likewise, the introduction of a job creation tax credit raises employer startups by 4.86% and employment by 7.32%. These findings indicate that new policy introductions—particularly in R&D and job creation tax credits—generate stronger impacts on startup activity than subsequent modifications of already existing incentives.

Growth ambition: The universe of nonemployers in our main sample includes newly started one-person business entities that might have no intention of growing or hiring workers. One way to identify nonemployers with more growth ambition in the data is to screen for nonemployers that register for an employer identification number (EIN). These nonemployers typically intend to hire or incorporate in the future. As a heterogeneity analysis, we only keep EIN-holding nonemployers in the sample. The results for total tax and incentive increases and decreases appear in Table B2. Comparing these estimates to those in Tables 2-5, we find that the estimated effects of three of these policy changes on the number of nonemployers with EIN have the same signs as the effects on all nonemployers, but the point estimates are closer to zero, making the effects of decreases in total taxes and incentives insignificant. In contrast, the estimated effect of increases in total incentives on nonemployers with EIN is positive and significant, in line with to the effect on employers. In fact, except for the case of total tax increases, the responses of nonemployers with EIN to policy changes become more similar to the responses of employers. This seems plausible because nonemployers with EIN are likely to be planning to become employers in the future.

4.4 Robustness Checks

A possible concern with our analysis could be that, when we analyze the effects of a large change in one policy, other policies might also change. State governments might reduce one type of tax and increase another type to maintain neutral revenues or for political appeasement, or they might reduce taxes and incentives simultaneously to help balance their budget. To analyze whether this happens systematically, we repeat our DID estimations assessing the effects of large policy changes, but use other policies as the dependent variable. We estimate these models at the state level using otherwise the same methods as before based on Callaway and Sant'Anna (2021), including the same control variables, because only residual correlation would be potentially problematic. Table B3 provides the matrix of results. The policy events (increases

and decreases) are shown in the columns, and treatment effects on other policies in the rows. We do not estimate effects of specific tax change events on total taxes and of specific incentive change events on total incentives as these are mechanically correlated. The table shows that in most cases, the tax policy events are not significantly associated with changes in other policies. There are some exceptions. For example, large increases in property tax abatement are correlated with increases in property taxes, and large decreases in property tax abatement with decreases in property taxes, which seems plausible.

Although policy changes are mostly uncorrelated with each other we conduct the following set of robustness checks directly controlling for changes in policies other than the focal policy. The DID method for panel data used in our main analysis suggested by Callaway and Sant'Anna (2021) only uses pre-treatment values of the control variables and does not allow for time-varying controls. Therefore, in this robustness check we use the stacked DID approach introduced by Cengiz et al. (2019) instead. This allows us to incorporate concurrent changes in other policies, thereby addressing potential omitted-variable bias from simultaneous reforms. We run the stacked DID regressions with and without adding controls for contemporaneous policy changes. The results for increases in the tax policies included in the PDID data appear in Table B4, comparing the estimates with and without other policy controls in adjacent columns (see row "Policy Controls"). The key finding is that the results are similar with or without the inclusion of other policy controls. This consistency illustrates that the main results are not driven by other concurrent policy changes. If anything, some of the estimated effects are larger in absolute terms when including other policy controls, suggesting that our main estimations without policy controls are conservative estimates.[18]

Another set of robustness checks considers alternative thresholds for defining large policy changes. In our baseline analysis, a "large" change is defined as being above the 95^{th} percentile (or below the 5^{th} percentile) of the distribution of policy changes. Following Cavallo et al. (2013), we re-estimate the regressions using more extreme thresholds at the $99^{th}/1^{st}$ percentiles and broader thresholds at the $90^{th}/10^{th}$ percentiles. The results, reported in Table B5 for total tax changes, are mostly consistent with our main findings: total tax increases reduce employer startups and employment, while total tax decreases reduce exit rates. An exception is the estimated effect of tax increases on nonemployers, which becomes positive when using the 90% cutoff, although not significant at the 5% level. It is possible that nonemployers respond differently depending on the scale of a tax increase. Overall, this test suggests that most results are not very sensitive to different definitions of large policy reforms.

[18]This robustness check also shows that the results differ somewhat depending on whether the method of Callaway and Sant'Anna (2021) or stacked DID (Cengiz et al., 2019) is used. We present the results based on the Callaway and Sant'Anna (2021) method as the preferred main results because the stacked DID implicitly uses weights determined by the number of treated units and the variance of treatment within each stacked event, which is arbitrary from an economic perspective (Roth et al., 2023).

5 Conclusion

This study provides evidence that local taxes and incentives affect extensive and intensive startup activity in numerous ways. For the first time, we analyze newly available administrative panel data that tracks the universe of startups (of all types) in the United States and a comprehensive set of tax policies. Our findings demonstrate that the effects of these tax policies vary substantially by both the type of tax policy and the type of startup impacted. The prior literature has not fully revealed these differences because of data limitations and focus on specific taxes or incentives instead of a comprehensive set as studied here.

The analysis provides several novel findings. First, we show that total local taxes predominantly affect the formation and expansion of employer startups. Increases in total taxes significantly decrease the number of employer startups and their employment, while decreases in total taxes have the opposite effect, fostering employment growth and reducing exit rates. The distinct responses of nonemployer and employer startups to fiscal policies may be driven by differences in scale, resource needs, and operational constraints. As employer businesses are more likely to be subject to the CIT, consistently we find that CIT reductions mostly drive employer startup growth, whereas PIT reductions benefit nonemployer businesses more. Thus, targeted tax reforms could be effective in fostering different segments of entrepreneurship.

We also find that incentive policies are effective in supporting startup growth and survival. Expanding investment and R&D tax credits significantly increases employer startup formation. In addition, investment credits reduce exit rates, and R&D tax credits drive employment growth, highlighting their role in fostering innovation. Expanding total incentives—such as investment and R&D credits—significantly increases employer startup formation and employment, whereas decreases have insignificant effects, indicating that increases and decreases of incentives do not work symmetrically. Compared with the more immediate effects of tax policy upon implementation, incentive policies typically have lagged effects. States experience larger positive effects when they first introduce the incentive, such as R&D tax credits and job creation tax credits, in comparison to incremental expansions.

The findings from the Callaway and Sant'Anna (2021) difference-in-differences approach are confirmed with results from event study regressions that examine pre-trends, timing, and dynamics effects. Indeed, we find that observed changes in outcomes occur only after policy implementation and not before. To account for time-varying concurrent policy changes, we also employ the stacked DID approach proposed by Cengiz et al. (2019) as a robustness check and find that the results are robust to including policy controls. The results are also mostly insensitive to alternative thresholds used to define policy changes. Pushing the analysis further, we examine heterogeneity along organizational form, industry, and geography. We find that nonemployers with EIN, an indicator of growth ambition, are more similar to employers than other nonemployers in terms

of their reactions to policy changes. The impacts also vary by industry: sales taxes disproportionately affect retail, while property taxes weigh more on manufacturing.

Our findings provide several policy insights. Governments need to consider tax policy options carefully weighing how to minimize burdens on new and small businesses, as excessive tax increases reduce startup formation. Incentive policies also represent a potentially effective tool, particularly for employer startups that benefit from investment, R&D, and job creation tax credits. Governments considering cutting these programs for budgetary reasons might disproportionately harm startups creating jobs, limiting employment growth and long-term economic expansion.

The results have implications for optimal tax policy and net sum gains for the country. We do not find significant effects of state policy changes on the relocation rate of startups across state borders. Consistent with this finding, effects are also not different between counties located at state borders and counties further away from state borders, suggesting that the estimated effects are not driven by reallocation of startup activity between states. This suggests that reducing tax rates or implementing tax incentives affects total startup activity rather than merely reallocating activity from one state to another, implying broader welfare effects through overall entrepreneurial activity. At the same time, the results suggest limited effective tax competition between states concerning startups: shifts in equilibria in terms of movement of startups and their jobs from one location to another do not appear to be a major concern. Some of the estimated tax policy effects are large and suggest that tax cuts might bring welfare gains through increases in startup activity and outcomes, although broader impacts of potential losses to government revenues and social welfare are extremely difficult to quantify and measure. Startups are found to fuel economic growth creating jobs for owners and employees (e.g., Audretsch et al., 2006; Baumol and Strom, 2007; Kerr and Robert-Nicoud, 2020). Therefore, an important avenue for further research is to incorporate tax effects on startup activity and outcomes, as quantified in this paper, in models of optimal taxation.

Overall, our study underscores that local government tax policies play a fundamental role in shaping entrepreneurship, and their effects vary based on the type of policy and the nature of the startup. Tax increases tend to suppress startup activity, while tax reductions and targeted incentives stimulate it. Business incentives, in particular, can provide support for employer startups, while taxes affect both firm formation and survival rates. Entrepreneurs are responsive to tax and incentive environments, adjusting their entry, hiring and closing decisions accordingly. While more research into the specific mechanisms is needed, our findings provide insights for local policymakers aiming to foster entrepreneurship through targeted mixes of taxes and incentive programs.

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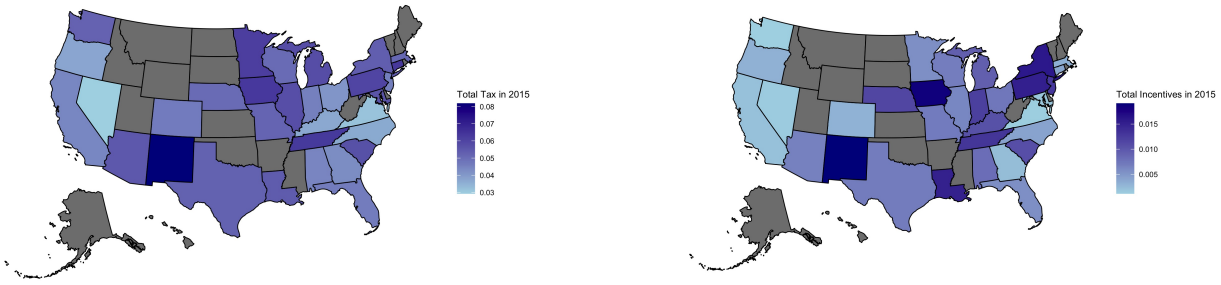
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Appendices

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.

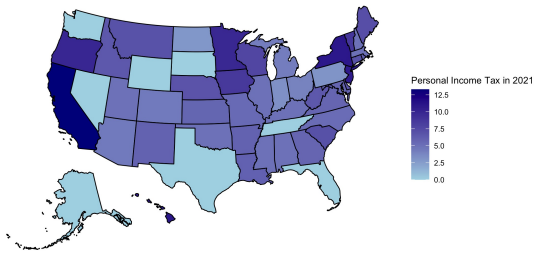
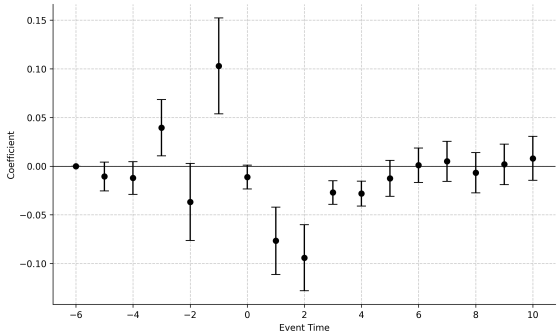


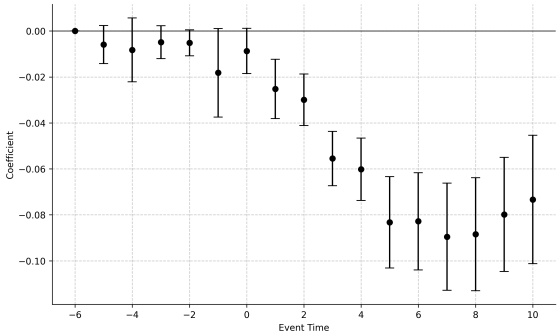
Figure A2: State-level Comparisons: Personal Income Tax Rates in 2021

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

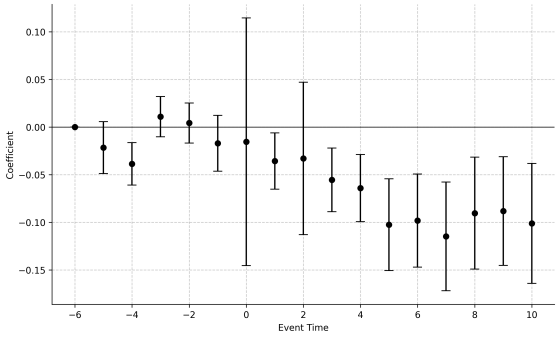
Data source: Tax Foundation.



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

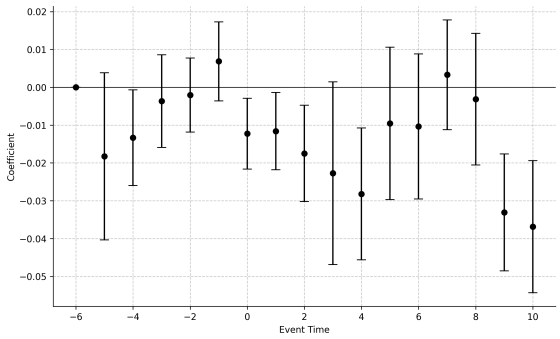


(b) Effect of total tax increases on ln(Employers)

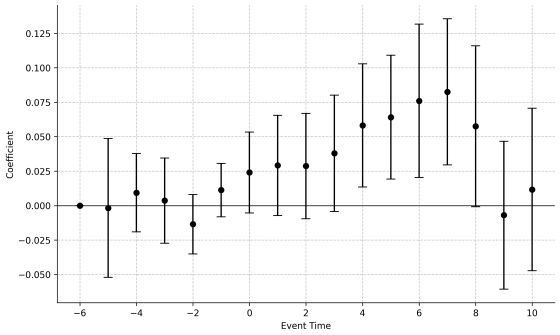


(c) Effect of total tax increases on ln(Employment)

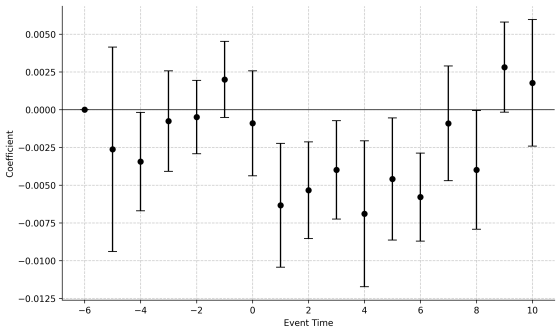
Figure A3: Event-study estimates of the effects of total tax increases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



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

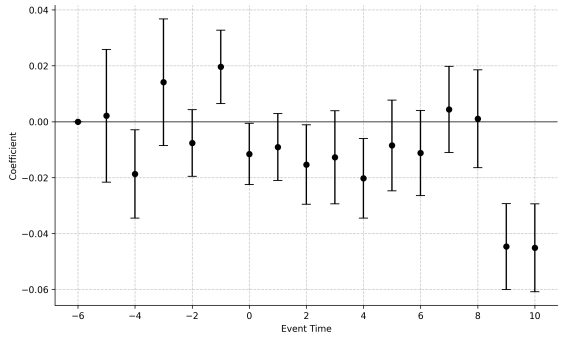


(b) Effect of total tax decreases on ln(Employment)

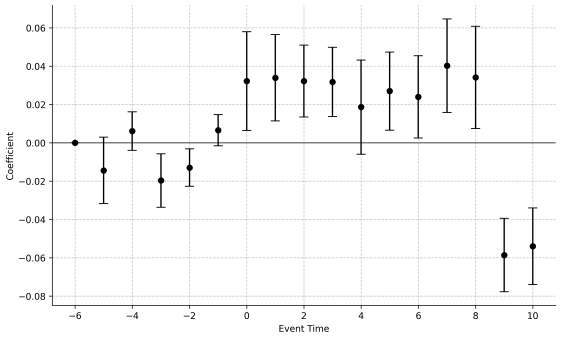


(c) Effect of total tax decreases on exit rate

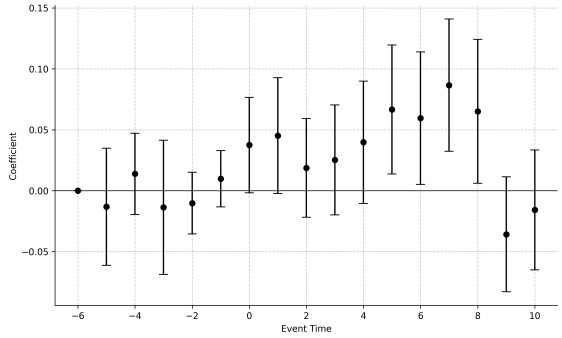
Figure A4: Event-study estimates of the effects of total tax decreases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



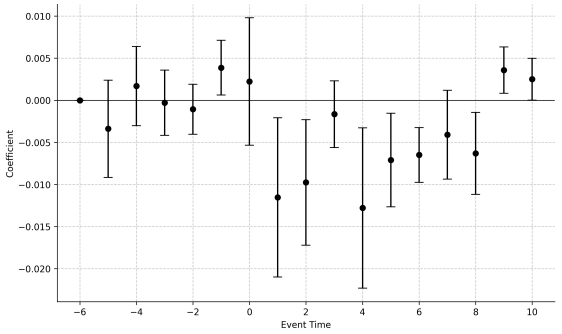
(a) Effect of property tax decreases on ln(Nonemployers)



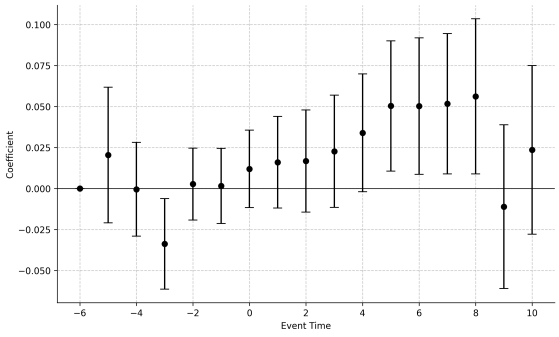
(b) Effect of property tax decreases on ln(Employers)



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

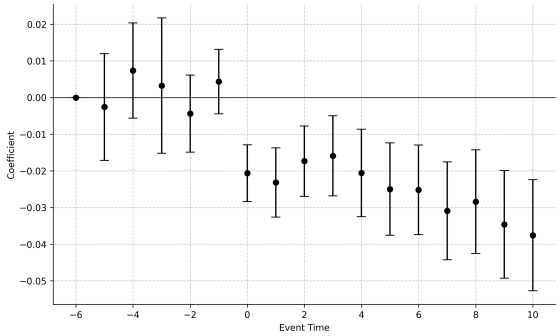


(d) Effect of property tax decreases on exit rate

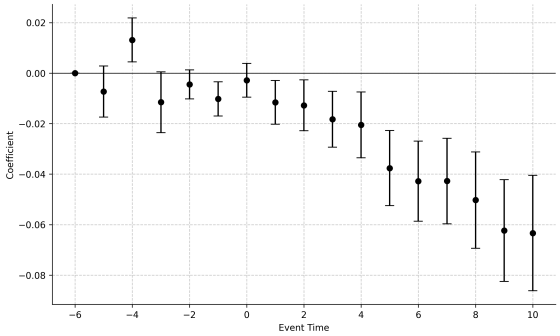


(e) Effect of property tax decrease on ln(Employers) in the manufacturing industry

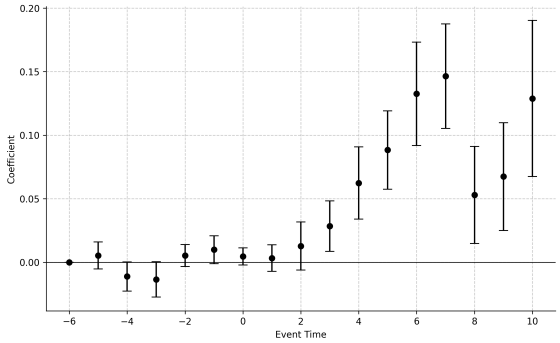
Figure A5: Event-study estimates of the effects of property tax decreases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



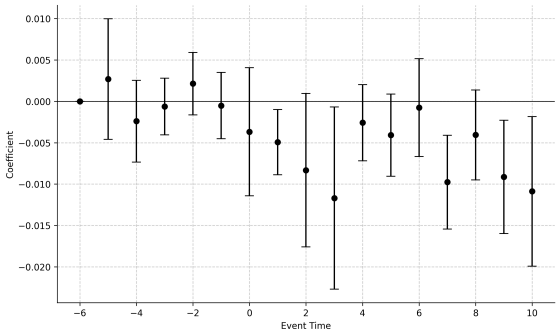
(a) Effect of CIT increases on ln(Nonemployers)



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



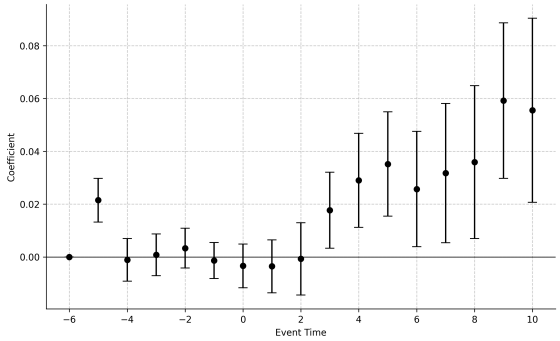
(c) Effect of CIT decreases on ln(Employers)



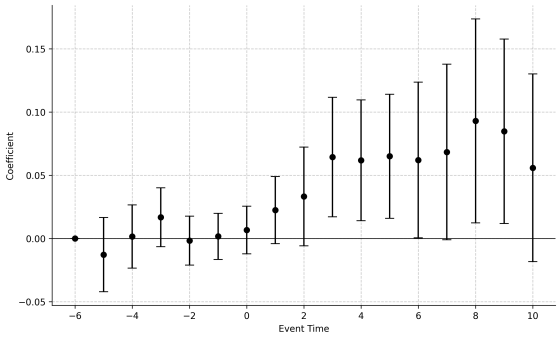
(d) Effect of CIT decreases on exit rate

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).



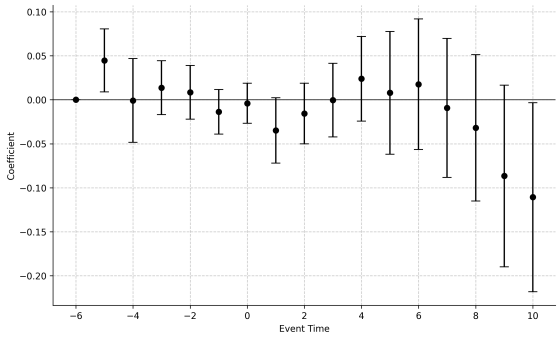
(a) Effect of PIT decreases on ln(Employers)



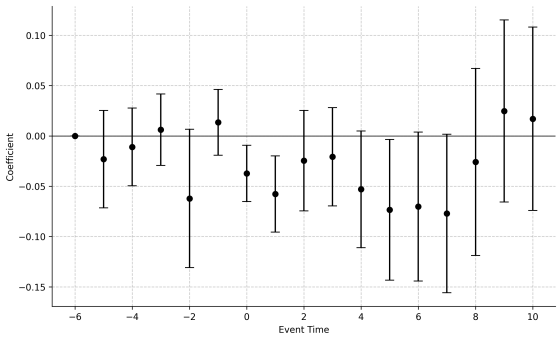
(b) Effect of PIT decreases on ln(Employment)

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).

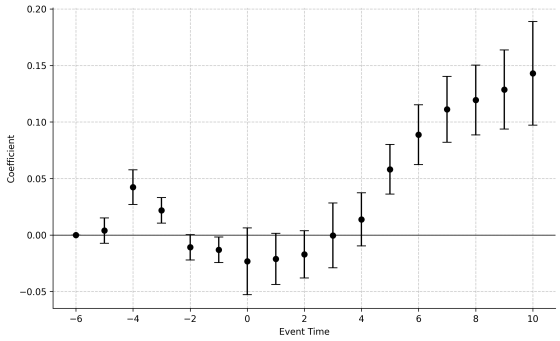


(a) Effect of sales tax increases on ln(Employment)

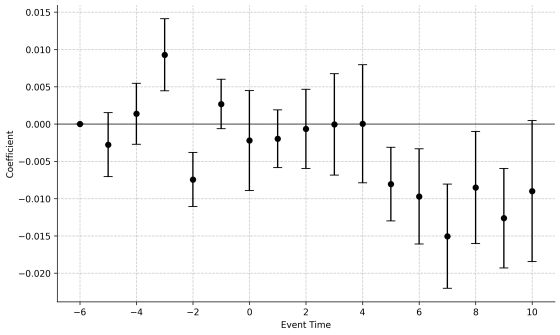


(b) Effect of sales tax increase on ln(Employment) in the retail industry

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).

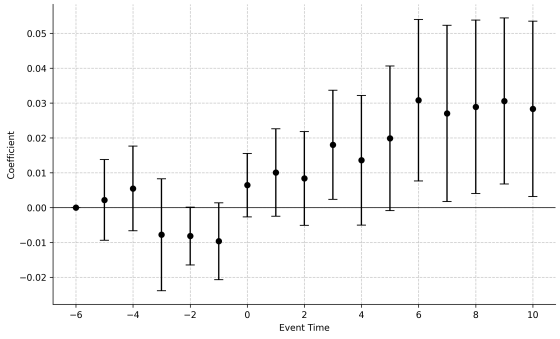


(a) Effect of total incentive increases on ln(Employers)

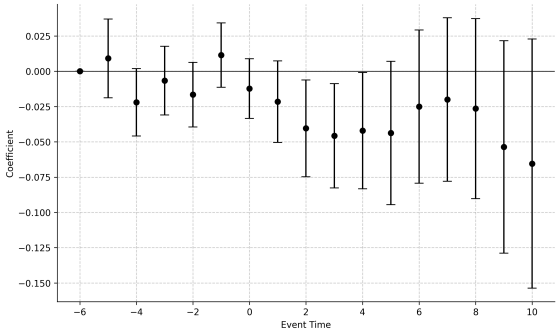


(b) Effect of total incentive increases on exit rate

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).

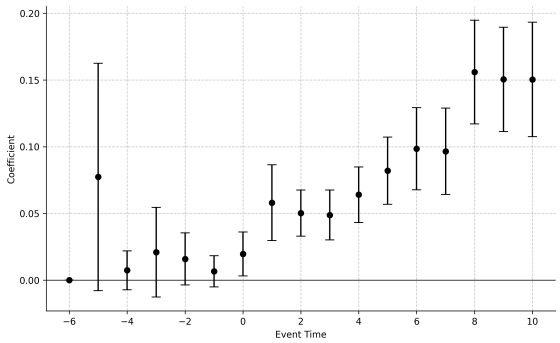


(a) Effect of investment tax credit increases on ln(Employers)



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

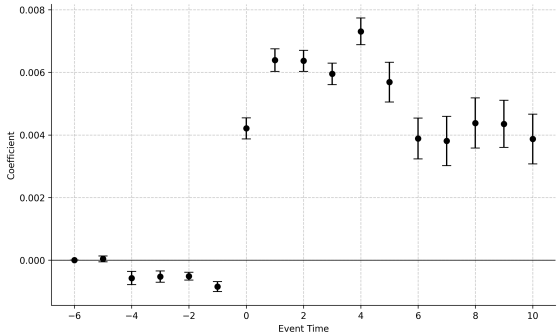
Figure A10: Event-study estimates of the effects of investment tax credit increases and decreases on startup outcomes
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



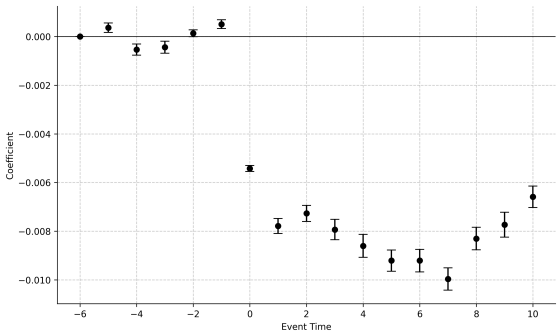
(a) Effect of research and development credit introduction on ln(Employers)

Figure A11: Event-study estimates of the effects of research and development credit introduction on startup outcomes

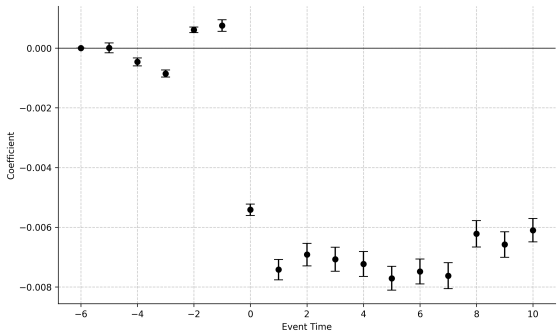
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



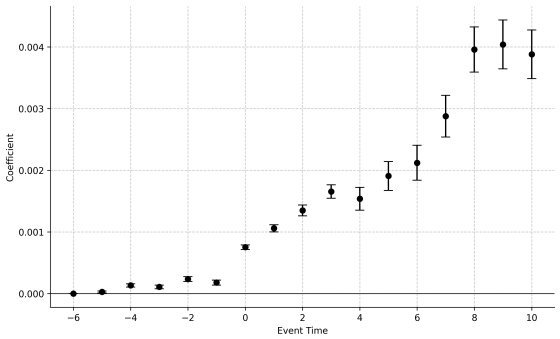
(a) Effect of total tax increases on total tax level



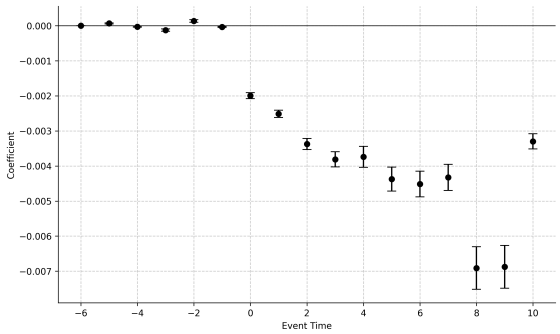
(b) Effect of total tax decreases on total tax level



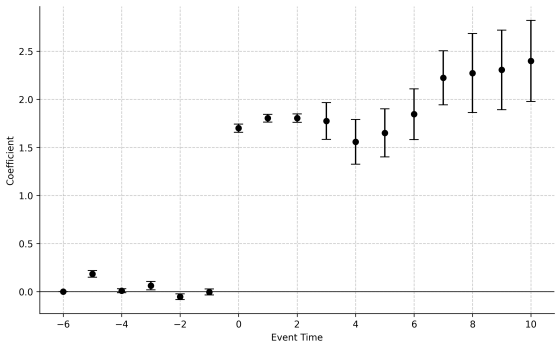
(c) Effect of property tax decreases on property tax level



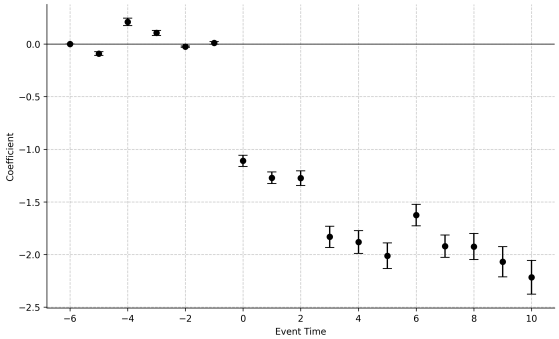
(d) Effect of CIT increases on CIT level



(e) Effect of CIT decreases on CIT level

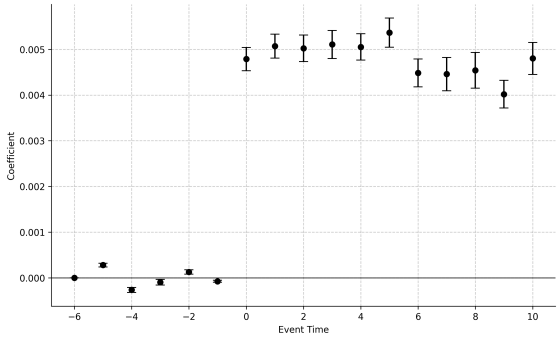


(f) Effect of PIT increases on PIT level

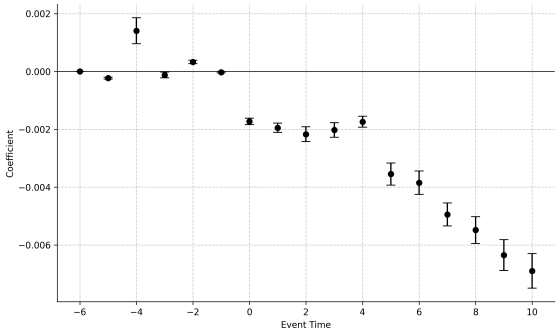


(g) Effect of PIT decreases on PIT level

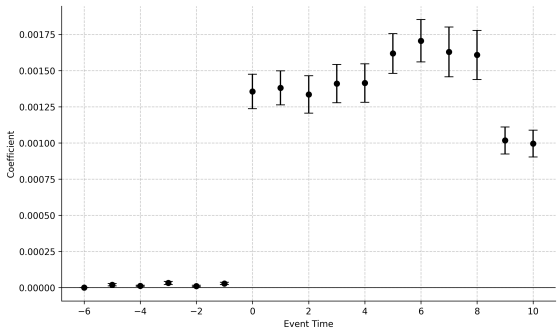
Figure A12: Event-study estimates of the effects of tax increases and decreases on tax level
Data source: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



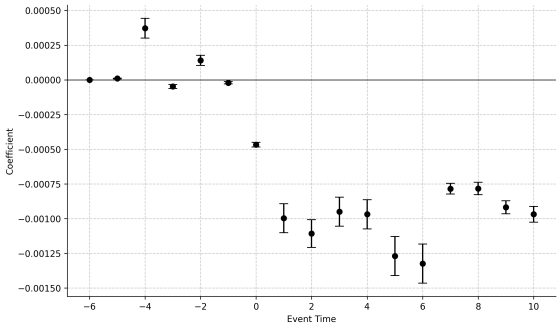
(a) Effect of total incentive increases on total incentive level



(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).

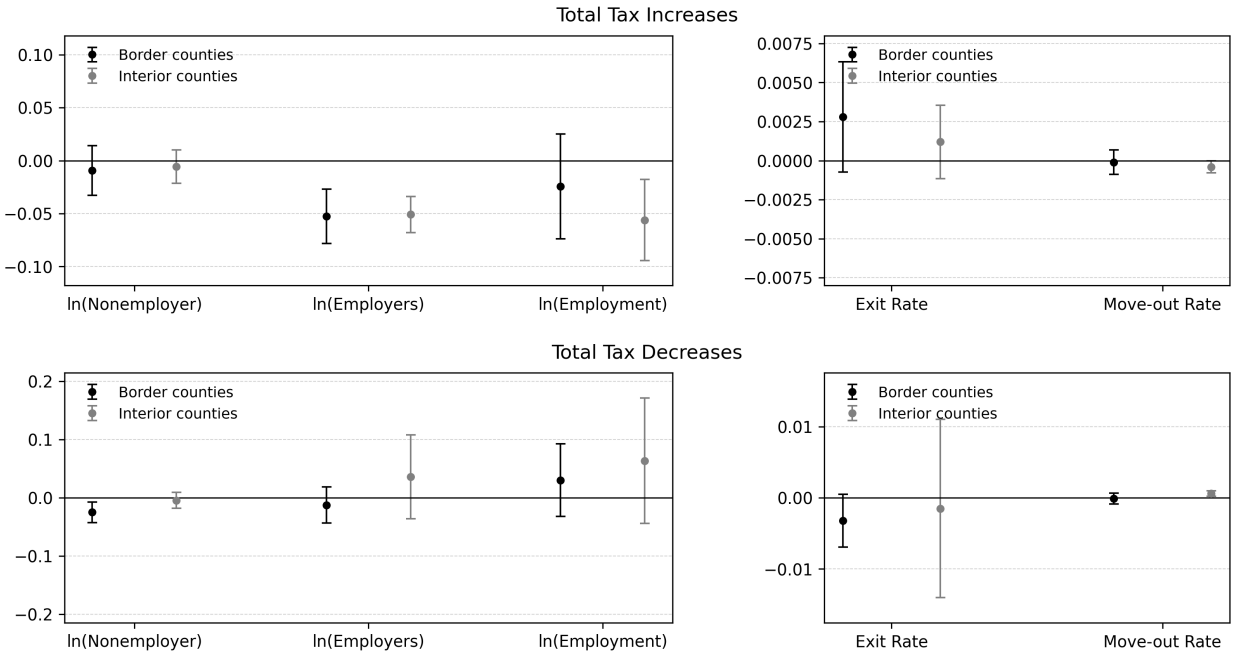


Figure A14: Effects of Large Total Tax Changes on Startup Outcomes (Border vs. Interior Counties)
Notes: This figure reports the effects of large changes in total taxes on startup outcomes at the county level, comparing counties that border other states with interior counties. Estimates are obtained 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).

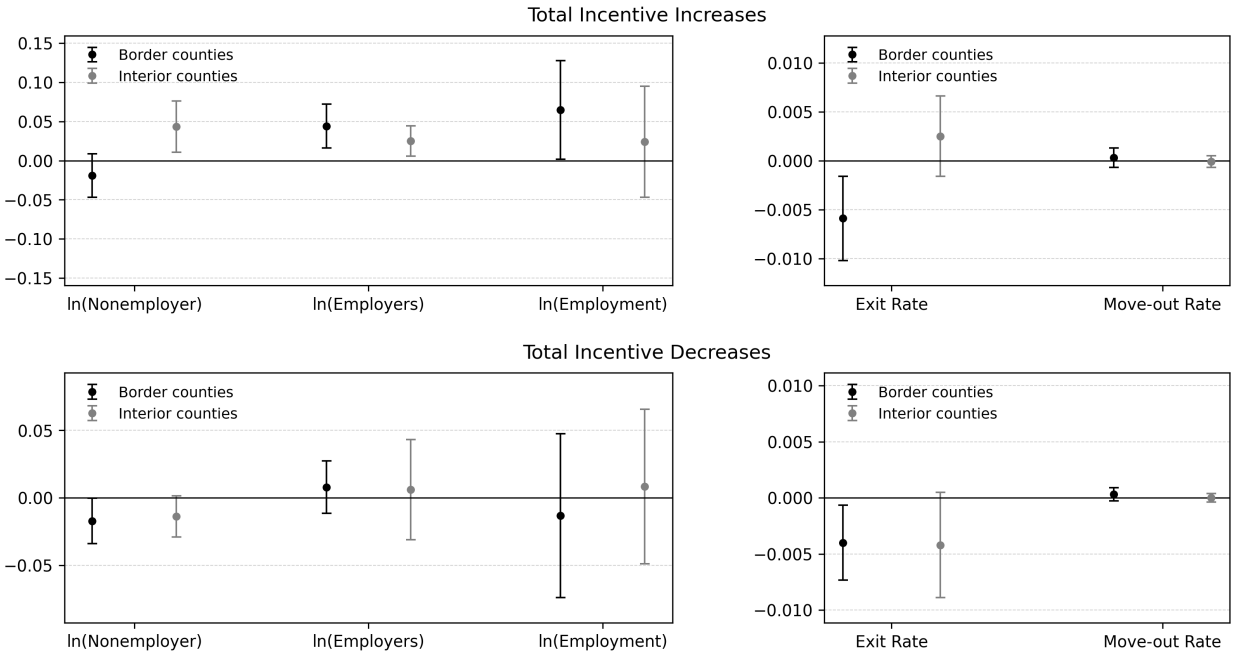


Figure A15: Effects of Large Total Incentive Changes on Startup Outcomes (Border vs. Interior Counties)
Notes: This figure reports the effects of large changes in total incentives on startup outcomes at the county level, comparing counties that border other states with interior counties. Estimates are 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).