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# The Effects of Government Policies on Entrepreneurship: Evidence from the Universe of U.S. Startups

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**Abstract**

This study provides the first analysis of the effects of a comprehensive set of government policies—including taxes, incentives, and regulations—on startup activity: their formation, employment, survival, and relocation. We use a new administrative dataset that covers the universe of startups in the United States from 2001 to 2015, the Comprehensive Startup Panel. Employing difference-in-difference, event study, and county-border regressions, we estimate the causal effects of various policy changes on startup formation, employment growth and additional outcomes. The key findings indicate that higher state taxes and minimum wages significantly discourage entrepreneurship, particularly reducing the number of employer startups and limiting their employment growth. Specifically, corporate income taxes have large negative effects on startup activity, and the effects of cuts and hikes are symmetric. Personal income taxes adversely affect nonemployer businesses. In contrast to these negative effects, targeted incentives, such as investment tax credits, have stimulating effects. We also find that expanding homestead bankruptcy exemptions does not spur startups but increases business closures. Finally, an analysis of counties along state borders suggests that state taxes and incentives lead to cross-state relocation of startups. The findings contribute to understanding the relatively unknown effectiveness of the numerous government interventions that exist to promote entrepreneurship.

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# 1 Introduction

Entrepreneurs generate a cascade of positive externalities. In addition to raising welfare for consumers and revenue for public goods, entrepreneurs drive employment growth and wage competition in the market for workers (Haltiwanger et al., 2013; Decker et al., 2016). Indeed, the number of entrepreneurs appears to be the limiting factor in job creation, due to the span-of-control problem: firms can effectively manage only so many workers (Beaudry et al., 2018). To foster entrepreneurial externalities, governments spend billions annually to encourage startups, from sweeping tax cuts to targeted financial incentives.[1]

State and local governments employ a myriad of policies to attract and grow startups, including cutting or adjusting tax rates (property, sales, corporate income (CIT) and personal income taxes (PIT)), and offering incentives such as job creation tax credits, investment tax credits, R&D credits, and property tax abatements. Policymakers also modify labor policies such as the minimum wage and the duration of unemployment insurance (UI) benefits as well as other regulations such as homestead exemptions in bankruptcy law. These varied approaches raise an important question: How do regional policies—including taxes, incentives, and regulatory policies—affect the decisions of potential entrepreneurs to start and scale new businesses?

This paper provides the first analysis of the effects of a comprehensive set of tax cuts, startup incentives, and regulations on startup creation, their employment dynamics, and their survival over time. Do any of these policies have an economically meaningful impact, and if so, which ones? To examine this question, we use a new administrative dataset, the Comprehensive Startup Panel (CSP), which covers the universe of startups during their first eight years of existence in the United States (Fairlie et al., 2023). We focus on data from 2001 to 2015. The data describe both nonemployer and employer startups, their transition from founding to first hire, their employment growth over time, and their survival. We link these records to detailed information on a comprehensive set of state and local policies that potentially affect new firms. The combination of detailed policy information and the universe of U.S. startups provides novel evidence on the effectiveness (or ineffectiveness) of specific business policies in fostering entrepreneurship across critical stages of development.

In our analysis we first employ a difference-in-differences (DID) design that exploits the timing of policy changes to estimate the causal effects of the policies while accounting for heterogeneous effects across counties and over time. By leveraging data on the full population U.S. startups, our approach compares the startup entry and employment of localities affected by each policy change (treatment group) to those not or not yet affected by a similar policy change (control group). The estimation strategy mitigates contamination

[1] For example, Texas, known for its pro-business environment, attempts to attract major corporations by offering zero state income tax and substantial tax abatements. Similarly, Florida emphasizes lower corporate taxes and less regulation to attract businesses.

from dynamic treatment effects, which can bias DID estimates when treatment timing varies (Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021). 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. Additionally, we use a county-border approach, comparing startup activity between border counties and interior counties to assess whether startups relocate across state lines in response to policy differentials. We aggregate microdata to the county level for some analyses and use firm-level data for others. Finally, to account for time-varying concurrent policy changes, we employ the stacked DID approach proposed by Cengiz et al. (2019) as a robustness check.

Our findings indicate that government policies significantly influence startup outcomes, and that the effects vary by policy type and startup nature. First, higher state taxes — particularly corporate income taxes (CIT), personal income taxes (PIT), and property taxes — significantly suppress entrepreneurial activity, reducing both the formation of employer startups and their employment growth, with symmetric effects for tax cuts and hikes. Second, targeted business incentives, such as investment tax credits and R&D subsidies, stimulate entrepreneurship, fostering job creation and improving startup survival. Third, policy impacts diverge sharply by business type: nonemployer startups are sensitive to PIT increases and minimum wage adjustments, which reduce their net earnings and raise the opportunity cost of self-employment, respectively. In contrast, employer startups, which are more likely to be incorporated and use more capital and labor, respond more strongly to the CIT, property taxes, and unemployment insurance benefits duration. Fourth, expanding homestead bankruptcy exemptions fails to spur new ventures but raises closure rates, likely by reducing entrepreneur's financial accountability. Finally, startups exhibit geographic mobility, with border counties experiencing stronger responses to tax and incentive changes, indicating some cross-border mobility or competition.

Our research contributes to the limited but growing literature on the effects of place-based policies (e.g., Audretsch, 2015; Cengiz et al., 2019), including tax breaks and incentive credits (e.g., Gentry and Hubbard, 2000; Bruce et al., 2020; Ferede, 2021), on entrepreneurship and job creation. Existing research offers fragmented insights. Taxes exhibit differential effects: CIT and property taxes raise fixed costs, deterring employer startups (Bartik, 1985; Cullen and Gordon, 2007), while PIT reduces net earnings for nonemployers, discouraging solo entrepreneurship (Gentry and Hubbard, 2000). Incentives like R&D credits spur innovation but often favor established firms over startups (Fazio et al., 2020), with a small number of firms receiving disproportionate R&D awards (Lanahan and Feldman, 2018). Job creation subsidies yield inconsistent employment gains (Faulk, 2002; Partridge et al., 2020). Regulatory policies further complicate outcomes: minimum wage hikes raise labor costs for employers (Luca and Luca, 2019), UI extensions encourage gig-economy nonemployers (Hombert et al., 2020), and bankruptcy exemptions trade risk-taking incentives for

credit constraints (Fan and White, 2003; Cerqueiro and Penas, 2017).

In contrast to previous studies, we take a comprehensive approach and systematically estimate the effectiveness of all of the major policies over the startup lifecycle. Previous studies also seldom distinguish between employer and nonemployer responses — a critical shortcoming given that 89% of U.S. startups lack employees (Fairlie et al., 2023). By examining a broad set of policy interventions, including tax changes, various incentives, and labor policies, our research provides a more complete and nuanced picture of how public policies shape or do not shape entrepreneurial dynamics. The findings have important implications for policymakers seeking to foster entrepreneurship, innovation, and economic growth. By providing rigorous evidence on the causal impact of these policies, we aim to inform the design and implementation of more effective strategies to support entrepreneurship. Importantly, our study highlights heterogeneity in policy effects, showing that different types of startups—employer and nonemployer businesses—respond differently to tax and incentive policies, which underscores the need for tailored policy approaches.

Moreover, this research contributes to the limited literature on nonemployer businesses and employment growth among early-stage startups. While policymakers often look to entrepreneurs as engines of job creation, and entrepreneurship training programs promote local job growth as a benefit, the reality is that most nonemployer businesses never transition to becoming employers (Fairlie and Miranda, 2017). Research on entrepreneurship dynamics in the U.S. has so far frequently focused on employer businesses (e.g., Haltiwanger et al., 2013), leaving nonemployer businesses understudied. By addressing this gap, we provide new evidence on how public policies influence both nonemployer and employer startups, offering insights into how these policies affect entrepreneurial entry, growth, and survival across different business types.

## 2 Literature Review

The belief among many state policymakers is that business policies significantly shape business decisions, thereby influencing employment and economic growth. Therefore, policymakers have adopted a range of targeted policies, including tax cuts, incentives, and other localized strategies, to encourage startup creation and job growth (Bartik, 1985). While the existing literature has concentrated on the effects of single policies on business establishment formation or location, this study examines the effect of multiple policies on the entry dynamics of both nonemployer and employer businesses, as well as employment. There are three major types of policies impacting business startups and employment: (i) taxes, including the total tax burden, sales taxes, property taxes, CIT, and PIT; (ii) incentives, such as investment tax credits, job creation tax credits, R&D credits, and property tax abatements; and (iii) regulatory policies, such as minimum wage and bankruptcy laws.

## 2.1 Taxes

While it has been argued that taxes impede both the establishment and expansion of businesses (e.g., Gentry and Hubbard, 2000), empirical results have been more nuanced (e.g., Cullen and Gordon, 2007; Fossen et al., 2020). To clarify the mechanisms through which various tax structures influence startup decisions, this study distinguishes between the effects of personal income, corporate income, property and sales taxes on different types of startups. 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.

In the U.S., small businesses, including sole proprietorships, partnerships, S-corporations, and limited liability companies (LLC), are typically taxed under the PIT system due to their "pass-through" nature, where business income is reported on the owner's personal tax return. Higher PIT rates often discourage entrepreneurship, as they reduce after-tax profits (Bruce, 2000; Chen et al., 2023). For example, Gentry and Hubbard (2000) find that progressive PIT discourage self-employment (see also Wen and Gordon, 2014; Arulampalam and Papini, 2023). Similarly, Ferede (2021) demonstrates that higher PIT rates have a negative effect on small business entry rates in Canadian provinces. While much of the literature suggests a negative relationship between PIT and entrepreneurship, some scholars argue that PIT could, under certain conditions, encourage business formation. Cullen and Gordon (2007) demonstrate that higher PIT shift some financial risk from entrepreneurs to the government if a business fails, thus providing a form of insurance and encouraging new business ventures.

For businesses structured as C-corporations, CIT affects startup decisions. Extensive research consistently shows that higher corporate tax rates have a negative impact on entrepreneurial entry. Cullen and Gordon (2007) suggest that reducing corporate tax rates could stimulate entrepreneurial activity. High corporate tax rates create financial disincentives by increasing the cost burden on new businesses and reducing the retained earnings available for reinvestment and growth. Studies such as Block (2021), Da Rin et al. (2011), and Henrekson and Sanandaji (2011) highlight that elevated corporate tax rates discourage business expansion and reinvestment, thereby curbing overall entrepreneurial activity. Similarly, Belitski et al. (2016) find that higher CIT reduce entrepreneurial entry in a panel study across 72 countries.

Sales tax is levied on goods and services sold. The imposition of sales taxes can affect business costs and consumer pricing strategies, potentially reducing customer demand and affecting overall business viability (Chen et al., 2023). This tax type can thus influence both the operational costs and market strategies of startups, as they must adjust prices to account for the tax burden while maintaining competitiveness in the market.

Property tax is 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, discouraging new businesses from entering the market and constraining the growth of existing firms. These effects are particularly pronounced for employer startups, which often depend on commercial or industrial properties. The additional tax burden increases fixed costs, leaving fewer resources for investment and hiring. However, the impact of property taxes may vary depending on the type of business. Nonemployer startups or businesses operating from home are likely to be less directly affected by property tax increases, as they typically do not face the same property-related expenses as employer firms.

Together, these four types of taxes–personal income, corporate income, property, and sales–collectively shape the economic environment for startups, influencing decisions on whether to initiate or expand business operations.

## 2.2 Tax Incentives

Business taxes and incentives interact complexly, affecting startup establishments and employment. While incentives such as tax credits and grants are designed to reduce the tax burden, their impact is not uniformly positive across all types of firms. These incentives allow the government more discretion in supporting selected projects, typically benefiting large firms such as Amazon, Tesla, and Foxconn.[2] However, such large-scale incentives may crowd out smaller startups and nonemployer businesses by increasing competition for critical resources like land, labor, and capital, thereby reducing opportunities for new firm entries.

Empirical studies on the effectiveness of business incentives have yielded mixed results on employment and business establishments, with effects varying by region and industry. For example, Wang et al. (2020) found that substantial increases in incentives resulted in small positive effects on overall employment.[3] 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. Similarly, Neumark and Kolko (2010) found no significant evidence of employment growth from incentives within California's enterprise zones. Furthermore, Gabe and Kraybill (2002) observed 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, suggesting that large incentive packages may hinder entrepreneurial growth rather than promote it.

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

[3] Wang et al. (2020) define the regime change as the 95th percentile of the distribution of incentive increases, with 41 regime changes in 27 states.

Different types of incentives further complicate the landscape. Job creation tax credits and job training subsidies help businesses hire and upskill their workforce, driving employer expansion (Faulk, 2002; Chirinko and Wilson, 2023). R&D tax credits promote innovation by making research activities more affordable, which can increase both the quality and quantity of entrepreneurship, particularly in high-tech sectors (Cappelen et al., 2012; Fazio et al., 2019, 2020). R&D tax credits also increase the demand for R&D workers, leading to higher employment opportunities in research-intensive sectors (Chen and Hshieh, 2024). However, government R&D funding is highly concentrated, with a few firms receiving disproportionate support. While some criticize this allocation, targeted programs like the Small Business Innovation Research (SBIR) program have been reported to effectively foster innovation and talent development, especially for small and young firms (Lanahan and Feldman, 2018; Feldman et al., 2022). Property tax abatement reduce long-term operating costs and can lead to 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). In conclusion, while business incentives are intended to promote firm growth and job creation, evidence on their effectiveness, especially for startups, is mixed.

## 2.3 Regulatory Policies

Minimum wage policies are primarily aimed at improving the income of low-wage workers, but their broader economic effects extend beyond wages, influencing employment as well as employer and nonemployer business startups. Minimum wages act as a form of wage rigidity, increasing hiring costs for employer businesses and raising opportunity costs for nonemployer firms. The rise in labor costs may discourage the formation of employer startups, as higher wages make it more expensive for entrepreneurs to hire employees (Parker, 2018; Patel, 2019; Égert and Mourougane, 2024). For instance, Luca and Luca (2019) found that higher minimum wages led to a reduction in the number of low-rated restaurants, illustrating how labor cost increases can strain small businesses in low-margin sectors. The effects of minimum wages on nonemployer businesses are more mixed and often vary by industry. Some studies suggest that higher minimum wages increase the probability of establishing nonemployer businesses overall, especially in the transportation sector, driven by the gig economy (Glasner, 2023). Furthermore, minimum wages are often found to have null or negative effects on employment (Holtemöller and Pohle, 2020; Jardim et al., 2022; Neumark and Shirley, 2022; Harasztosi and Lindner, 2019), which may be explained by job cuts in response to rising wage levels. This effect is particularly pronounced in regions that rely heavily on low-wage workers (Ganserer et al.,

2022).

Homestead exemptions are important in determining the amount bankruptcy filers must pay to their unsecured creditors, as well as shaping access to bankruptcy protections. These exemptions, which protect home equity from liquidation during Chapter 7 bankruptcy, are the primary source of variation in exemption levels across states and are also subject to significant changes within states.[4] The potential effects of homestead exemptions on entrepreneurial activity are mixed. On the positive side, exemption laws can encourage entrepreneurial activity by reducing personal financial risks. For example, exemptions have been found to attract new businesses and support the survival of existing ones by enabling entrepreneurs to take on risks without jeopardizing their homes (Fan and White, 2003; Fossen, 2014; Rohlin and Ross, 2016). However, other studies identify potential downsides. Increased exemptions can reduce credit availability, particularly for mid-wealth entrepreneurs, leading to lower operational efficiency, reduced employment, and higher failure rates (Cerqueiro and Penas, 2017). Exemption increases have also been associated with higher bankruptcy filing rates, particularly among wealthier households, suggesting strategic use of exemptions (Pattison and Hynes, 2020). Furthermore, Cole et al. (2024) document that higher exemptions contract small business credit markets, leading to fewer small establishments and reduced employment.

UI benefits duration policies, designed to provide income security during joblessness, may have dual effects on entrepreneurial activity, balancing liquidity support for experimentation against unintended distortions in venture quality. Studies demonstrate that extending UI benefits can lower entry barriers for unemployed individuals, increasing self-employment rates and fostering firm creation without compromising short-term survival (Hombert et al., 2020). However, such policies often prioritize low-risk, marginal ventures — particularly nonemployer startups — that crowd out employment in existing firms and generate minimal net job gains (Hombert et al., 2020). Conversely, overly generous UI benefits suppress opportunity-driven entrepreneurship, delaying transitions to scalable ventures and disproportionately deterring high-potential entrants who might otherwise innovate or hire employees (Bilan and Apostoaie, 2023). Meanwhile, stricter UI benefits durations sharpen incentives for business creation but reduce liquidity buffers, disproportionately harming survival-oriented startups (Bilan and Apostoaie, 2023).

[4]Exemption amounts vary across states and may be higher in certain states if filing as a couple. For example, Alabama, Connecticut, Georgia, Illinois, Indiana, Kentucky, Maine, Montana, Nebraska, Nevada, New Hampshire, New Mexico, New York, North Carolina, Ohio, South Carolina, Utah, Virginia, West Virginia, Wisconsin, and Wyoming double the homestead bankruptcy protection for couples filing jointly, allowing the parties to exempt double the worth of equity in their primary residence. Some states, like Arkansas, Florida, Iowa, Kansas, Oklahoma, South Dakota, and Texas, provide unlimited homestead bankruptcy exemptions. In this study, we use homestead bankruptcy exemptions for single filers.

# 3 Data and Measures

We draw entrepreneurial outcomes from the firm-level CSP, while the key explanatory variables are sourced from the Panel Database on Incentives and Taxes (PDIT), the Tax Foundation, and the U.S. Department of Labor, along with data from Dube and Lindner (2021), Dieterle et al. (2020), and Pattison and Hynes (2020). 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 examine 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, property tax abatement, minimum wages,UI benefits duration, and bankruptcy exemption. The goal is to examine a fairly comprehensive set of policies commonly used by states and counties to attract startups and create jobs in local areas.

## 3.1 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 2021. 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 reports.[5]

The novelty of the CSP is that it provides information to track nonemployer startups and their pre-employment years. An examination of early-stage job creation among startups requires identifying nonemployer states and their transitions to employer status.[6] Since the data links the employer and nonemployer universes, it provides an accurate identification of the startup point for business units, allowing each annual cohort of startups to be followed longitudinally.[7] Furthermore, the database includes detailed information on businesses, such as legal form, industry, geographic location, establishment year, employment dynamics,

[5]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.

[6]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 establishments.

[7]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.

payroll distributions, and exit status.

Our analysis primarily focuses on six startup outcomes sourced from the CSP: the count of new nonemployer startups, the count of employer startups, total employment by startups, the annual exit rate of startups, as well as the rate at which startups relocated into a county from other states (move-in rate) and 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. At the firm level, we analyze five startup outcomes: whether a firm transitioned from a nonemployer to an employer startup, changes in employment, and whether the startup exited, moved in from a different state, or moved out to a different state.

### 3.2 Measurement of Business Taxation, Incentives, and Regulatory Policies

Taxes and incentives data are mostly sourced from the PDIT, a new dataset provided by the W.E. Upjohn Institute for Employment Research (Bartik, 2017). The PDIT provides state-level information on a broad range of incentives and taxes for an average firm in 47 cities across 33 states (as shown in Figure A2) and 45 industries, covering the period from 1990 to 2015. These 33 states account for over 90% of U.S. output. 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 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.

The PDIT simulates taxes and incentives by assuming the establishment of a new business in a specific city, state, 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.[8] By combining the firm's balance sheet data with state and local 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 industries and states.[9] We use total taxes, total incen-

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

[9]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

tives, minimum wage, unemployment insurance (UI) benefits duration, homestead bankruptcy exemptions, property tax, sales tax, CIT, PIT, job creation tax credits, R&D tax credits, property tax abatements, and investment tax credits as key policy variables.

In addition to the simulated taxes and incentives from the PDIT, we include nominal marginal state PIT rates for the highest tax brackets, sourced from the Tax Foundation (Tax Foundation, 2021). The Tax Foundation provides comprehensive data on state-level tax policies. The tax rates as direct measures of policy instruments complement the simulated business tax burden information from the PDIT. For PIT, local income taxes—imposed by approximately ten states at the county or city level—are excluded to ensure consistency across states.

We also construct county-level minimum wage policies by combining state-level minimum wages for all 50 states, sourced from the U.S. Department of Labor, with city-level minimum wages from Dube and Lindner (2021). We use the minimum wage of the largest city in a county as an approximate measure for the county's minimum wage.[10] The minimum wage is coded as the nominal county-level minimum wage effective on January 1 of each year. Additionally, we obtain data on state-level UI benefits duration from Dieterle et al. (2020) and on state-level homestead bankruptcy exemptions from Pattison and Hynes (2020). For the latter, we use the exemption level applicable to married individuals, which is higher than that for singles in some states.

### 3.3 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 entrepreneurial outcomes. A set of socioeconomic controls are sourced from various databases. Population data at the county level is obtained from the Bureau of Economic Analysis (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 American Community Survey (ACS) for the years 2010 to 2021. In the ACS, detailed demographic data is not consistently available for years prior to 2010. To address 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, covering 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, specifically, the shares of high-school and college graduates,

environments but does not simulate actual business tax liabilities over time.

[10]There are two cities where the city-level minimum wage is also the county-level minimum wage. San Francisco is both a city and a county, and it established its own minimum wage in 2004 at $8.50 per hour. Similarly, Denver is both a city and a county, and it began setting its own minimum wage in 2020.

we draw on data from the Economic Research Service (ERS) from 2001 to 2009, released by the U.S. Department of Agriculture.

# 4 Methods

To estimate the causal effects of various government policies on entrepreneurial outcomes, we employ difference-in-differences (DID) and event study methodologies at the county level, and two-way fixed effects (TWFE) models at the firm level.

## 4.1 Difference-in-differences Model and Event Study

The initial county-level model employs a DID specification and an event study analysis to estimate both the average and dynamic impacts of large changes in policy measures. We employ 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.

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 four measures of startup creation and employment in county $c$ at time $t$: the log count of new nonemployer startups in their first year, the log count of employer startups, the log of total startup employment, the exit rate, as well as the move-in and move-out rates within their first eight years of existence. $Treat_{ct}$ 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 policy variable (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).[11] The coefficient of interest, $\beta_1$, captures the effect of the policy change on startup creation and employment growth. The policy variables include state-level policies such as total incentives, taxes, CIT, PIT, property tax, sales tax, job creation tax credits, investment tax credits, R&D credits, property tax abatements, minimum wages, and homestead bankruptcy exemptions.

[11] 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.

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, except when the number of new nonemployers is the dependent variable, which are all age 0 by definition. County and year fixed effects, $\gamma_c$ and $\tau_t$, capture county-specific time-invariant effects and common shocks. In a robustness check, we also control for other policy changes.

The sample is trimmed to include up to 6 pre-treatment and 10 post-treatment years in each 2x2 DID experiment. We use the doubly robust DID estimator based on stabilized inverse probability weighting and OLS outcome regression adjustment (Callaway and Sant'Anna, 2021). Standard errors are based on the multiplicative Wild Bootstrap procedure with 999 repetitions and clustered at the state and county level (two-way clustering).

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: while treatment assignment is presumed to be exogenous, the event study enables an examination of parallel trends prior to the policy implementation. This allows to assess whether observed post-treatment effects can be confidently attributed to the policy intervention rather than pre-existing trends. 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 significant policy change, with $k = 0$ representing the event year itself. This framework enables an examination of the policy's precursor and subsequent impacts. The coefficient $\beta_k$ captures the differential time-specific effects between the treatment and control groups for each year relative to the event, extending from $-5$ to 10 years, 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 base category. The fixed effects $\gamma_c$ and $\tau_t$ control for unobserved heterogeneity across counties and calendar time, respectively.

To validate 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 outcome 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 Figures A3g and A3h in Appendix A, show consistent patterns with sharp changes at the time of the treatment and stable pre-treatment trends. Similar consistency is observed in other policy dimensions, including Figures A4c, A5e, A9b, A9c, A10d, A11c, A12c, and A12d. These consistent pre-treatment trends

and significant changes following treatment underscore the validity of the DID approach in capturing the effects of large policy shifts.

### 4.2 Firm-level Analysis

We further augment our analysis with a TWFE specification at the firm level. It is estimated based on a sample including both employer and nonemployer startups within their first eight years of existence. The firm-level model is constructed as follows:

$$Y_{ict} = \beta_0 + \beta_1 \, Policy_{s,t-1} + \beta_2 \, X_{ict} + \tau_i + \gamma_t + \epsilon_{it} \tag{3}$$

The dependent variable, $Y_{ict}$, represents five startup outcomes for firm $i$ located in county $c$ at time $t$: whether the firm transitioned from a nonemployer to an employer startup, changes in employment, and whether the startup closed or moved in or out across a state border. The policy variable, $Policy_{s,t-1}$, is a continuous variable capturing the policy level. Controls, $X_{ict}$, mirror those used at the county level, with the addition of dummies representing the age of the startup (ranging from 1 to 8 years). Firm and year fixed effects, $\tau_i$ and $\gamma_t$, are included to account for unobserved heterogeneity.

## 5 Empirical Results

### 5.1 Descriptive Statistics

Covering the universe of startups in the United States, the CSP includes over 100 million startups and more than 850 million startup-year observations (Fairlie et al., 2023). On average, it records approximately 4.1 million startups annually, of which 3.7 million are nonemployer startups, representing 89% of all startups. Table 1 (Panel A) presents summary statistics for the startup outcomes, policy variables, and control variables at the county level across the 33 states included in the PDIT from 2001 to 2015. The dependent variables capture various aspects of startup activity and business dynamics. On average, total employment in startups (up to eight years old) is 8,000 workers per county, with substantial variation in labor market size across regions. Nonemployer startups, including sole proprietorships and microenterprises, average 4,700 per county, while employer startups average only 900 per county, indicating that most new businesses operate on a smaller scale. The annual transition rate from nonemployer to employer is relatively low, at 0.86% on average, underscoring the challenges small businesses face in scaling up. In contrast, the exit rate is much higher, averaging 29.85%, indicating a high degree of business turnover. Business relocation dynamics are captured by the move-out and move-in rates across a state border. Panel B of Table 1 presents statistics

for firm-specific variables. Employment per firm averages 1.37 workers, and the annual employment growth rate is 4% on average. The relocation patterns align with the county-level rates.

While these descriptive statistics provide a snapshot of the entrepreneurial landscape, the dynamic trends over time offer additional insights into startup activity. Figures 1 and 2 illustrate the year-by-year changes in startup formation, survival, and employment. Figure 1 shows the trends in startup formation and total employment of firms in their first year of business from 2010 to 2020. Total startups are primarily driven by the large number of new nonemployer businesses, while employer startups remain consistently low in comparison. Total employment within newly founded firms fluctuates between 2.6 million and 3.3 million over the 11 years analyzed (right scale). Figure 2 provides startup survival and employment dynamics from one to eight years after establishment by startup cohort. For total startups, the first-year survival rate is between 55% and 60%, but it declines to 25%–30% by the eighth year. This trend is largely driven by the lower survival rates of nonemployer startups, whose first-year survival rate is around 55%. In contrast, employer startups exhibit much higher survival rates: over 90% of employer businesses survive the first year, and more than 50% continue to operate eight years after establishment. The average employment per startup is modest, with most startups supporting only one or two employees on average.

Turning to the policy variables in Table 1, 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 coporate income tax at 1.01%, based on the PDIT data from 2001 to 2015.[12] Additionally, state-level top CIT rates, average 6.19%, while state top PIT rates average 5.23%. The overall incentives average 0.75% of value-added, with the investment tax credit being 0.16%, job creation tax credits being 0.24%, R&D credits being 0.04%, and property tax abatement being 0.26%. 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.

The minimum wage at the county level averages $6.41 between 2001 and 2015. The federal minimum wage in the U.S. was set at $5.15 in 1997 and remained unchanged until 2007. It was then raised to $6.55 in 2008 and to $7.25 in 2009, where it has remained through 2021. In 2001, 10 states and Washington, D.C., set their minimum wage higher than the federal level. By 2021, 30 states and D.C. had done so, as shown in Figure A1a in Appendix A. Additionally, some cities, like San Francisco and Denver, set higher minimum wages than their states, and we include this source of variation in our analysis. Furthermore, the average UI benefits duration is 46.17 weeks, and the average homestead bankruptcy exemption, which provides asset protection for homeowners in bankruptcy cases, is $214,900. These policies, particularly UI benefits duration

[12]The PDIT data for incentives or taxes are expressed as a proportion of value-added for an average firm in a particular state.

and bankruptcy exemptions, influence startup activity by shaping financial security and risk tolerance for potential entrepreneurs.

In addition to the dependent and policy variables, Table 1 includes several control variables that capture demographic and economic characteristics at the county level. Business characteristics are controlled for using firm age distributions. The average firm age 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, with decreasing proportions as firms age.

In summary, startup activity in the U.S. exhibits a dual nature, highlighting both its volatility and its potential contribution to job creation. While exit rates are high, with nearly 30% of startups closing annually, and the transition 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 structural 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.

## 5.2 Estimation Results

The empirical results, presented in Tables 2 and 3, provide robust evidence on the impact of large policy changes—categorized as increases or decreases in taxes, incentives, and labor regulations—on various dimensions of startup activity and business dynamics. The tables show aggregate average treatment effects on the treated (ATT) of a large increase or decrease event in a policy on four outcome variables of startups, as estimated using the DID method by Callaway and Sant'Anna (2021). The size of the policy increases and decreases, i.e., the size of the treatment analyzed, can be seen from the event study plots showing the effect of a policy change event on the policy in Appendix A.

Focusing initially on total tax increases, Panel A of Table 2 reveals that significant increases in total taxes negatively affect entrepreneurial activity. The treatment, a large increase in total taxes, corresponds to about 0.04 percentage points of total value added of a firm (see Figure A3g), which is about 8% of the average shown in Table 1. Such an increase in total taxes reduces the number of new nonemployer startups by 2.91% and employer startups by 5.46% (Table 2). These dynamics are illustrated in event study plots in Figures A3a and A3d, respectively. 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

Table 1: Descriptive Statistics

| **Panel A: County-Level Variables** | **Mean** | **Standard Deviation** |
|---|---|---|
| **Dependent Variables** | | |
| 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 |
| Move-in Rate (%) | 2.09 | 1.45 |
| **Policy Variables** | | |
| 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 Corporate Income Tax (%) | 6.19 | 3.02 |
| State Top Personal Income Tax (%) | 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 |
| Minimum Wages (City) ($) | 6.41 | 1.12 |
| Unemployment Insurance Duration (weeks) | 46.17 | 28.71 |
| Bankruptcy Exemption ($1,000s) | 214.9 | 371.40 |
| **Control Variables** | | |
| Firm Age | 2.35 | 0.46 |
| Firm Age = 0 | 29.06 | 4.70 |
| Firm Age = 1 | 16.59 | 2.98 |
| Firm Age = 2 | 13.13 | 2.62 |
| Firm Age = 3 | 11.43 | 2.58 |
| Firm Age = 4 | 9.36 | 3.10 |
| Firm Age = 5 | 7.87 | 3.36 |
| Firm Age = 6 | 6.71 | 3.50 |
| Firm 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 B: Firm-Level Variables** | **Mean** | **Standard Deviation** |
| **Dependent Variables** | | |
| 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 |
| **Control Variables** | | |
| Firm Age | 2.06 | 1.97 |

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

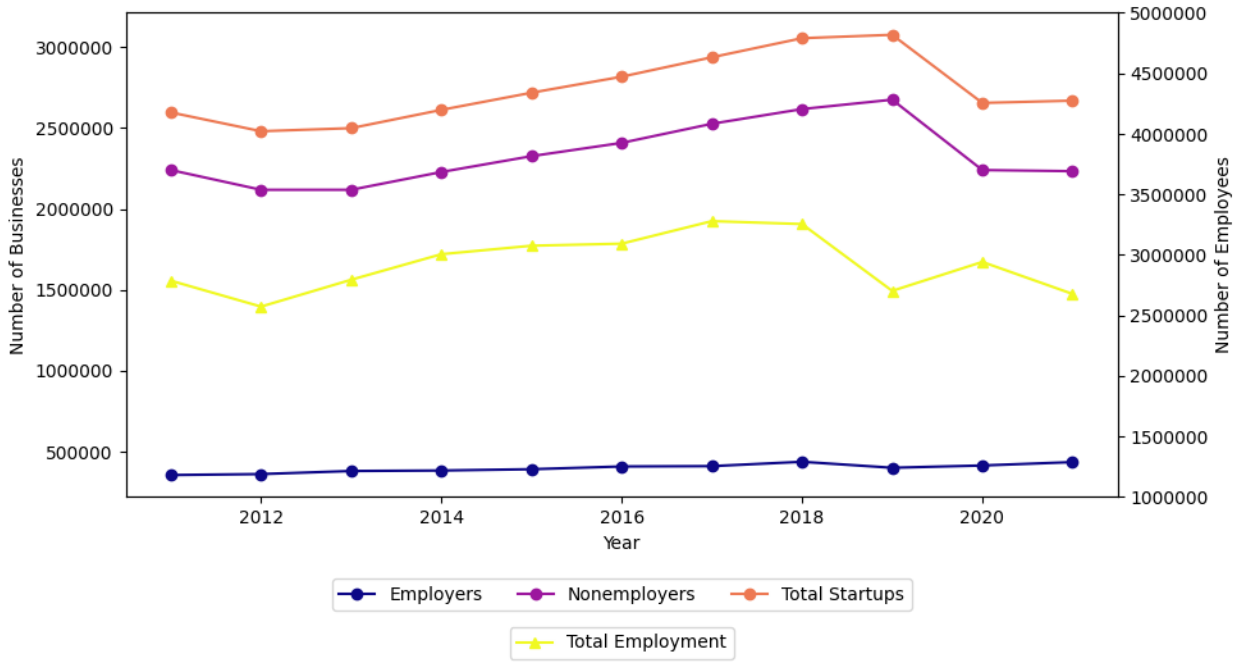



Figure 1: Trend in Startup Formation (2011-2021)

*Note:* This figure presents the number of new employers, new nonemployers, and total new startups, along with their employment in their first year of operation.

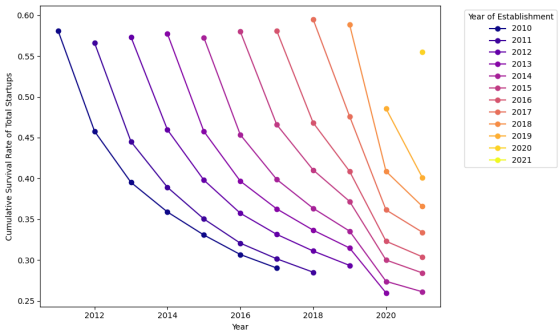
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

extra tax burdens (Figure A3b). Exit rates show a marginal and insignificant increase of 0.19%, indicating that tax increases primarily suppress the formation of new ventures rather than trigger widespread closures.

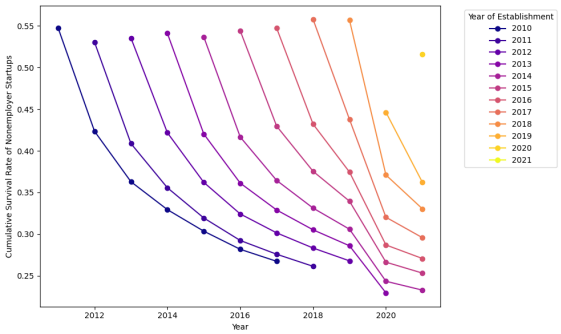
Firm-level regression results in Table A1 in Appendix A (two-way fixed effects) provide additional insights into the mechanisms behind the negative effects. An increase in the total state tax burden by 10% of a firm's value added significantly increases the probability of a startup moving out of a county into another state by 0.36 percentage points, suggesting that heightened financial pressures drive firms to relocate to lower-tax states. Conversely, transitions from nonemployer to employer businesses are positively affected by the increase in total tax burden, potentially reflecting a shift in the composition of entrepreneurial activity.

Consistent with the estimated effects of tax increases, large decreases in total taxes, reported in Panel A of Table 2, generate positive effects, particularly for new nonemployer startups and total employment. A large cut in total taxes increases total employment by 4.55% (Figure A3c). However, the effect on new nonemployer startups is both negative and significant (Figure A3e), suggesting that solo-run businesses may not benefit directly from tax reductions. This apparent contradiction can be partly attributed to the composition of total taxes, especially the role of property taxes.

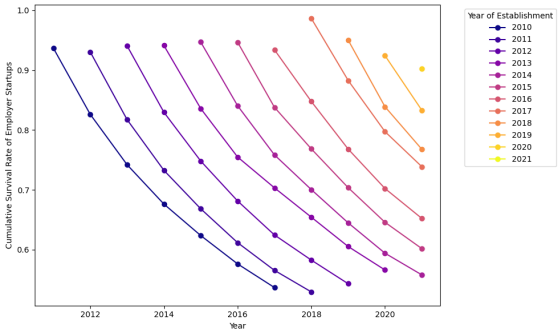
Having found that total state taxes affect startups, the next question is which specific taxes contribute



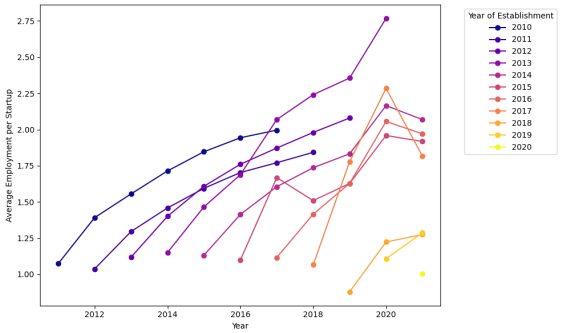

(a) Survival Rate of Total Startups by Year




(b) Survival Rate of Nonemployer Startups by Year




(c) Survival Rate of Employer Startups by Year




(d) Average Employment per Startup by Year

Figure 2: Startup Survival Rates By Startup Cohort (2010-2020)
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

to this effect. Tables 4 and 5 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 tax has only marginal impacts on entrepreneurial outcomes. Property taxes, which constitute approximately 47% of total state taxes, significantly affect startup outcomes. Panel A of Table 5 indicates that a large reduction in property taxes negatively impacts new nonemployer startups but positively affects employer startups and total employment, as depicted in Figures A10a, A10b, and A10c. These reductions lower the fixed costs associated with scaling up, encouraging the transition of nonemployer businesses into employer businesses. Consequently, some ventures that might have started as nonemployer startups start as employers instead, thereby reducing the immediate count 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, as seen in Panel A of Table 5. Additionally, exit rates decline significantly by 0.53%, reflecting the stabilizing influence of property tax cuts on existing businesses (Figure A10b).

Panel B of Table 4 shows that a large increase in CIT reduces nonemployer startups by 2.48% and

employer startups by 3.13%, as visualized in Figure A8a. Mirroring these effects, Panel B of Table 5 demonstrates that a large decrease in CIT increases nonemployer startups by 3.29%, employer startups by 4.71% (Figure A8c), and total employment in startups by 3.91%. Similarly, a decrease in top PIT rates boosts nonemployer startups by 2.1%, employer startups by 1.96%, and total employment by 4.97% (Panel C of Table 5), although Figures A11a and A11b 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. An intriguing exception arises with the top state CIT rate. Surprisingly, an increase in the top state CIT rate does not deter startups — in fact, it could increase the nonemployer startups by 2.48%, employer startups by 7.12%, and employment by 7.31% in Table 4, as visualized in Figures A8b, A8e and A8g. As only corporations pay the CIT, one interpretation could be that predominantly large, profitable firms are affected by the state CIT, so a CIT increase could put small startups at a relative advantage. The figures illustrate that the positive effects for startups emerge gradually, potentially reflecting delayed market reallocation as incumbents lose ground to new entrants. In contrast, cuts in the state CIT rate produce positive effect on startups, potentially because many startups aspire to incorporate in the future.

Turning to total incentives offered by states, Tables 2 and 3 highlight positive effects on entrepreneurial activity. Panel B of Table 2 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%. These dynamics are illustrated in Figures A4a and A4b. These results indicate that incentives are particularly effective in fostering growth among larger-scale startups, which are better positioned to leverage such benefits to expand. Consistent with this, Panel B of Table 3 shows that a decrease in total incentives is associated with a 0.37% increase in exit rates, underlining the stabilizing role of incentives for existing businesses. Beyond these aggregate effects, firm-level analysis in Table A1 further shows that larger incentives increase the move-in rate of startups by 2.8 percentage points, whereas the move-out rate rises by only 1.5 percentage points. This asymmetry suggests that incentives are more effective at attracting new firms than at preventing existing firms from leaving.

A closer examination of specific types of incentives, presented in Tables 6 and 7 for increases and decreases, reveals varying levels of effectiveness.[13] Among all incentives, investment tax credits have the clearest positive effect on startup creation and survival. A large increase in investment tax credits results in a 1.92% rise in employer startups, as shown in Panel B of Table 6 and Figure A12a. Conversely, reductions in investment tax credits, detailed in Panel B of Table 7 and Figure A12b, negatively affect total employment, emphasizing their role in supporting labor-intensive growth.

[13]PDIT includes five types of incentives, but job training subsidies are excluded due to limited variability over time.

Similarly, R&D tax credits exhibit strong effects in fostering high-growth, innovation-driven enterprises. Panel C of Table 6 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 implies that R&D support helps new tech or innovation-oriented firms to scale up their hiring. Conversely, reductions in R&D tax credits, as presented in Panel C of Table 7, lead to a 1.21% rise in exit rates, implying that some innovation-driven startups may fail without that support.

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 A of Table 6. 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 is that property tax abatements may often benefit existing firms or relocating firms more than smaller startups.[14] 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.

For local regulations, we analyze three key policies: the minimum wage, UI benefits duration, and bankruptcy exemption. We find that increases in minimum wages exhibit significant negative impacts on startup activity.[15] Panel C of Table 2 indicates that a large increase in minimum wages reduces nonemployer startups by 2.52% and employer startups by 1.87%. Intuitively, higher wage floors raise labor costs for prospective employers, which can deter individuals from starting businesses that rely on hired workers. Also, some nonemployers start businesses as an alternative to low-wage jobs; if the wage floor rises, staying in paid employment becomes relatively more attractive than starting a marginal business. Figures A5a and A5d show that these effects occur with a delay, especially for employers. These declines are further reflected in total startup employment, which falls by 3.58% with a delay, as shown in Figure A5b. Additionally, exit rates rise modestly by 0.22% when minimum wages rise, as depicted in Figure A5c. Moreover, startups that do launch in high minimum wage environments face greater difficulty in survival, as the exit rate increases by 0.22%. These findings highlight how higher labor costs hinder small businesses' growth and viability, particularly in labor-intensive sectors. Firm-level results in Table A1 in Appendix A confirm the negative effects of minimum wage increases on startup dynamics. A 1 US$ increase in minimum wages decreases employment in startups by 2.37% and raises the probability that a startup closes by 0.22 percentage points.

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

[15] There were no minimum wage decreases in our period of analysis.

Minimum wages also affect the mobility of startups statistically significantly, although the effects sizes are small: A 1 US$ higher minimum wage leads to a 0.04 percentage points higher probability that a startup leaves a state and a 0.06 percentage points lower probability of moving in.

Unemployment insurance generosity can influence entrepreneurship by affecting workers' incentives and the availability of employees. We examine changes in the maximum duration of unemployment benefits. The results, as shown in Panel E in Table 2 and Panel C in Table 3, indicate that increasing UI benefits tends to reduce employer startups. When the duration of UI is increased, the number of new employer startups falls by 3.07% (Figure A6a), and shortening the duration of UI has the opposite effect: it significantly boosts the number of employer startups by 10.9% and the total employment at startups by 10.6% (Figures A6b and A6c). One possible mechanism is that more generous UI benefits makes potential employees less eager to immediately seek jobs at startups, which are riskier and typically offer lower wages and benefits than larger firms, making it harder for startups to recruit workers. When unemployment benefits are cut back, new firms find it easier to hire employees. There is not a strong effect of UI changes on nonemployer startups — the primary impact is on employers, which is plausible given the hiring mechanism.

Personal bankruptcy laws set the amount of homestead assets that are protected if an entrepreneur fails and declares bankruptcy. Panel D of Table 2 indicates that expanding homestead bankruptcy exemptions in private bankruptcy law significantly increases the rate of business closures by 0.22 percentage points (Figure A7b). This suggests that more forgiving bankruptcy laws enable struggling businesses to close more readily, leveraging the financial safety net provided.

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), incentives, and UI benefits duration shocks, while nonemployer startups are more sensitive to PIT, minimum wage and bankruptcy exemption.

### 5.3 Heterogeneity Analysis

We uncover significant geographic heterogeneity by comparing border counties (counties located along a state border) with interior counties (away from state borders). The idea is that a county on the border might see entrepreneurs "vote with their feet" — if one state enacts an unfavorable policy, startups might relocate to a neighboring county just across the state line with a stable policy; likewise, the implementation of a favorable state policy might attract startups from across the state border. In fact, policymakers often cite competition between states as a motivation for state policy changes. Moving across state borders is more costly for startups located in interior counties because the entrepreneurs and their employees are less likely

Table 2: Effect of Large Policy Increases on Startup Outcomes

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate | (6) Move-in Rate |
|---|---|---|---|---|---|---|
| **Panel A: Total Taxes** | | | | | | |
| $Treat_{ct}$ | -0.0291*** | -0.0546*** | -0.0640*** | 0.0019 | -0.0003 | 0.000955 |
| | (0.0073) | (0.0095) | (0.0162) | (0.0057) | (0.0002) | (0.00135) |
| No. of Obs. | 34,500 | 34,500 | 34,500 | 34,500 | 34,500 | 34,500 |
| **Panel B: Total Incentives** | | | | | | |
| $Treat_{ct}$ | 0.0040 | 0.0421*** | 0.0524* | -0.0053** | -0.0006* | -0.000699 |
| | (0.0160) | (0.0103) | (0.0283) | (0.0026) | (0.0003) | (0.000432) |
| No. of Obs. | 34,500 | 34,500 | 34,500 | 34,500 | 34,500 | 34,500 |
| **Panel C: Minimum Wage (City)** | | | | | | |
| $Treat_{ct}$ | -0.0252*** | -0.0187*** | -0.0358** | 0.0022** | -0.0006*** | -0.00106* |
| | (0.0072) | (0.0072) | (0.0149) | (0.0009) | (0.0002) | (0.000635) |
| No. of Obs. | 55,000 | 55,000 | 55,000 | 55,000 | 55,000 | 55,000 |
| **Panel D: Bankruptcy Exemption** | | | | | | |
| $Treat_{ct}$ | -0.0144* | 0.0097 | 0.0082 | 0.0022** | 0.0002 | -0.00067 |
| | (0.0076) | (0.0074) | (0.0181) | (0.0011) | (0.0003) | (0.000928) |
| No. of Obs. | 40,000 | 40,000 | 40,000 | 40,000 | 40,000 | 40,000 |
| **Panel E: UI Duration (weeks)** | | | | | | |
| $Treat_{ct}$ | -0.0110 | -0.0307*** | -0.0187 | -0.0070** | 0.0004 | 0.000708 |
| | (0.0137) | (0.0107) | (0.0187) | (0.0035) | (0.0006) | (0.000969) |
| No. of Obs. | 21,500 | 21,500 | 21,500 | 21,500 | 21,500 | 21,500 |

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 Callaway and Sant'Anna (2021).

A large increase generally refers to the top 5% of the distribution of policy changes. If the 5th 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, as all firms in the sample are age 0), 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 3: Effect of Large Policy Decreases on Startup Outcomes

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate | (6) Move-in Rate |
|---|---|---|---|---|---|---|
| **Panel A: Total Taxes** | | | | | | |
| $Treat_{ct}$ | -0.0149** | 0.0117 | 0.0455** | -0.0036** | 0.0003* | 0.000746 |
| | (0.0066) | (0.0083) | (0.0199) | (0.0015) | (0.0002) | (0.000464) |
| No. of Obs. | 36,500 | 36,500 | 36,500 | 36,500 | 36,500 | 36,500 |
| **Panel B: Total Incentives** | | | | | | |
| $Treat_{ct}$ | -0.0131** | 0.0076 | -0.0114 | -0.0037** | 0.0003 | 0.00103** |
| | (0.0063) | (0.0137) | (0.0206) | (0.0015) | (0.0003) | (0.000461) |
| No. of Obs. | 35,500 | 35,500 | 35,500 | 35,500 | 35,500 | 35,500 |
| **Panel C: UI Duration (weeks)** | | | | | | |
| $Treat_{ct}$ | -0.0497*** | 0.1090*** | 0.1060** | -0.0250*** | -0.0008 | -0.00129 |
| | (0.0145) | (0.0389) | (0.0504) | (0.0055) | (0.0007) | (0.00128) |
| No. of Obs. | 21,500 | 21,500 | 21,500 | 21,500 | 21,500 | 21,500 |

This table shows the effects of large 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 policy changes. If the 5th percentile was equal to the median (usually zero), the bottom 1% was 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, as all firms in the sample are age 0), 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).

to be able to commute to the new workplace without relocating their home, and it is less likely that they can maintain their existing relationships with customers and suppliers. Therefore, if relocation of startups across state borders is a relevant response to policy changes, we expect to see larger effects in border than in interior counties.

Tables A2, A3, A4, and A5 contrast the effects of policy increases and decreases on startup outcomes in border and interior counties. Our analysis indeed finds differential reactions consistent with cross-border movement or competition. Specifically, changes in tax incentives show stronger effects in state border areas: when a state increases its incentives, border counties experience an especially large jump in employer startup formation and employment, and a sharp drop in exits. An increase in total incentives raises the number of employer startups by 4.41% in border counties versus 2.51% in interior counties. This implies that firms might be flocking to or staying in the state in response to better incentives, particularly in regions where crossing the border is an easy option. Likewise, when incentives are reduced, border counties see a larger decline in nonemployer startups than interior counties — possibly because some entrepreneurs in border regions relocate their one-person businesses to a neighboring state with better incentives. In contrast, interior counties are less sensitive to incentive changes, showing only modest declines in nonemployers when incentives drop. Incentive cuts reduce nonemployer startups more severely in border regions by 1.72% versus 1.37% in interior counties. This pattern suggests that policy competition is most acute at state borders: a

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

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate |
|---|---|---|---|---|---|
| **Panel A: Property Tax (Upjohn)** | | | | | |
| $Treat_{ct}$ | 0.0155 | -0.0057 | 0.0027 | -0.0017 | -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 |
| **Panel B: Corporate Income Tax (Upjohn)** | | | | | |
| $Treat_{ct}$ | -0.0248*** | -0.0313*** | -0.0106 | -0.0013 | 0.0003 |
| | (0.0049) | (0.0062) | (0.0159) | (0.0009) | (0.0002) |
| No. of Obs. | 36,000 | 36,000 | 36,000 | 36,000 | 36,000 |
| **Panel C: Corporate Income Tax State** | | | | | |
| $Treat_{ct}$ | 0.0410* | 0.0712*** | 0.0731*** | 0.0005 | 0.0000 |
| | (0.0224) | (0.0157) | (0.0271) | (0.0018) | (0.0007) |
| No. of Obs. | 60,000 | 60,000 | 60,000 | 60,000 | 60,000 |
| **Panel D: Personal Income Tax State** | | | | | |
| $Treat_{ct}$ | -0.0301 | -0.0034 | -0.0130 | -0.0009 | -0.0003 |
| | (0.0284) | (0.0461) | (0.0275) | (0.0020) | (0.0008) |
| No. of Obs. | 65,000 | 65,000 | 65,000 | 65,000 | 65,000 |
| **Panel E: Sales Tax (Upjohn)** | | | | | |
| $Treat_{ct}$ | 0.0134 | -0.0278** | -0.0169 | 0.0024 | 0.0003 |
| | (0.0095) | (0.0125) | (0.0185) | (0.0017) | (0.0003) |
| No. of Obs. | 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 5th 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. Corporate income tax state and personal income tax state are the top marginal rates in the state.

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, as all firms in the sample are age 0), ln(Population), percentage of individuals aged 25 to 64, high school graduation percentage, college graduation percentage, female percentage, African American percentage, Asian percentage, and Hispanic percentage.

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

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

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

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

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate |
|---|---|---|---|---|---|
| **Panel A: Property Tax (Upjohn)** | | | | | |
| $Treat_{ct}$ | -0.0136** | 0.0200** | 0.0402** | -0.0053** | 0.0006* |
| | (0.0059) | (0.0078) | (0.0179) | (0.0018) | (0.0003) |
| No. of Obs. | 36,500 | 36,500 | 36,500 | 36,500 | 36,500 |
| **Panel B: Corporate Income Tax (Upjohn)** | | | | | |
| $Treat_{ct}$ | 0.0329*** | 0.0471*** | 0.0391** | -0.0059** | -0.0007* |
| | (0.0065) | (0.0082) | (0.0233) | (0.0024) | (0.0003) |
| No. of Obs. | 33,000 | 33,000 | 33,000 | 33,000 | 33,000 |
| **Panel C: Corporate Income Tax State** | | | | | |
| $Treat_{ct}$ | 0.0191*** | 0.0386*** | 0.0443*** | 0.0000 | -0.0007** |
| | (0.0057) | (0.0072) | (0.0162) | (0.0010) | (0.0002) |
| No. of Obs. | 55,000 | 55,000 | 55,000 | 55,000 | 55,000 |
| **Panel D: Personal Income Tax State** | | | | | |
| $Treat_{ct}$ | 0.0210*** | 0.0196** | 0.0497** | -0.0002 | -0.0001 |
| | (0.0051) | (0.0076) | (0.0210) | (0.0010) | (0.0002) |
| No. of Obs. | 55,000 | 55,000 | 55,000 | 55,000 | 55,000 |

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. Property tax and corporate income tax are sourced from the Upjohn dataset. Corporate income tax state and personal income tax state are the top marginal rates in the state.

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, as all firms in the sample are age 0), 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 6: Effect of Large Incentive Policy Increases on Startup Outcomes

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate |
|---|---|---|---|---|---|
| **Panel A: JCTC (Upjohn)** | | | | | |
| $Treat_{ct}$ | -0.0171 | 0.0282** | 0.0269 | -0.0053 | -0.0006 |
| | (0.0185) | (0.0118) | (0.0294) | (0.0029) | (0.0004) |
| No. of Obs. | 34,000 | 34,000 | 34,000 | 34,000 | 34,000 |
| **Panel B: Investment Tax Credit (Upjohn)** | | | | | |
| $Treat_{ct}$ | 0.0219** | 0.0192* | -0.0086 | -0.0024* | 0.0001 |
| | (0.0067) | (0.0073) | (0.0195) | (0.0012) | (0.0003) |
| No. of Obs. | 35,500 | 35,000 | 35,000 | 35,000 | 35,000 |
| **Panel C: Research and Development Credit (Upjohn)** | | | | | |
| $Treat_{ct}$ | 0.0177 | 0.0705** | 0.0750* | 0.0009 | -0.0019* |
| | (0.0216) | (0.0268) | (0.0390) | (0.0040) | (0.0010) |
| No. of Obs. | 33,500 | 33,500 | 33,500 | 33,500 | 33,500 |
| **Panel D: Property Tax Abatement (Upjohn)** | | | | | |
| $Treat_{ct}$ | -0.0037 | -0.0145* | 0.0173 | -0.0019 | -0.0001 |
| | (0.0054) | (0.0060) | (0.0160) | (0.0010) | (0.0002) |
| No. of Obs. | 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 5th percentile was equal to the median (usually zero), the top 1% was used instead. Job creation tax credit, investment tax credit, and research and development credit are sourced from the Upjohn dataset. Property tax abatement reflects local policies on business incentives.

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, as all firms in the sample are age 0), 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 7: Effect of Large Incentive Policy Decreases on Startup Outcomes

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate |
|---|---|---|---|---|---|
| **Panel A: JCTC (Upjohn)** | | | | | |
| $Treat_{ct}$ | 0.0308 | -0.0043 | -0.0469 | -0.0004 | 0.0019 |
| | (0.0200) | (0.0245) | (0.0505) | (0.0021) | (0.0009) |
| No. of Obs. | 34,000 | 34,000 | 34,000 | 34,000 | 34,000 |
| **Panel B: Investment Tax Credit (Upjohn)** | | | | | |
| $Treat_{ct}$ | 0.0015 | -0.0128 | -0.0334* | -0.0015 | -0.0001 |
| | (0.0055) | (0.0067) | (0.0162) | (0.0010) | (0.0002) |
| No. of Obs. | 35,500 | 35,500 | 35,500 | 35,500 | 35,500 |
| **Panel C: Research and Development Credit (Upjohn)** | | | | | |
| $Treat_{ct}$ | 0.0212** | -0.0099 | 0.0116 | -0.0121** | -0.0013* |
| | (0.0059) | (0.0190) | (0.0437) | (0.0042) | (0.0008) |
| No. of Obs. | 35,000 | 35,000 | 35,000 | 35,000 | 35,000 |
| **Panel D: Property Tax Abatement (Upjohn)** | | | | | |
| $Treat_{ct}$ | -0.0139** | -0.0105 | -0.0011 | -0.0022 | -0.0001 |
| | (0.0060) | (0.0116) | (0.0222) | (0.0016) | (0.0002) |
| No. of Obs. | 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, and research and development credit are sourced from the Upjohn dataset. Property tax abatement reflects local policies on business incentives.

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, as all firms in the sample are age 0), 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).

county whose neighbors are in different jurisdictions faces a higher risk of losing startups if its state's policies become unattractive.

For total taxes, we observe a similar pattern: border counties react more strongly to tax increases, whereas interior counties are somewhat insulated. When taxes go up, border counties see a pronounced decrease in employer startups, but in interior counties that decrease is smaller (5.27% for border, and 5.08% for interior counties). However, the effect on employment is more pronounced in interior counties, compared with border counties (5.61% for interior counties, insignificant for border counties). This could be because some entrepreneurs in a high-tax border county can easily shift their activities to a neighboring lower-tax state, meaning the treated border county does lose as many startups.

For bankruptcy exemption changes, we find a similar sensitivity in border counties: a rise in the bankruptcy exemption leads to a larger decrease in nonemployer startups and a bigger increase in exits in border counties compared with interior ones (Tables A2 and A4). For minimum wage and UI policies, the border versus interior analysis did not reveal a clear pattern. Both border and interior counties showed mixed effects when those policies changed, with no consistent difference in magnitude. We do not see a border-specific moderation or amplification for these policies.

### 5.4 Robustness Checks

The universe of startups in our main sample includes newly started one-person businesses that have no intention of growing or hiring workers (Hurst and Pugsley, 2011). Firms with the potential of growth and job creation have more impact on the economy and are therefore in the focus of policymaking. One way to identify startups with more growth ambition in the data is to screen for startups that register for an employer identification number (EIN). These firms include not only employers but also nonemployers that are typically intending to hire or incorporate. In a set of robustness checks, we only keep EIN-holding startups in the analysis sample. The results mirror those from the main analysis but with smaller effect sizes. In other words, policy shocks still push these firms in the same direction, but the changes are not as large. For example, in Tables A6 and A7, the effect sizes of policy changes are reduced compared to Tables 2 and 3. This implies that startups with employees or poised to have employees might be somewhat more buffered — perhaps because they are on average larger, more prepared, or have better access to resources, making them less sensitive to policy than the universe of all startups which includes very small one-person businesses. Nonetheless, the direction of effects remains aligned, confirming that our main findings are not driven solely by startups without growth ambition.

Another concern could be that when we analyze the effects of a large change in one policy, other policies

might also change. Therefore, in a second set of robustness checks, we control for changes in policies other than the focal policy. The DID approach for panel data used in our main analysis suggested by Callaway and Sant'Anna (2021) does not allow for time-varying control variables. 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. The method involves stacking multiple policy-change events into a unified event-study framework. Each policy event is treated as a separate mini-DID and combined, allowing us to control for overlapping interventions. We run the stacked DID regressions with and without adding controls for contemporaneous policy changes. A key finding from the stacked DID estimates, as shown in Tables A8, A9, A11, and A12, is that the results are almost the same with or without the inclusion of other policy controls. This consistency illustrates that the main results are not driven by other concurrent policy changes.

# 6 Conclusion

This study provides evidence that state-level policies, including taxes, incentives, and regulations, affect extensive and intensive startup activity. We analyze novel administrative data on the universe of employer and nonemployer startups in the United States. Our findings demonstrate that the effects of these policies vary significantly by policy type and the type of startup impacted.

First, the analysis reveals that total state 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. We also find distinct responses of nonemployer and employer startups to fiscal policies, potentially driven by differences in scale, resource needs, and operational constraints. For example, CIT reductions drive employer startup growth, whereas PIT reductions benefit nonemployer businesses, suggesting that targeted tax reforms could be effective in fostering different segments of entrepreneurship.

Incentive policies are effective in supporting startup growth and survival. Expanding investment and R&D tax credits significantly increases employer startup formation and employment, while reducing exit rates. In particular, R&D tax credits not only promote business entry but also drive employment growth, highlighting their role in fostering innovation. Labor market regulations impose significant constraints on startup activity. Minimum wage increases reduce both nonemployer and employer startups, decrease employment, and increase exit rates, with adverse effects becoming more pronounced over time. Extending unemployment insurance (UI) benefits duration discourages employer startup formation and job creation, as potential entrepreneurs or employees delay entering the labor market. These results indicate that labor poli-

cies should be carefully balanced to avoid discouraging new business formation, particularly among employer startups that contribute to job creation.

Our findings provide several policy insights. Tax policy should be designed to minimize burdens on new and small businesses, as excessive tax increases directly reduce startup formation. Incentive policies also represent a potentially effective tool, particularly for employer startups that benefit from investment and R&D tax credits. Cutting these programs might disproportionately harm high-growth firms, limiting job creation and long-term economic expansion. Concerning labor policies, including minimum wages and UI duration regulations, their effects on startup activity should be considered. While worker protections are important, excessive labor costs discourage new business formation and increase the risk of firm closures. Policymakers could explore alternative approaches such as wage subsidies for new businesses or tiered minimum wage policies to support entrepreneurship while maintaining labor protections. Interstate competition for entrepreneurs underscores the need for strategic policymaking. States should recognize that firms respond to tax and incentive differentials, meaning that policy changes can either attract or drive away business activity. Coordination between neighboring states or tailored regional policies could help mitigate unintended business relocation effects.

Overall, our study underscores that government 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 and higher labor costs tend to suppress startup activity, while tax reductions and targeted incentives stimulate it. Business incentives, in particular, provide critical support for employer startups, while tax and labor policies affect both firm formation and survival rates. Entrepreneurs are responsive to policy environments, adjusting their location and business decisions accordingly. Policymakers aiming to foster entrepreneurship should prioritize targeted tax reforms, sustain effective incentive programs, and consider the potential trade-offs of labor policies.

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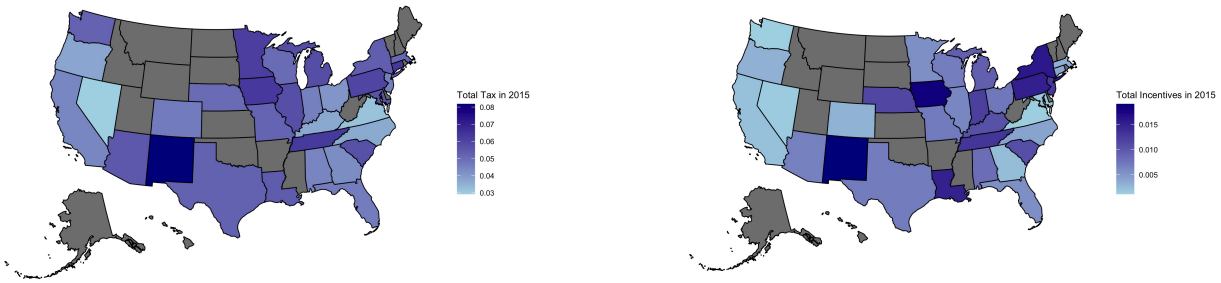
# Appendices

## A Supplemental Figures



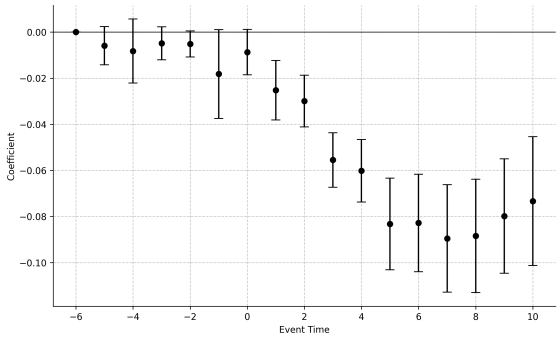

(a) Minimum Wages in 2021 (b) Personal Income Tax Rates in 2021

Figure A1: State-level Comparisons: Minimum Wage, and Personal Income Tax

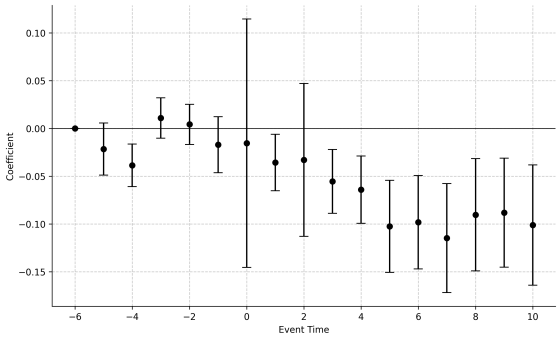



(a) Total Taxes in 2015 (b) Total Incentives in 2015

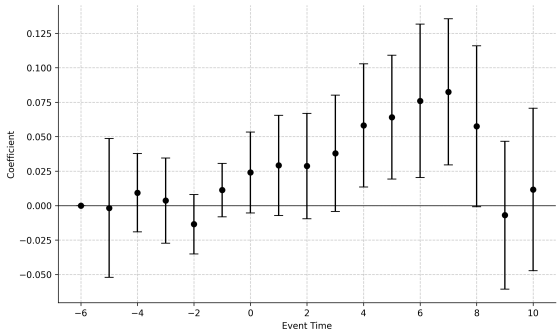
Figure A2: State-level comparisons: total Tax, and total Incentives
*Note:*Grey color indicates unavailable data. Source: Own illustration based on Upjohn Institute's PDIT. *Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



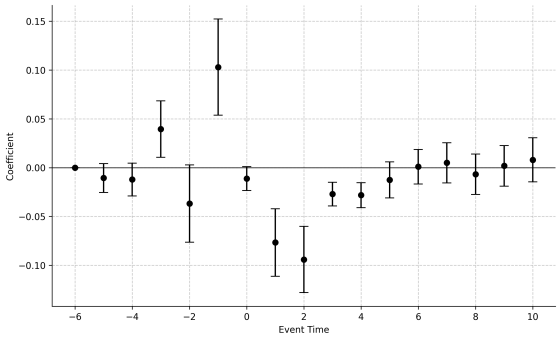

(a) Effect of total tax increases on ln(Employers)



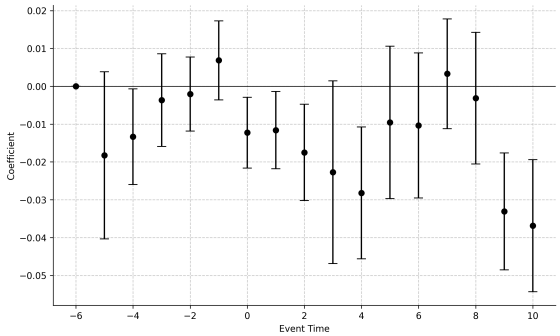

(b) Effect of total tax increases on ln(Employment)



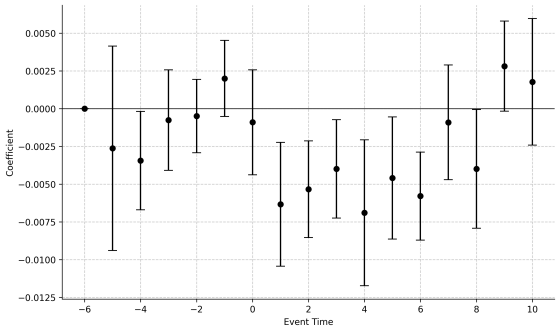

(c) Effect of total tax decreases on ln(Employment)



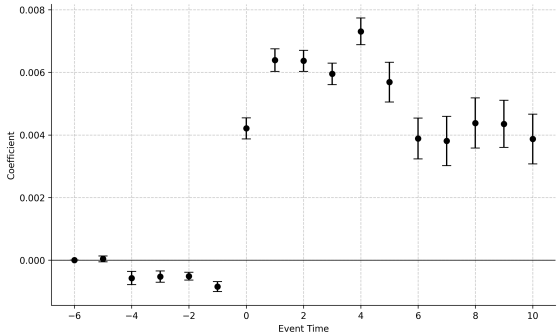

(d) Effect of total tax increases on ln(Nonemployers)



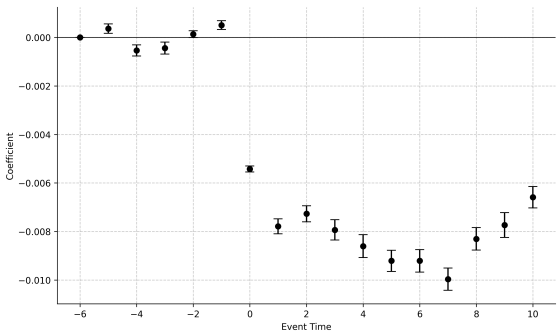

(e) Effect of total tax decreases on ln(Nonemployers)




(f) Effect of total tax decreases on exit rate

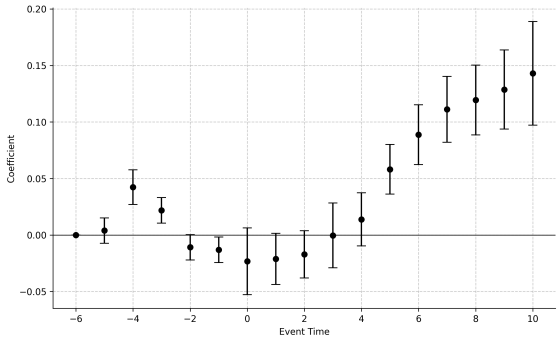



(g) Effect of total tax increases on total tax

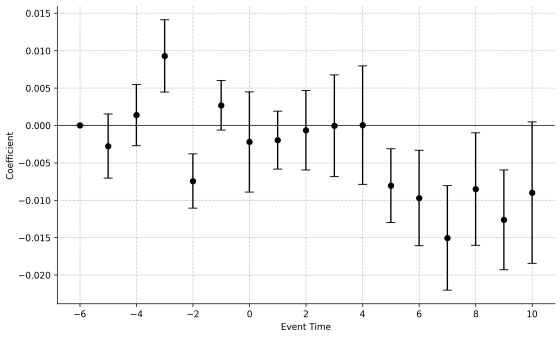



(h) Effect of total tax decreases on total tax

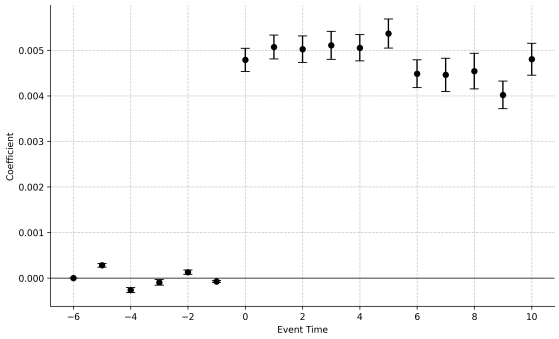
Figure A3: Effects of total tax increases and decreases on entrepreneurial outcomes
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).




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

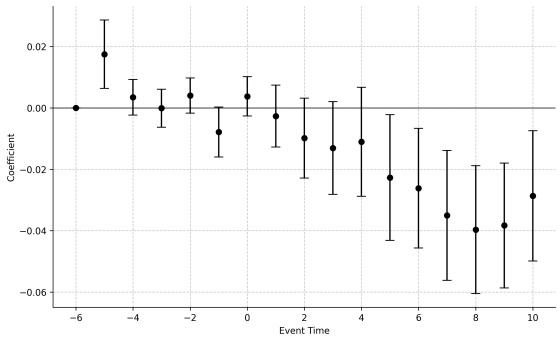



(b) Effect of total incentive increases on exit rate

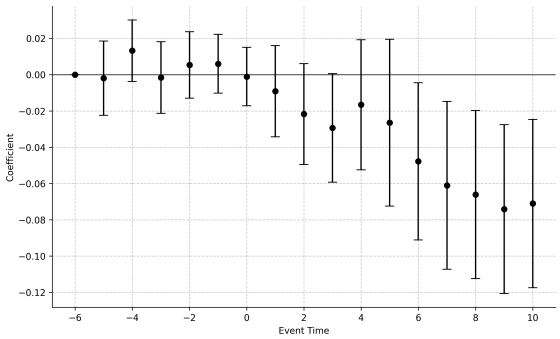



(c) Effect of total incentive increases on total incentive

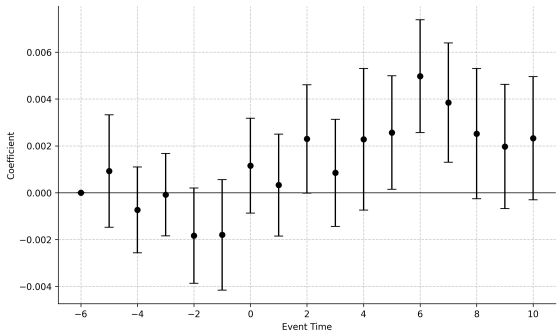
Figure A4: Effects of total incentive increases on entrepreneurial outcomes
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



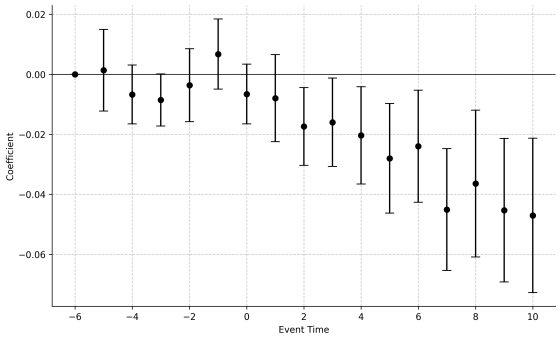

(a) Effect of minimum wages increase on ln(Employers)



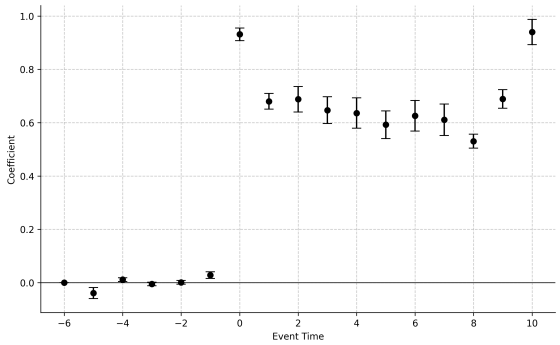

(b) Effect of minimum wages increase on ln(Employment)




(c) Effect of minimum wages increase on exit rate

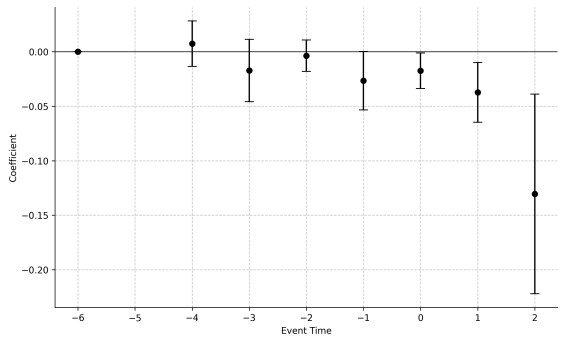



(d) Effect of minimum wages increase on ln(Nonemployers)

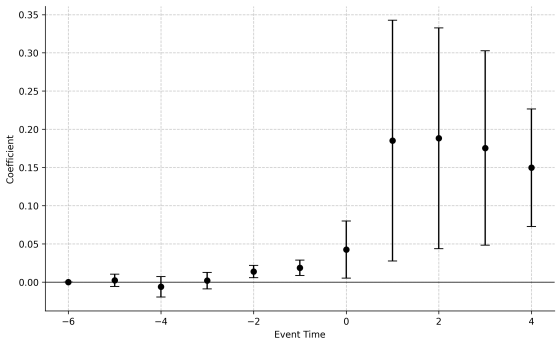



(e) Effect of minimum wages increase on minimum wages

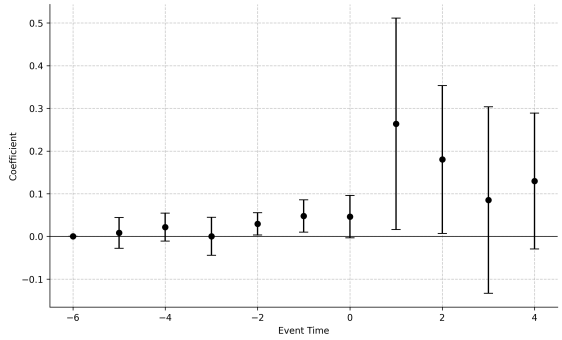
Figure A5: Effects of minimum wages increases on entrepreneurial outcomes
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



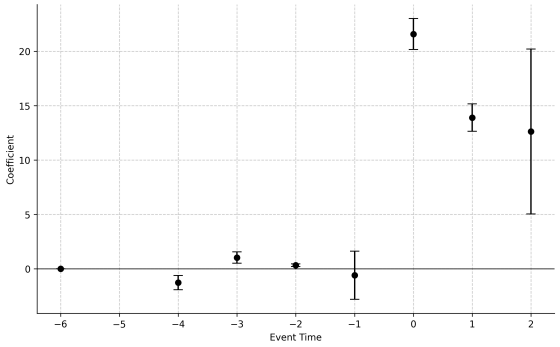

(a) Effect of unemployment insurance duration increases on ln(Employers)



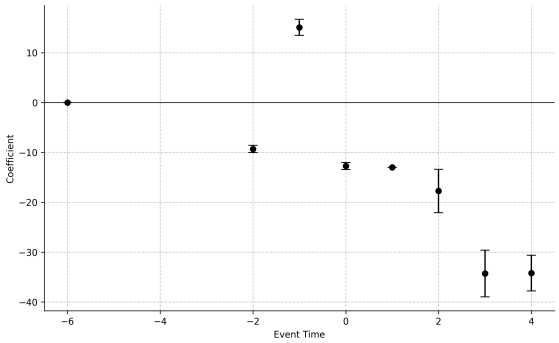

(b) Effect of unemployment insurance duration decreases on ln(Employers)




(c) Effect of unemployment insurance duration decreases on ln(Employment)

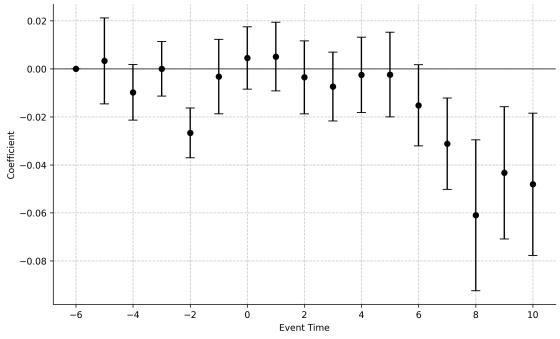



(d) Effect of unemployment insurance duration increases on unemployment insurance

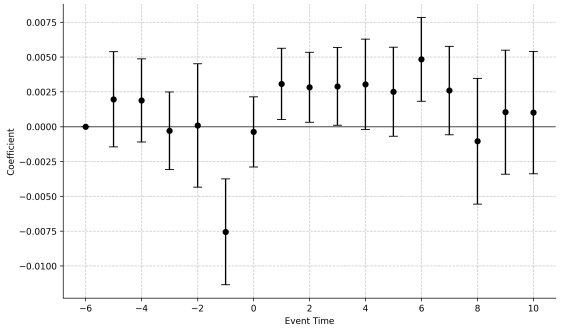



(e) Effect of unemployment insurance duration decreases on unemployment insurance

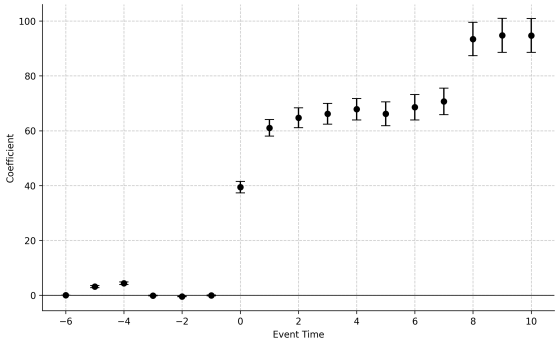
Figure A6: Effects of unemployment insurance duration increases and decreases on entrepreneurial outcomes *Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).




(a) Effect of bankruptcy exemption increases on ln(Nonemployers)

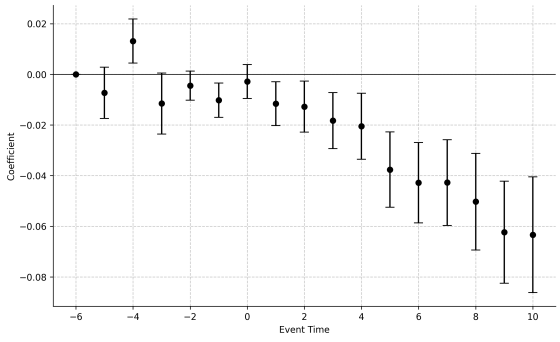



(b) Effect of bankruptcy exemption increases on exit rate

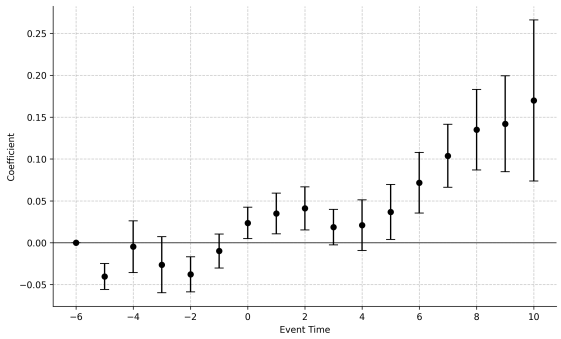



(c) Effect of bankruptcy exemption increases on bankruptcy exemption

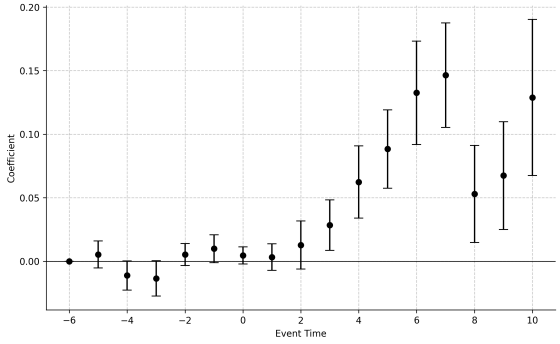
Figure A7: Effects of bankruptcy exemption increases on entrepreneurial outcomes
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



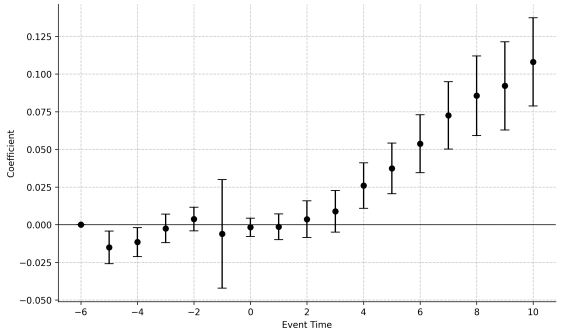

(a) Effect of CIT (Upjohn) increases on ln(Employers)



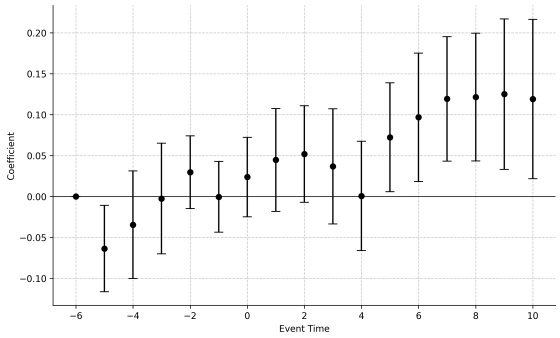

(b) Effect of CIT (State) increases on ln(Employers)



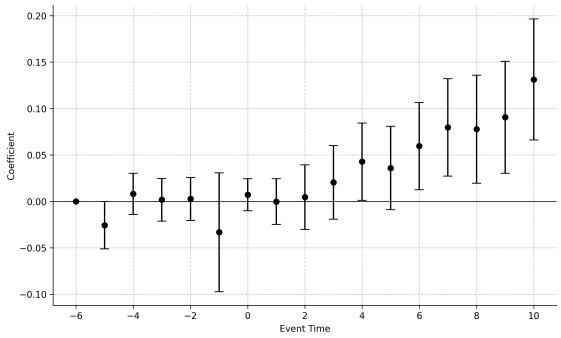

(c) Effect of CIT (Upjohn) decreases on ln(Employers)



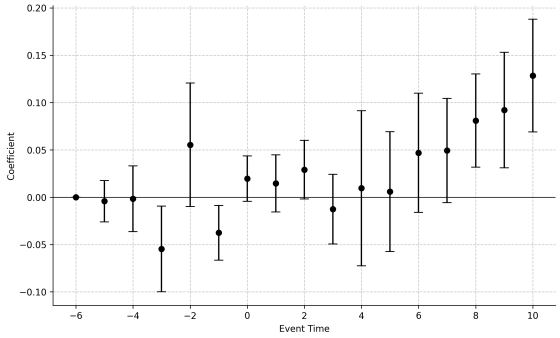

(d) Effect of CIT (State) decreases on ln(Employers)



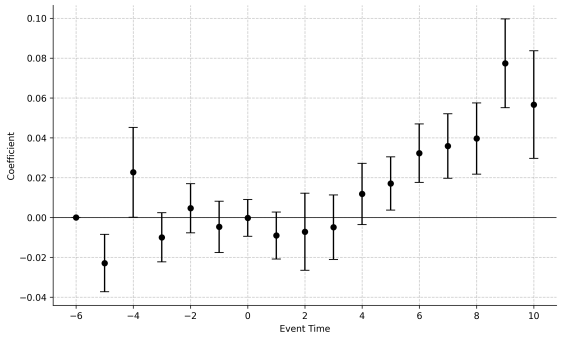

(e) Effect of CIT (State) increases on ln(Employment)




(f) Effect of CIT (State) decreases on ln(Employment)



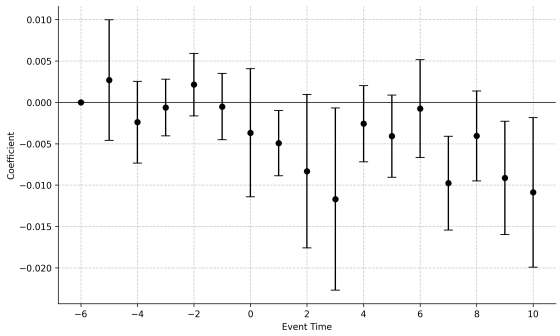

(g) Effect of CIT (State) increases on ln(Nonemployers)



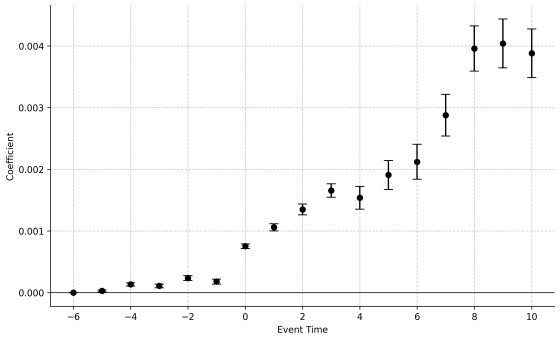

(h) Effect of CIT (State) decreases on ln(Nonemployers)

Figure A8: Effects of CIT increases and decreases on entrepreneurial outcomes

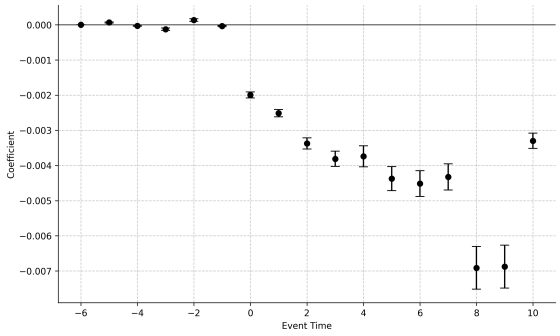
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



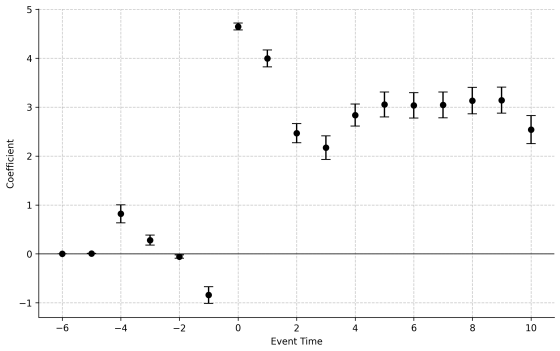

(a) Effect of CIT (Upjohn) decreases on exit rate



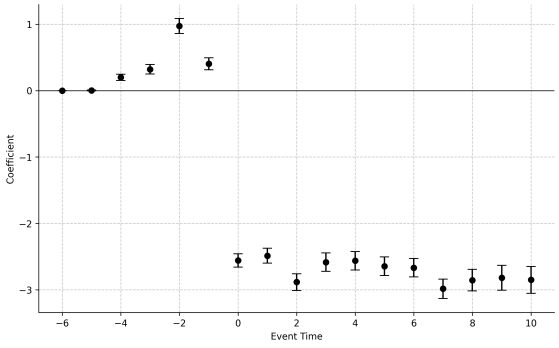

(b) Effect of CIT (Upjohn) increases on CIT (Upjohn)




(c) Effect of CIT (Upjohn) decreases on CIT (Upjohn)

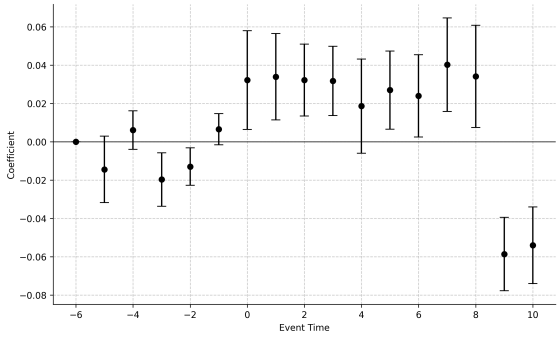



(d) Effect of CIT (State) increases on CIT (State)

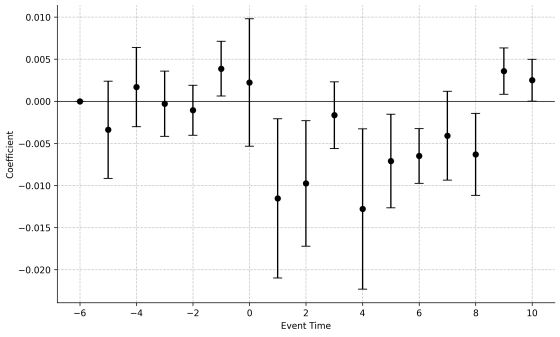



(e) Effect of CIT (State) decreases on CIT (State)

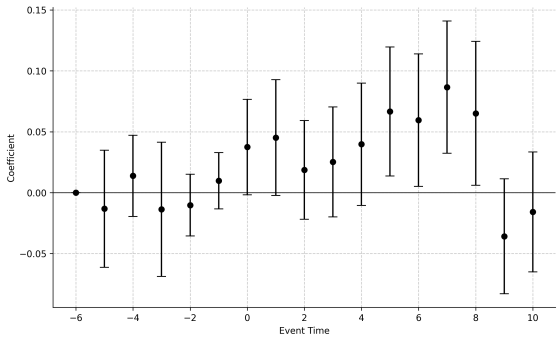
Figure A9: Effects of CIT increases and decreases on entrepreneurial outcomes
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



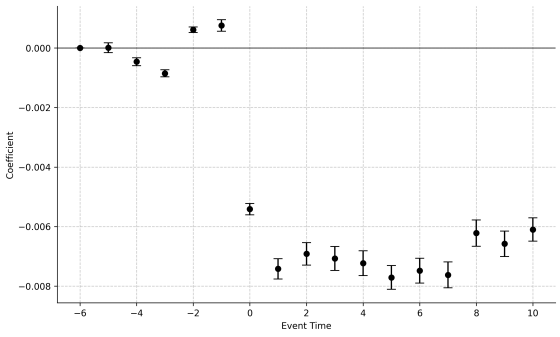

(a) Effect of property tax decreases on ln(Employers)




(b) Effect of property tax decreases on exit rate

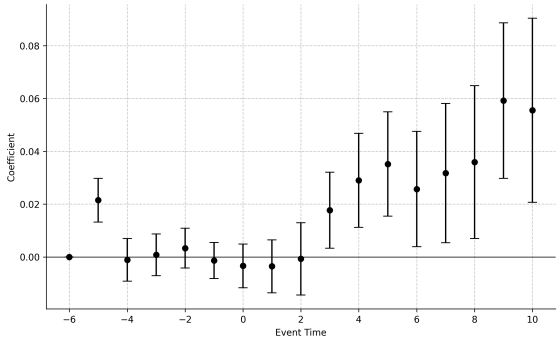



(c) Effect of property tax decreases on ln(Employment)

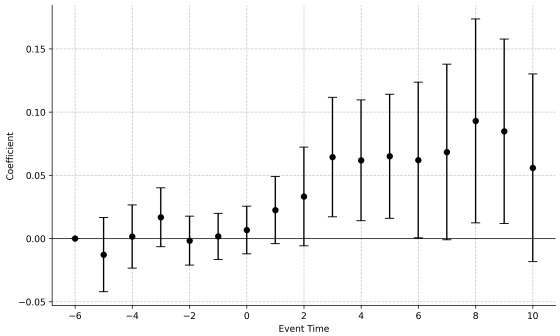



(d) Effect of property tax decreases on property tax

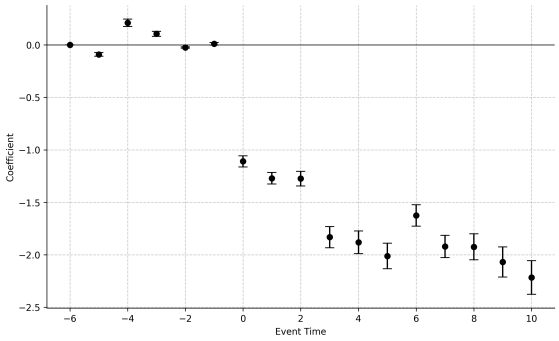
Figure A10: Effects of property tax decreases on entrepreneurial outcomes
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).




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

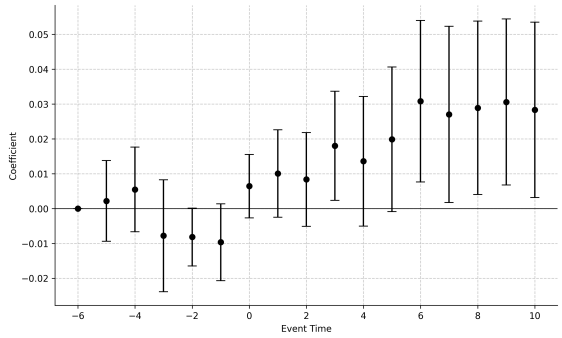



(b) Effect of PIT decreases on ln(Employment)

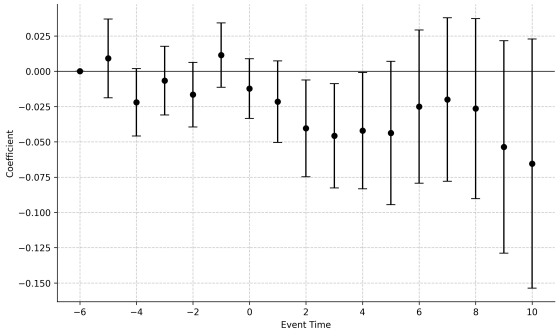



(c) Effect of PIT decreases on PIT

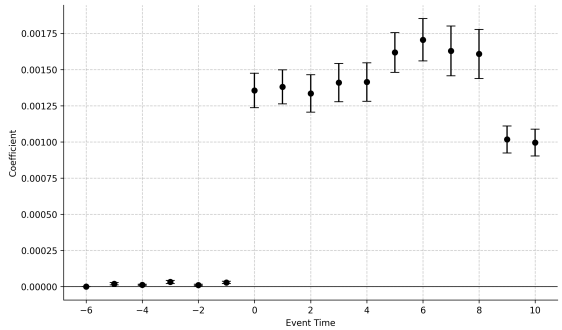
Figure A11: Effects of PIT decreases on entrepreneurial outcomes
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).



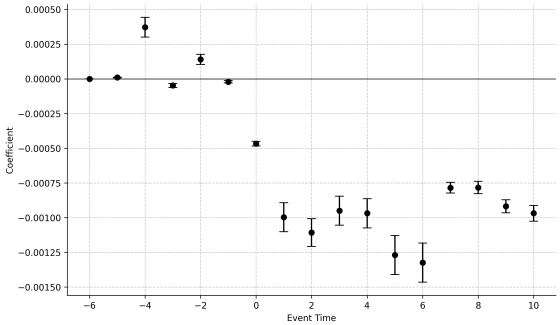

(a) Effect of investment tax credits increase on ln(Employers)




(b) Effect of investment tax credits decrease on ln(Employment)




(c) Effect of investment tax credits increase on investment tax credits




(d) Effect of investment tax credits decrease on investment tax credits

Figure A12: Effects of investment tax credit increases and decreases on entrepreneurial outcomes
*Data source*: Comprehensive Startup Panel, FSRDC Project No. 2936 (CBDRB-FY25-P2936-R11747, CBDRB-FY25-P2936-R12020).

# B Supplemental Tables

Table A1: Effects of Policies on Startup Outcomes (Firm Level)

| | (1) Transition | (2) ΔEmployment | (3) Exit | (4) Move-Out | (5) Move-In |
|---|---|---|---|---|---|
| **Panel A: Minimum Wage (City)** | | | | | |
| *Minimum Wage*$_{ct}$ | -0.0001 | -0.0237*** | 0.0022*** | 0.0004*** | -0.0006** |
| | (0.0001) | (0.0089) | (0.0006) | (0.0001) | (0.0002) |
| No. of Obs. | 168,700,000 | 268,800,000 | 303,600,000 | 303,400,000 | 242,000,000 |
| **Panel B: Total Taxes** | | | | | |
| *Total Taxes*$_{ct}$ | 0.0245** | 0.1850 | 0.1680* | 0.0357*** | 0.131* |
| | (0.0436) | (1.1910) | (0.0982) | (0.0159) | (0.0644) |
| No. of Obs. | 111,700,000 | 176,300,000 | 201,100,000 | 200,900,000 | 151,000,000 |
| **Panel C: Total Incentives** | | | | | |
| *Total Incentives*$_{ct}$ | -0.0229 | 0.9610 | 0.1620* | 0.0272** | 0.0618* |
| | (0.0177) | (2.8660) | (0.1170) | (0.0167) | (0.0439) |
| No. of Obs. | 111,700,000 | 176,300,000 | 201,100,000 | 200,900,000 | 151,000,000 |

This table shows the effects of minimum wage, total taxes, and total incentives on startup outcomes at the firm level. The estimates are obtained using a two-way fixed effects model, controlling for firm and year fixed effects.

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

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 A2: Effect of Large Policy Increases on Startup Outcomes (Border Counties)

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate |
|---|---|---|---|---|---|
| **Panel A: UI Duration (weeks)** | | | | | |
| $Treat_{ct}$ | -0.0359*** | 0.0086 | 0.0391 | -0.0022 | -0.0001 |
| | (0.0133) | (0.0127) | (0.0260) | (0.0030) | (0.0005) |
| Counties | 6,000 | 6,000 | 6,000 | 6,000 | 6,000 |
| **Panel B: Total Taxes** | | | | | |
| $Treat_{ct}$ | -0.0093 | -0.0527*** | -0.0244 | 0.0028 | -0.0001 |
| | (0.0120) | (0.0131) | (0.0253) | (0.0018) | (0.0004) |
| Counties | 13,000 | 13,000 | 13,000 | 13,000 | 13,000 |
| **Panel C: Total Incentives** | | | | | |
| $Treat_{ct}$ | -0.0190 | 0.0441*** | 0.0647** | -0.0059*** | 0.0003 |
| | (0.0142) | (0.0143) | (0.0321) | (0.0022) | (0.0005) |
| Counties | 12,500 | 12,500 | 12,500 | 12,500 | 12,500 |
| **Panel D: Minimum Wage (City)** | | | | | |
| $Treat_{ct}$ | -0.0152 | -0.0286** | -0.0188 | -0.0000 | 0.0007 |
| | (0.0464) | (0.0112) | (0.0250) | (0.0016) | (0.0007) |
| Counties | 13,000 | 13,000 | 13,000 | 13,000 | 13,000 |
| **Panel E: Bankruptcy Exemption** | | | | | |
| $Treat_{ct}$ | -0.0322* | 0.0117 | -0.0001 | 0.0043* | 0.0007 |
| | (0.0187) | (0.0101) | (0.0311) | (0.0023) | (0.0009) |
| Counties | 10,000 | 10,000 | 10,000 | 10,000 | 10,000 |

This table shows the effects of large policy increases on startup outcomes at the county level, using counties that border other states. 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 5th 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, as all firms in the sample are age 0), 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 A3: Effect of Large Policy Decreases on Startup Outcomes (Border Counties)

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate |
|---|---|---|---|---|---|
| **Panel A: UI Duration (weeks)** | | | | | |
| $Treat_{ct}$ | -0.0289 | 0.0799*** | 0.0083 | -0.0114* | -0.0015 |
| | (0.0321) | (0.0274) | (0.0864) | (0.0066) | (0.0010) |
| Counties | 6,000 | 6,000 | 6,000 | 6,000 | 6,000 |
| **Panel B: Total Taxes** | | | | | |
| $Treat_{ct}$ | -0.0249*** | -0.0125 | 0.0304 | -0.0032* | -0.0001 |
| | (0.0090) | (0.0158) | (0.0318) | (0.0019) | (0.0004) |
| Counties | 13,500 | 13,500 | 13,500 | 13,500 | 13,500 |
| **Panel C: Total Incentives** | | | | | |
| $Treat_{ct}$ | -0.0172** | 0.0079 | -0.0133 | -0.0040** | 0.0003 |
| | (0.0086) | (0.0099) | (0.0310) | (0.0017) | (0.0003) |
| Counties | 13,000 | 13,000 | 13,000 | 13,000 | 13,000 |

This table shows the effects of large policy decreases on startup outcomes at the county level, using counties that border other states. 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 policy changes. If the 5th percentile was equal to the median (usually zero), the bottom 1% was 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, as all firms in the sample are age 0), 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 A4: Effect of Large Policy Increases on Startup Outcomes (Interior Counties)

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate |
|---|---|---|---|---|---|
| **Panel A: UI Duration (weeks)** | | | | | |
| $Treat_{ct}$ | -0.0312** | -0.0147 | -0.0039 | -0.0014 | 0.0001 |
| | (0.0125) | (0.0126) | (0.0281) | (0.0028) | (0.0005) |
| Counties | 10,500 | 10,500 | 10,500 | 10,500 | 10,500 |
| **Panel B: Total Taxes** | | | | | |
| $Treat_{ct}$ | -0.0056 | -0.0508*** | -0.0561*** | 0.0012 | -0.0004* |
| | (0.0080) | (0.0087) | (0.0195) | (0.0012) | (0.0002) |
| Counties | 22,000 | 22,000 | 22,000 | 22,000 | 22,000 |
| **Panel C: Total Incentives** | | | | | |
| $Treat_{ct}$ | 0.0436*** | 0.0251** | 0.0242 | 0.0025 | -0.0001 |
| | (0.0167) | (0.0099) | (0.0362) | (0.0021) | (0.0003) |
| Counties | 22,000 | 22,000 | 22,000 | 22,000 | 22,000 |
| **Panel D: Minimum Wage (City)** | | | | | |
| $Treat_{ct}$ | -0.0159** | -0.0143 | -0.0151 | -0.0035*** | -0.0004* |
| | (0.0077) | (0.0092) | (0.0260) | (0.0013) | (0.0002) |
| Counties | 23,000 | 23,000 | 23,000 | 23,000 | 23,000 |
| **Panel E: Bankruptcy Exemption** | | | | | |
| $Treat_{ct}$ | 0.0194* | 0.0130 | 0.0454* | 0.0015 | 0.0006* |
| | (0.0106) | (0.0112) | (0.0263) | (0.0015) | (0.0003) |
| Counties | 16,500 | 16,500 | 16,500 | 16,500 | 16,500 |

This table shows the effects of large policy increases on startup outcomes at the county level, using interior counties that do not border other states. 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 5th 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, as all firms in the sample are age 0), 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 A5: Effect of Large Policy Decreases on Startup Outcomes (Interior Counties)

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate |
|---|---|---|---|---|---|
| **Panel A: UI Duration (weeks)** | | | | | |
| $Treat_{ct}$ | -0.0316 | 0.1400* | 0.1600* | -0.0379** | -0.0002 |
| | (0.0229) | (0.0784) | (0.0929) | (0.0154) | (0.0009) |
| Counties | 10,500 | 10,500 | 10,500 | 10,500 | 10,500 |
| **Panel B: Total Taxes** | | | | | |
| $Treat_{ct}$ | -0.0043 | 0.0361 | 0.0637 | -0.0015 | 0.0006** |
| | (0.0071) | (0.0366) | (0.0550) | (0.0064) | (0.0002) |
| Counties | 23,500 | 23,500 | 23,500 | 23,500 | 23,500 |
| **Panel C: Total Incentives** | | | | | |
| $Treat_{ct}$ | -0.0137* | 0.0061 | 0.0084 | -0.0042* | -0.0000 |
| | (0.0078) | (0.0189) | (0.0292) | (0.0024) | (0.0002) |
| Counties | 22,500 | 22,500 | 22,500 | 22,500 | 22,500 |

This table shows the effects of large policy decreases on startup outcomes at the county level, using interior counties that do not border other states. 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 policy changes. If the 5th percentile was equal to the median (usually zero), the bottom 1% was 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, as all firms in the sample are age 0), 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 A6: Effect of Large Policy Increases on Startups with EIN

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate | (6) Move-in Rate |
|---|---|---|---|---|---|---|
| **Panel A: Total Tax** | | | | | | |
| $Treat_{ct}$ | -0.0224** | -0.0414** | -0.0508** | -0.00314** | 0.000123 | -0.0000876 |
| | (0.0108) | (0.0165) | (0.0248) | (0.00157) | (0.000441) | (0.00347) |
| Counties | 34,500 | 34,500 | 34,500 | 34,500 | 34,500 | 34,500 |
| **Panel B: Total Incentives** | | | | | | |
| $Treat_{ct}$ | 0.0803*** | 0.0334*** | 0.0393 | -0.00409 | 0.000213 | -0.00104 |
| | (0.0146) | (0.00971) | (0.0293) | (0.00373) | (0.000289) | (0.000679) |
| Counties | 34,500 | 34,500 | 34,500 | 34,500 | 34,500 | 34,500 |
| **Panel C: Minimum Wage (City)** | | | | | | |
| $Treat_{ct}$ | -0.0205* | 0.0345 | 0.0350 | 0.00107 | -0.00133** | -0.00477* |
| | (0.0108) | (0.0310) | (0.0471) | (0.00160) | (0.000664) | (0.00270) |
| Counties | 57,000 | 57,000 | 57,000 | 57,000 | 57,000 | 57,000 |
| **Panel D: Bankruptcy Exemption** | | | | | | |
| $Treat_{ct}$ | -0.00842 | 0.00771 | 0.00218 | 0.00560*** | 0.000366 | 0.000157 |
| | (0.0138) | (0.00789) | (0.0180) | (0.00165) | (0.000357) | (0.000553) |
| Counties | 40,000 | 40,000 | 40,000 | 40,000 | 40,000 | 40,000 |
| **Panel E: UI Duration (weeks)** | | | | | | |
| $Treat_{ct}$ | -0.0467** | -0.0169* | -0.0161 | -0.00432 | 0.000187 | -0.00178 |
| | (0.0235) | (0.00885) | (0.0194) | (0.00374) | (0.000573) | (0.00119) |
| Counties | 21,500 | 21,500 | 21,500 | 21,500 | 21,500 | 21,500 |

This table shows the effects of large policy increases on 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 5th 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, as all firms in the sample are age 0), 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 A7: Effect of Large Policy Decreases on Startups with EIN

| | (1) ln(Nonemployers) | (2) ln(Employers) | (3) ln(Employment) | (4) Exit Rate | (5) Move-out Rate | (6) Move-in Rate |
|---|---|---|---|---|---|---|
| **Panel A: Total Tax** | | | | | | |
| $Treat_{ct}$ | -0.00718 | 0.0077 | 0.0390** | -0.00114 | -0.000393* | 0.000493 |
| | (0.0128) | (0.00615) | (0.0168) | (0.00138) | (0.000222) | (0.000488) |
| Counties | 36,500 | 36,500 | 36,500 | 36,500 | 36,500 | 36,500 |
| **Panel B: Total Incentives** | | | | | | |
| $Treat_{ct}$ | -0.00825 | 0.00228 | -0.0179 | -0.00296** | -0.00037 | 0.000605 |
| | (0.0101) | (0.00802) | (0.0177) | (0.00147) | (0.000239) | (0.000411) |
| Counties | 35,500 | 35,500 | 35,500 | 35,500 | 35,500 | 35,500 |
| **Panel C: UI Duration (weeks)** | | | | | | |
| $Treat_{ct}$ | 0.00976 | 0.0415*** | 0.0631 | -0.0109*** | -0.000726 | 0.000013 |
| | (0.0240) | (0.00852) | (0.0442) | (0.00263) | (0.000455) | (0.000815) |
| Counties | 21,500 | 21,500 | 21,500 | 21,500 | 21,500 | 21,500 |

This table shows the effects of large policy decreases on startups with EIN 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 policy changes. If the 5th percentile was equal to the median (usually zero), the bottom 1% was 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, as all firms in the sample are age 0), 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 A8: Effect of Large Policy Increases on Startups (Stacked DID)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | ln(Nonemployers) | | ln(Employers) | | ln(Employment) | | Exit Rate | | Move-out Rate | | Move-in Rate | |
| **Panel A: UI Duration (weeks)** | | | | | | | | | | | | |
| *Post Treat* | -0.0090** | -0.0090*** | -0.0049 | -0.0048 | -0.0049 | -0.0046 | 0.0009 | 0.0009 | 0.000070 | 0.000067 | 0.000242** | 0.000245*** |
| | (0.0043) | (0.0034) | (0.0037) | (0.0038) | (0.0051) | (0.0056) | (0.0007) | (0.0008) | (0.000083) | (0.000077) | (0.000097) | (0.000094) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel B: Total Taxes** | | | | | | | | | | | | |
| *Post Treat* | -0.0153 | -0.0139 | -0.0395** | -0.0401** | -0.0385* | -0.0397* | -0.0003 | -0.0001 | -0.000125 | -0.000120 | 0.000606 | 0.000747 |
| | (0.0173) | (0.0175) | (0.0159) | (0.0160) | (0.0197) | (0.0204) | (0.0013) | (0.0013) | (0.000165) | (0.000162) | (0.000503) | (0.000521) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel C: Total Incentives** | | | | | | | | | | | | |
| *Post Treat* | -0.0037 | -0.0102 | 0.0311** | 0.0320** | 0.0343* | 0.0346* | -0.0060*** | -0.0069*** | -0.00048** | -0.000514*** | -0.000135 | -0.000244 |
| | (0.0129) | (0.0144) | (0.0140) | (0.0141) | (0.0197) | (0.0205) | (0.0021) | (0.0021) | (0.000187) | (0.000183) | (0.000630) | (0.000679) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel D: Minimum Wage (City)** | | | | | | | | | | | | |
| *Post Treat* | -0.0368** | -0.0395** | -0.0314 | -0.0280 | -0.0589** | -0.0529** | -0.0019 | -0.0022 | -0.000126 | -0.000136 | -0.000364 | -0.000409 |
| | (0.0177) | (0.0177) | (0.0202) | (0.0201) | (0.0250) | (0.0237) | (0.0026) | (0.0026) | (0.000175) | (0.000177) | (0.000530) | (0.000556) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel E: Bankruptcy Exemption** | | | | | | | | | | | | |
| *Post Treat* | 0.0287 | 0.0259 | -0.0071 | -0.0079 | -0.0029 | 0.0004 | 0.0035 | 0.0033 | 0.000091 | -0.000004 | -0.000192 | -0.000113 |
| | (0.0219) | (0.0221) | (0.0239) | (0.0236) | (0.0271) | (0.0263) | (0.0032) | (0.0032) | (0.000228) | (0.000225) | (0.000511) | (0.000541) |
| Policy Controls | No | Yes | 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 generally refers to the top 5% of the distribution of policy changes. If the 5th 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.

Policy control variables include total taxes, total incentives, and minimum wages, excluding total taxes (incentives) when focusing on tax (incentive) events. In addition, 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, as all firms in the sample are age 0), 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 A9: Effect of Large Policy Decreases on Startups (Stacked DID)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | ln(Nonemployers) | | ln(Employers) | | ln(Employment) | | Exit Rate | | Move-out Rate | | Move-in Rate | |
| **Panel A: UI Duration (weeks)** | | | | | | | | | | | | |
| *Post Treat* | 0.0099 | 0.0056 | 0.0249*** | 0.0250*** | 0.0411*** | 0.0358*** | -0.0072*** | -0.0068*** | -0.00032*** | -0.000319*** | 0.000413* | 0.000256 |
| | (0.0075) | (0.0079) | (0.0055) | (0.0077) | (0.0080) | (0.0120) | (0.0014) | (0.0014) | (0.000070) | (0.000065) | (0.000217) | (0.000217) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel B: Total Taxes** | | | | | | | | | | | | |
| *Post Treat* | -0.0093 | -0.0174 | 0.0117 | 0.0140 | 0.0277 | 0.0299 | -0.0023 | -0.0027 | 0.00024 | 0.00019 | 0.000606 | 0.000853* |
| | (0.0165) | (0.0172) | (0.0235) | (0.0239) | (0.0362) | (0.0370) | (0.0019) | (0.0020) | (0.000177) | (0.000173) | (0.000422) | (0.000437) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel C: Total Incentives** | | | | | | | | | | | | |
| *Post Treat* | -0.0167 | -0.0167 | -0.0038 | -0.0130 | -0.0275 | -0.0416* | -0.0029 | -0.0026 | 0.000072 | 0.000077 | 0.000357 | 0.000653 |
| | (0.0201) | (0.0201) | (0.0216) | (0.0224) | (0.0244) | (0.0255) | (0.0022) | (0.0021) | (0.000231) | (0.000230) | (0.000429) | (0.000459) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |

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

A large decrease generally refers to the bottom 5% of the distribution of policy changes. If the 5th percentile was equal to the median (usually zero), the bottom 1% was 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.

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 A10: Effects of Taxes and Incentives on Startup Outcomes (Firm Level)

| | (1) Transition | (2) ΔEmployment | (3) Exit | (4) Move-Out | (5) Move-In |
|---|---|---|---|---|---|
| **Panel A: Personal Income Tax (State)** | | | | | |
| *Personal Income Tax*$_{ct}$ | 0.00007 | -0.00534 | 0.00060 | 0.00016*** | -0.00030** |
| | (0.00014) | (0.00415) | (0.00083) | (0.00006) | (0.00012) |
| No. of Obs. | 168,700,000 | 268,800,000 | 303,600,000 | 303,400,000 | 242,000,000 |
| **Panel B: Corporate Income Tax (State)** | | | | | |
| *Corporate Income Tax*$_{ct}$ | 0.00002 | -0.00411 | -0.00017 | 0.00006 | -0.00004 |
| | (0.00005) | (0.00437) | (0.00027) | (0.00006) | (0.00008) |
| No. of Obs. | 168,700,000 | 268,800,000 | 303,600,000 | 303,400,000 | 242,000,000 |
| **Panel C: Corporate Income Tax (Upjohn)** | | | | | |
| *Corporate Income Tax*$_{ct}$ | -0.11600 | -1.24100 | 0.25100 | 0.06720 | -0.10800 |
| | (0.08110) | (5.55100) | (0.22300) | (0.05370) | (0.09910) |
| No. of Obs. | 111,700,000 | 176,300,000 | 201,100,000 | 200,900,000 | 151,000,000 |
| **Panel D: Property Tax (Upjohn)** | | | | | |
| *Property Tax*$_{ct}$ | 0.06030 | -0.13000 | 0.13300 | 0.02570 | 0.17400** |
| | (0.06080) | (1.63600) | (0.10300) | (0.01590) | (0.08070) |
| No. of Obs. | 111,700,000 | 176,300,000 | 201,100,000 | 200,900,000 | 151,000,000 |
| **Panel E: Investment Tax Credit (Upjohn)** | | | | | |
| *Investment Tax Credit*$_{ct}$ | -0.07160 | 7.24900 | 0.57400*** | 0.01240 | 0.10600 |
| | (0.05980) | (7.09200) | (0.18500) | (0.04440) | (0.10600) |
| No. of Obs. | 111,700,000 | 176,300,000 | 201,100,000 | 200,900,000 | 151,000,000 |

This table shows the effects of taxes and incentives on startup outcomes at the firm level. The estimates are obtained using a two-way fixed effects model, controlling for firm and year fixed effects.

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

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 A11: Effects of Large Tax Policy Increases on Startups (Stacked DID)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | ln(Nonemployers) | | ln(Employers) | | ln(Employment) | | Exit Rate | | | Move-out Rate |
| **Panel A: Sales Tax (Upjohn)** | | | | | | | | | | |
| *Post Treat* | 0.0307* | 0.0278 | -0.0276** | -0.0277** | -0.0181 | -0.0175 | 0.00326* | 0.00327* | 0.0001 | 0.000097 |
| | (0.0185) | (0.0189) | (0.0117) | (0.0115) | (0.0139) | (0.0148) | (0.00187) | (0.00181) | (0.000188) | (0.000204) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel B: Property Tax (Upjohn)** | | | | | | | | | | |
| *Post Treat* | 0.00841 | 0.00855 | -0.0301* | -0.0356** | -0.0226 | -0.0296 | -0.000568 | 0.000187 | 0.0000898 | 0.000121 |
| | (0.0276) | (0.0269) | (0.018) | (0.0175) | (0.0231) | (0.0226) | (0.00145) | (0.00162) | (0.000202) | (0.000194) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel C: Corporate Income Tax (Upjohn)** | | | | | | | | | | |
| *Post Treat* | -0.0239* | -0.0247* | -0.0217 | -0.0487*** | -0.0338** | -0.0778*** | -0.0016 | 0.000705 | 0.000153 | 0.000167 |
| | (0.0137) | (0.0142) | (0.0135) | (0.0128) | (0.0157) | (0.0185) | (0.00132) | (0.00155) | (0.000193) | (0.000188) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel D: Corporate Tax (State)** | | | | | | | | | | |
| *Post Treat* | 0.0366 | 0.0269 | 0.0897** | 0.0853** | 0.124* | 0.12* | -0.00404** | -0.00429** | -0.0000168 | 0.0000259 |
| | (0.0265) | (0.0263) | (0.0415) | (0.0402) | (0.0656) | (0.0647) | (0.00193) | (0.002) | (0.000286) | (0.000256) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel E: Personal Income Tax (State)** | | | | | | | | | | |
| *Post Treat* | -0.0199* | -0.0431** | 0.00761 | 0.0126 | 0.0271 | 0.044* | -0.00174 | -0.00452** | 0.0000516 | -0.000035 |
| | (0.0109) | (0.0191) | (0.0197) | (0.0224) | (0.0227) | (0.0248) | (0.00149) | (0.0019) | (0.000265) | (0.000262) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |

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 Cengiz et al. (2019).

A large increase generally refers to the top 5% of the distribution of policy changes. If the 5th 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.

Policy control variables include total taxes, total incentives, and minimum wages, excluding total taxes (incentives) when focusing on tax (incentive) events. In addition, 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, as all firms in the sample are age 0), 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 A12: Effects of Large Tax Policy Decreases on Startups (Stacked DID)

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | ln(Nonemployers) | | ln(Employers) | | ln(Employment) | | Exit Rate | | Move-out Rate | |
| **Panel A: Sales Tax (Upjohn)** | | | | | | | | | | |
| *Post Treat* | -0.000736 | 0.00185 | 0.000342 | -0.00707 | -0.0364** | -0.0489** | 0.00069 | 0.00163 | -0.000133 | -0.00016 |
| | (0.0154) | (0.0148) | (0.0165) | (0.0188) | (0.0191) | (0.0246) | (0.00188) | (0.00234) | (0.000202) | (0.000209) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel B: Property Tax (Upjohn)** | | | | | | | | | | |
| *Post Treat* | -0.0084 | -0.013 | 0.0212 | 0.0143 | 0.0288 | 0.0214 | -0.00198 | -0.00171 | 0.000126 | 0.00017 |
| | (0.0148) | (0.0162) | (0.0237) | (0.0248) | (0.038) | (0.0397) | (0.00209) | (0.00191) | (0.000186) | (0.00018) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel C: Corporate Income Tax (Upjohn)** | | | | | | | | | | |
| *Post Treat* | 0.0108 | -0.0105 | 0.0475** | 0.0508** | 0.0549** | 0.0557** | -0.00378** | -0.00454** | 0.00000723 | -0.0000117 |
| | (0.02) | (0.021) | (0.0407) | (0.0373) | (0.0498) | (0.0471) | (0.00178) | (0.00169) | (0.000258) | (0.000247) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel D: Corporate Tax (State)** | | | | | | | | | | |
| *Post Treat* | 0.0138 | -0.0143 | 0.0729** | 0.0742** | 0.0735** | 0.0722** | -0.00236* | -0.00296* | -0.000359 | -0.000338 |
| | (0.0205) | (0.0223) | (0.0359) | (0.0341) | (0.0492) | (0.0476) | (0.00166) | (0.00155) | (0.0002) | (0.000192) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |
| **Panel E: Personal Income Tax (State)** | | | | | | | | | | |
| *Post Treat* | -0.00278 | -0.00354 | -0.0308** | -0.0247** | -0.0346** | -0.0547** | 0.00444 | 0.00545 | 0.000745 | 0.000479 |
| | (0.0111) | (0.0227) | (0.0237) | (0.0281) | (0.0317) | (0.0455) | (0.002) | (0.00276) | (0.000227) | (0.000323) |
| Policy Controls | No | Yes | No | Yes | No | Yes | No | Yes | No | Yes |

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 Cengiz et al. (2019).

A large decrease generally refers to the bottom 5% of the distribution of policy changes. If the 5th percentile was equal to the median (usually zero), the bottom 1% was 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.

Policy control variables include total taxes, total incentives, and minimum wages, excluding total taxes (incentives) when focusing on tax (incentive) events. In addition, 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, as all firms in the sample are age 0), 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).