# Tax Policy and Business Entry*

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**Abstract**

This paper measures the effects of state corporate and personal income tax reforms on business entry using an event study research design. We focus on reforms that do not coincide with federal tax changes, are preceded and followed by stable tax policy, and substantially change tax burdens. Corporate tax reforms cause meaningful changes in business entry: we measure a five-year elasticity of 2.7 with respect to the net-of-tax rate. This is driven primarily by large effects of tax cuts. Corporate tax cuts also reduce the predicted growth potential of entrants. We do not find strong evidence of cross-border spillovers, and find no evidence that personal income tax reforms affect business entry.

*Keywords*: state taxes, taxes, firm entry, young firms, entrepreneurship

*JEL codes*: H24, H25, H32, H71, L26

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# 1 Introduction

Economists have long understood that taxation distorts firms' investment, employment and capital structure decisions. Do taxes also distort prospective entrepreneurs' decisions to start businesses in the first place? If so, do higher taxes screen out entrants with particularly high or low growth potential? These questions are important because young businesses play an outsized role in job creation (Haltiwanger, 2012; Haltiwanger, Jarmin and Miranda, 2013) as well as innovation and growth (Klenow and Li, 2021).

This paper measures the impacts of U.S. state tax reforms on business entry using a stacked event study research design and data covering all new employer firms between 1978 and 2021. Our methodology approximates a set of ideal policy experiments comparing states where large, persistent tax reforms take place to "control" states which do not experience significant tax changes. We analyze both corporate and personal income tax reforms because profits of entrepreneurial businesses may be taxed under either schedule, depending on their organizational form. We show that corporate tax cuts which move state tax rates roughly 2 percentage points on average boost the entry of employer firms by approximately 10 percent five years after they take place. Similarly-sized corporate tax increases reduce firm entry by about 4 percentage points five years out, though estimates are less precise. Together, these effects translate to an elasticity of 2.7 with respect to the combined state-federal net-of-tax rate. Event study estimates for personal income tax reforms are centered around zero. We find no evidence that tax reforms generate entrepreneurial spillovers across states: business entry does not shift when economically connected states change corporate tax rates.

We also measure the effects of corporate tax reforms on entrants' quality. Research emphasizes the skewness of returns to entrepreneurship: most ventures remain small or die quickly, while just a handful become economically consequential (Decker, Haltiwanger, Jarmin and Miranda, 2014). Our findings' policy implications hinge on whether the ventures deterred by tax hikes or spurred by tax cuts create value for stakeholders such as owners or employees, and contribute significantly to economic growth. We address this question using data from Guzman and Stern (2020) who form an "Entrepreneurial Quality Index" by predicting businesses' potential to achieve successful exits based on their ex ante characteristics. Our results suggest that corporate tax cuts decrease average predicted growth probability of new firms by 0.25 standard deviations 5 years after a reform. This result is consistent with models in which lowering taxes on entrepreneurial profits reduces the entrepreneurial ability threshold above which an agent will choose to start a business (Gentry and Hubbard, 2000; Scheuer, 2014).

Our analysis is closely related and complementary to a study by Fairlie, Fossen, Johnston and Lyu (2025) (henceforth FFJL), which uses microdata from the US Census Bureau Comprehensive Startup Panel to study how numerous policy instruments, including taxes, affect business formation. A key difference between FFJL and our study is the measures we use to identify reform events: we use movements in the top state-level statutory tax rates, whereas FFJL use tax and incentive measures constructed by the Upjohn Institute's Panel Database on Incentives and Taxes.[1] Moreover, FFJL separately examine entry of employer and non-employer firms in 33 states from 2001 to 2015 while we focus on employer firms in 48 states from 1978 to 2021. Nonetheless, FFJL find results that qualitatively align with ours when estimating event study regressions around changes in corporate taxes.

Some earlier empirical scholarship on taxation and entrepreneurship focuses on new firm manufacturing establishments (Papke, 1991) and new firm employment (Curtis and Decker, 2018) using Poisson regression and a border counties research design, respectively. Our empirical approach differs in two key ways. First, our research design nonparametrically identifies treatment effects across a broad set of geographic units and industries under a parallel trends assumption whose plausibility we evaluate graphically. Second, we use data that counts the number of firms that enter, rather than establishments or employment of new firms. These latter measures commingle the extensive margin of firm entry with the intensive margin of entrepreneurial investment.

Our study adds to a body of work using Census Bureau data to assess the impacts of specific tax instruments on business entry, including angel investor tax credits (Denes, Howell, Mezzanotti, Wang and Xu, 2023), and unemployment insurance taxes (Guo and Wallskog, 2024). In contrast, we analyze the effects of the primary instruments used to tax business profits. Other studies examine the effects of taxation on business entry in international contexts, using cross-sectional variation in taxes across countries (Djankov, Ganser, McLiesh, Ramalho and Shleifer, 2010) and quasi-experimental methods in European countries (Da Rin, Di Giacomo and Sembenelli, 2011; Venâncio, Barros and Raposo, 2022; Zawisza and Klejdysz, 2024). The United States' outsized role in global high-growth entrepreneurship make our results an important complement to high-quality evidence from other contexts. Our work also complements studies by Cullen and Gordon (2007) and Smith and Miller (2025), who theoretically characterize entrepreneurial tax optimization. We measure two building blocks of their models – comparative statics with respect to the corporate and personal income tax rates – in reduced-form fashion.

[1]See Bartik (2017) for details on how these variables are constructed from industry aggregates.

## 2 Data

We assemble a panel data set covering counties in the contiguous United States, tracking taxes, employer business entry, employment, growth potential of new firms, population density and personal income per capita.

**Sample definitions.** Our analysis sample for business entry and employment covers all counties outside of Hawai'i and Alaska and spans 1978–2021, while our sample for businesses' growth potential covers the same counties and spans 1988–2016.

Table 1: Summary Statistics

| | Mean | Median | SD | Min | Max | Observations |
|---|---|---|---|---|---|---|
| *Panel A. Features of the tax code* | | | | | | |
| State corporate rate | 6.3 | 6.5 | 2.8 | 0 | 12.3 | 129,331 |
| State-federal combined corporate rate | 40.3 | 39.4 | 5.6 | 21.0 | 54.2 | 129,331 |
| State personal rate | 4.6 | 5.1 | 3.0 | 0 | 15.0 | 129,331 |
| State-federal combined personal rate | 43.2 | 42.5 | 7.6 | 28.0 | 74.5 | 129,331 |
| *Panel B. Business Dynamics Statistics outcomes* | | | | | | |
| New firms | 152 | 34 | 557 | 0 | 20,751 | 125,984 |
| Total employment | 32,705 | 5,674 | 124,747 | 0 | 4,143,590 | 129,280 |
| New firm employment | 851 | 157 | 3,399 | 0 | 144,998 | 125,984 |
| Incumbent firm employment | 32,708 | 5,809 | 123,044 | 0 | 4,039,694 | 125,941 |
| *Panel C. Other economic outcomes* | | | | | | |
| Entrepreneurial Quality Index | 0.03 | 0.03 | 0.05 | 0 | 5.46 | 89,369 |
| Population density | 223 | 40 | 1,638 | 0 | 73,736 | 129,331 |
| Pre-tax income per capita | 20,785 | 18,417 | 12,297 | 2,046 | 232,932 | 129,331 |

*Notes:* This table shows the distribution of our key independent and outcome variables across county-year observations. See section 2 for variable sources and descriptions. Tax rates and EQI are expressed in percent terms.

**Taxes.** Our principal tax measures are the top marginal state tax rates on corporate and personal income. We focus on changes in statutory tax rates because they are salient instruments used by policymakers and often the subject of popular debate.[2] Both corporate

[2] An alternative to statutory tax rates is the effective tax rate (ETR) on business capital, a measure of the difference between pre-tax and post-tax returns that incorporates both statutory rates and other provisions of the tax code such as depreciation allowances and investment incentives. Policy debates more often focus on changes to statutory rates than ETRs, however, and as Devereux and Griffith (2003) point out, the relevant ETR for the extensive margin of business creation is not the marginal ETR for an additional unit

and personal income taxes may affect new business formation because firms can be organized either as C corporations, whose profits face corporate taxation, or as pass-through businesses – including S corporations, partnerships, and sole proprietorships – whose profits face personal income taxation at the shareholder level.[3] We draw top state corporate tax rates from Giroud and Rauh (2019) and extend the series forward to 2021 using the Council of State Governments' Book of States publications.[4] We measure the top state-federal combined tax rate on corporate income by integrating these data with the Tax Foundation's federal corporate tax rate series and state tax deductibility information from Suárez Serrato and Zidar (2018).[5] We track top state and state-federal combined personal income tax rates using data from NBER TAXSIM. Table 1 panel A summarizes the distribution of tax rates across county-year observations. Taxes vary widely across space and time. Four states have no corporate taxes throughout our sample period, while eight never tax individual income. The largest state corporate and personal tax rates are 12.3 percent and 15.0 percent, respectively.

**Business entry and employment.** We measure business entry using Business Dynamics Statistics (BDS) tables, which aggregate the US Census Bureau's Longitudinal Business Database (LBD) microdata and cover the universe of private, non-farm business establishments with employees (Haltiwanger, Jarmin and Miranda, 2009).[6] The BDS disaggregates counts of firms and employment by firm age; we measure firm entry, our key outcome, as the number of "age zero" firms. We use county-by-firm age and county-by-industry tables. Panel B of Table 1 shows the distribution of new firms and employment by firm age. The average county-year in our sample sees 152 new employer businesses, which create 851 jobs.

**Growth potential.** We also measure the average predicted growth potential of entrants using data from Guzman and Stern (2020), who link state business registry data to Thomson Reuters SDC financial data and predict "the achievement of an IPO or significant

of capital but the average ETR over the range of a new firm's assets. We are not aware of previous efforts to calculate an average state-level ETR for new firms.

[3]Gordon and MacKie-Mason (1994, 1997) discuss considerations for organizational form choice and conclude that non-tax factors are dominant. Robb and Robinson (2014) show that approximately 28 percent of new firms in the Kauffman Firm Survey are organized as corporations, but do not disaggregate by C or S status and include non-employer firms comprising 60 percent of the sample. To the best of our knowledge, no publicly available data shows the share of *new employer* firms organized as C corporations.

[4]We treat franchise, gross receipts, or other non-standard business taxes as corporate taxes (see Giroud and Rauh (2019) for a discussion).

[5]We use the formula $\tau_{s,t} \equiv \tau_t^{\text{fed}} \cdot (1 - \tau_t^{\text{state}}) + \tau_t^{\text{state}} \times (1 - D_{s,t} \cdot \tau_t^{\text{fed}}) \times \tau_{s,t}^{\text{state}}$ where $\tau_t^{\text{state}}$, $\tau_t^{\text{fed}}$ are statutory state and federal corporate income tax rates in state $s$ at time $t$ and $D_{s,t}$ is an indicator for whether federal income taxes are deductible from the state tax base.

[6]The LBD and BDS exclude nonemployer businesses, which the Census Bureau tracks in the Integrated Longitudinal Business Database. Davis, Haltiwanger, Jarmin, Krizan, Miranda, Nucci and Sandusky (2007) report that only 16.45 percent of age zero firms in the "employer universe" covered by the LBD link to the "nonemployer universe," suggesting that BDS firm entry measures primarily reflect new business entry, rather than transitions from nonemployer to employer status.

acquisition within 6 years of founding" using firms' initial characteristics, such as their legal form of organization, Delaware jurisdiction, and industry. Guzman and Stern (2020) refer to geographic averages of the resulting firm-level predicted growth probabilities as an "Entrepreneurial Quality Index" (EQI). These data cover 1988–2016 and are constructed from the registrations of both employer and non-employer firms. We multiply the EQI measure by 100 to interpret it as the average success probability in percent terms for businesses in a county. Businesses rarely achieve Guzman and Stern (2020) success metrics: Table 1 panel C shows that the average county-year has an EQI of 0.03 percent.

**Control variables.** County population data come from ACS Decennial estimates; between decennial censuses, we interpolate population linearly. County income per capita comes from Gaubert, Kline, Vergara and Yagan (2021).

# 3 Empirical approach and findings

## 3.1 Empirical approach

Our goal is to estimate the causal effect of a persistent shift in the corporate or personal income tax rate on the quantity and composition of business entry. These estimands are appealing because they correspond to standard comparative statics in models of entrepreneurial entry (e.g., Gentry and Hubbard, 2000; Cullen and Gordon, 2007; Scheuer, 2014; Humphries, 2017; Gersbach, Schetter and Schneider, 2019; Catherine, 2022; Smith and Miller, 2025). Ideally, we could randomly assign a jurisdiction a large, persistent tax change, and compare the evolution of business entry there to an identical jurisdiction whose tax system remains under the status quo, holding fixed other features of the policy and economic environment. A sufficiently large tax change would ensure a strong test of the null hypothesis that taxes do not affect entrepreneurship, as inattention and adjustment frictions are likely to attenuate the effects of small tax changes. Imposing a persistent reform, neither preceded by nor followed by other tax changes, would address concerns that (non-)effects of tax reforms are driven by anticipation of future policy movements (Auerbach and Hines, 1988) or adjustment frictions. Holding fixed the tax treatment of control units' entrepreneurial profits would help ensure that changes in their outcomes are a plausible substitute for the unobserved counterfactual changes in the outcomes of the treatment group.

Few state tax reforms resemble the ideal experimental variation described above. Between 1978–2021, state corporate and personal income taxes changed 267 and 612 times, respectively. The overwhelming majority of these tax changes were small: only 63 corporate reforms and 85 personal tax reforms shifted state tax rates by one percentage point or more.

Of those, only 27 corporate and 20 personal reforms had stable tax rates – changing less than one percentage point in every year – in the five years before and after the reform took place. Further filtering yields 25 corporate and 16 personal tax reforms under stable federal tax regimes, i.e., did not immediately follow the Tax Reform Act of 1986 or coincide with opposite-signed federal reforms.[7] Our main specification estimates the effects of this small subset of state tax reforms, for which we show the distribution of reform sizes and dates in panels A and B, respectively, of Figure A.1.

We study these reforms using the stacked event study and difference-in-differences approach of Cengiz, Dube, Lindner and Zipperer (2019). This approach enables us to explicitly construct treatment-control comparison groups comprising states that experienced large, persistent tax reforms which did not coincide with federal reforms, as well as control units which did not experience any significant changes in tax policy in the several years before or after each state reform took place. Concretely, let $e_r$ be the event date associated with a particular tax reform cohort – a set of tax reforms that occur in the same year – indexed by $r$. We define an event-time indicator $D^k_{s(i),r,t} \equiv \text{Treat}_{s(i),r} \times \mathbf{1}\{t = e_r + k\}$ for an event year $k \in \mathcal{K} = \{-5, -4, \ldots, -2, 0, 1, \ldots, 5\}$, as the interaction between an indicator $\text{Treat}_{s(i),r}$ for whether a unit $i$ in state $s(i)$ was treated in reform cohort $r$ and an indicator $\mathbf{1}\{t = e_r + k\}$ for a period $t$ being $k$ periods after treatment. For each reform date $e_r$, we generate a separate eleven-year panel dataset ("stack") comprising only counties which are treated at reform date $e_r$ and "clean control" counties. Clean controls are counties in which no tax reforms shifting state tax rates more than 1 percentage point take place in a eleven-year window around the reform date. We then append these stacks into one dataset and estimate the event study and difference-in-differences specifications:

$$Y_{irt} = \alpha_{ir} + \sum_{k \in \mathcal{K}} \beta_k D^k_{s(i),r,t} + \mathbf{X}'_{irt}\Lambda + \varepsilon_{irt} \tag{1}$$

$$Y_{irt} = \delta_{ir} + \beta \text{Treat}_{s(i),r} \times \mathbf{1}\{t \geq e_r\} + \mathbf{X}'_{irt}\Gamma + \nu_{irt} \tag{2}$$

where $\alpha_{ir}$ and $\delta_{ir}$ are county-by-stack fixed effects, and $\mathbf{X}_{irt}$ is a vector of lagged population density quintile indicators, lagged personal income quintile per capita indicators, and stack-by-year fixed effects. We cluster standard errors at the state level, the unit at which treatment is assigned, and weight all regressions by the number of firms in 1978, the first year BDS data are available.

We also estimate local projection specifications which use a continuous measure of treatment intensity – variation in the log net-of-tax rate – to facilitate comparison with estimates

[7] TRA86 expanded the federal tax base. Many states relied on this definition and lowered tax rates to keep revenue constant, creating an ambiguous effect on state-level tax rates faced by entrepreneurs.

in the literature and test the sensitivity of our conclusions to identifying variation from selected reforms. These specifications take the form

$$Y_{i,t+h} - Y_{i,t-1} = \beta_h \Delta \log(1 - \tau_{s(i),t}) + \mathbf{X}_{it}'\Lambda + \varepsilon_{ith} \tag{3}$$

where $h$ is a horizon in $\{-5, -4, -3, -2, 0, 1, \ldots, 5\}$; $i$ and $t$ index county and year, respectively; $Y_{it}$ is the log of new firms; $\log(1 - \tau_{s(i),t})$ is the log net-of-tax rate, combining the state and federal rates, in state $s(i)$; $\mathbf{X}_{it}$ is a vector of controls including year fixed effects and lags of quintile indicators for population density and per capita personal income. The coefficients $\beta_h$ can be interpreted as elasticities with respect to the net-of-tax rate. To measure elasticities which correspond to tax variation from our selected reforms, we instrument the net-of-tax rate term $\Delta \log(1 - \tau_{s(i),t})$ with indicators for whether one of our positive or negative selected reforms takes place.

## 3.2 Results

**Business entry.** Figure 1 shows event study coefficients estimated from Equation 1, reflecting the evolution of tax rates and business entry before and after our selected reforms. Panel A shows that state tax rates in treated and control units trended together before the reforms took place, at which point reform states experienced large, persistent shifts. On average, corporate tax increases and decreases shifted state tax rates by 2.1 and −2.5 percentage points, while personal tax increases and decreases shifted state rates by 2.3 and −1.7 percentage points. These effects persisted across all five post-periods, and are large relative to the median state corporate and personal income tax rates of 6 and 5 percentage points, respectively, over county-years in our sample.[8]

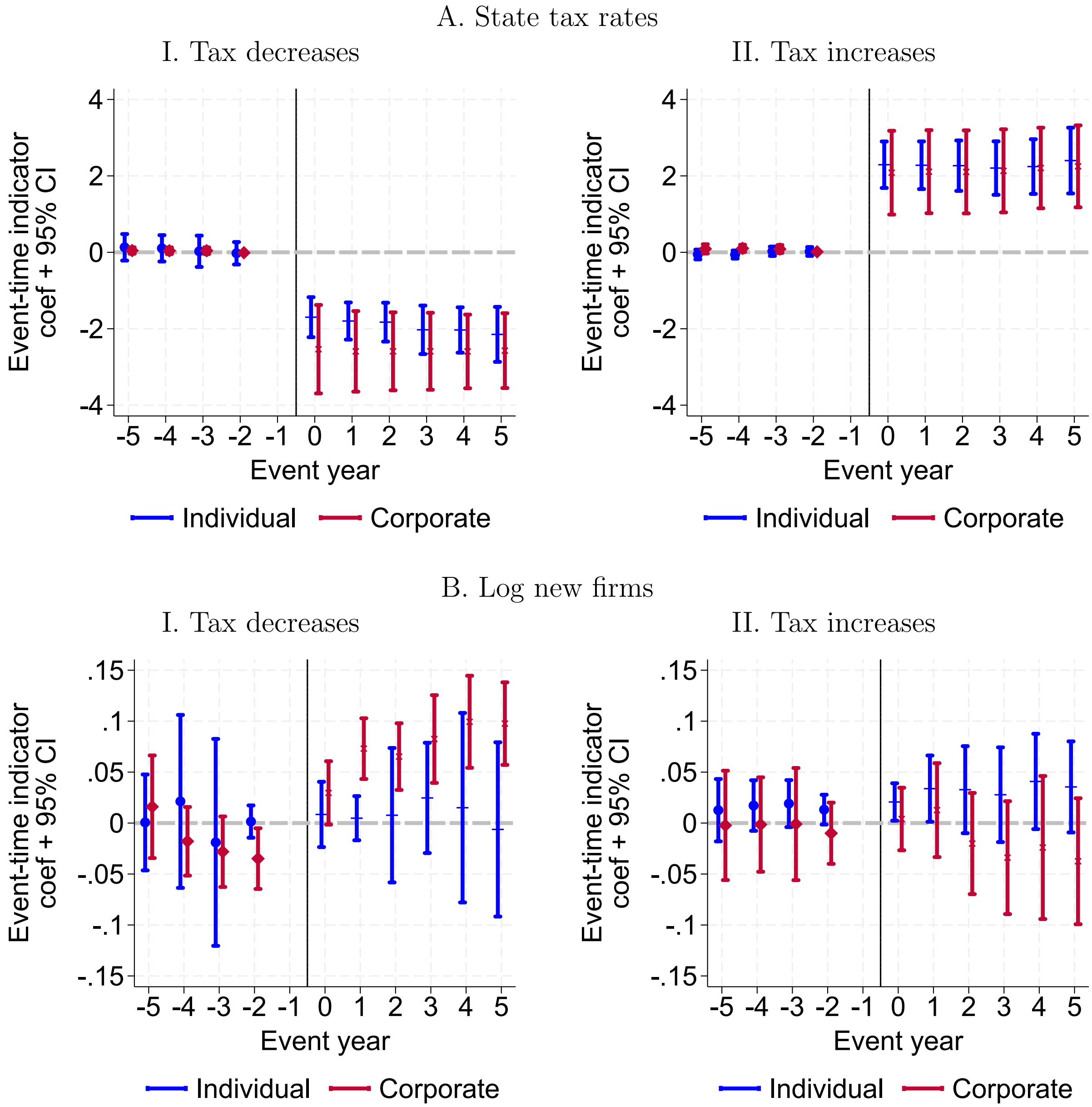
Panel B shows how these tax reforms affected business entry, plotting coefficients from a regression of log new firms. Pre-trends are minimal; most pre-period coefficients are centered around zero, suggesting that business entry in tax reform states trended with business entry in control states before reforms took place. Post-period coefficients show gradual changes following corporate tax reforms, consistent with adjustment frictions or inattention moderating behavioral responses. Five years after corporate tax cuts take place, firm entry increases by roughly 10 percent. Corporate tax hikes decrease firm entry, though we measure a more muted and less precise response: firm entry declines by 4 percent at the five year horizon.[9]

Figure 1 suggests that personal taxes matter less for business entry. Effects of personal

[8]Table A.1 panel A shows the effects of these tax reforms on the combined state and federal log net-of-tax rates. The selected reforms shift log net-of-tax rates by roughly two percent.

[9]Table A.2 shows stacked difference-in-differences estimates of 8.7 percent for corporate tax decreases and -2.0 percent for corporate tax increases.

Figure 1: Effects of state tax reforms on tax rates and business entry



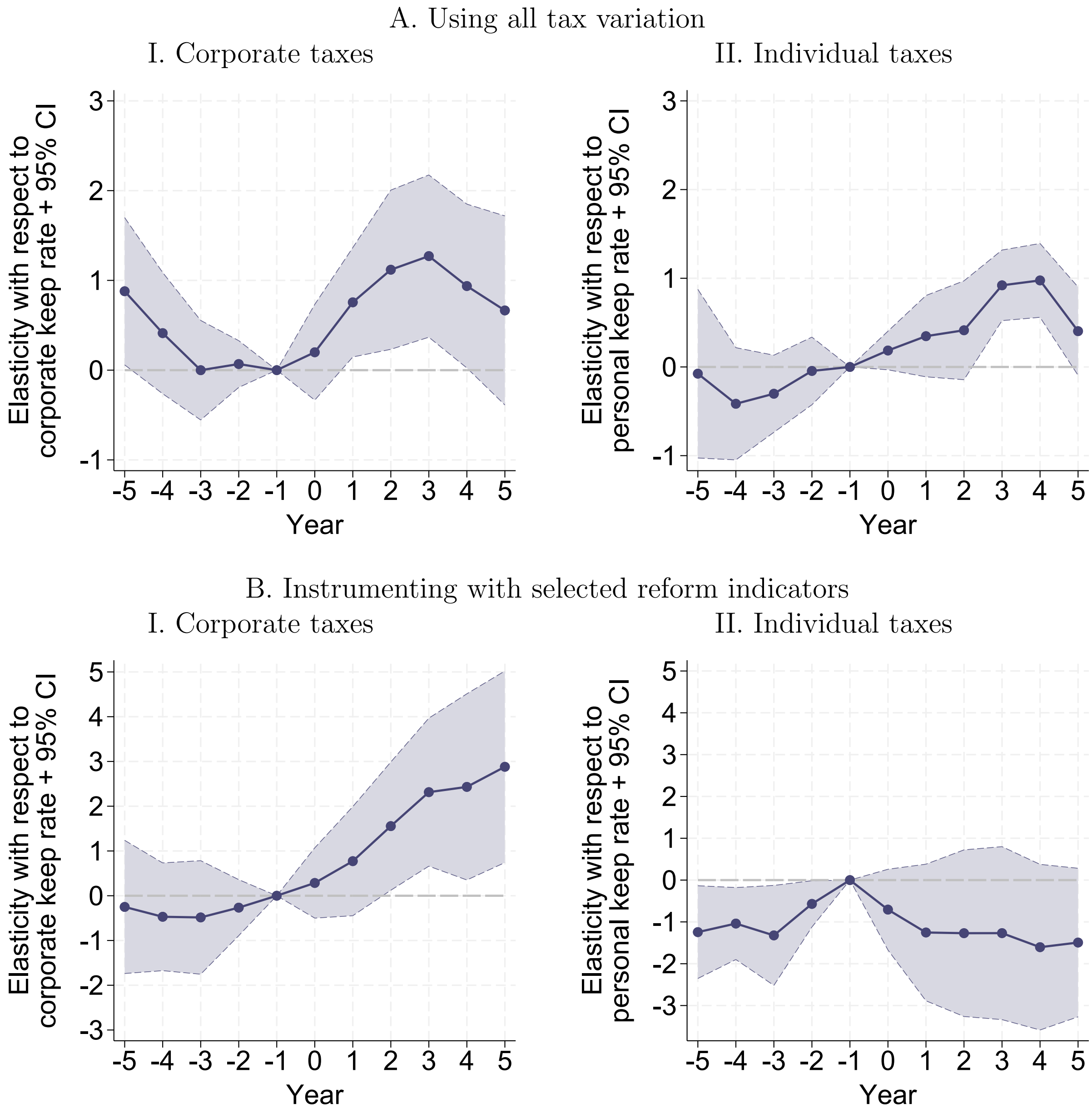

*Notes:* This figure plots estimates of event study coefficients $\beta_k$ from Equation 1 separately for corporate and individual reforms. In Panel A, the tax rate outcome is the same rate used to construct reforms. Standard errors are clustered at the state level and the regression is estimated weighting by each county's 1978 total number of firms.

tax hikes are slightly positive only at some horizons but statistically indistinguishable from zero, while effects of personal tax cuts on new firms are more clearly centered near zero and also not statistically significant. Together, these results suggest that individual taxation does not deter entry of employer firms. One potential explanation for this non-effect is that the top marginal tax rate is not relevant for prospective entrepreneurs who would organize their ventures as pass-through businesses, because entrepreneurs frequently claim net operating losses in their early years and, unlike corporate businesses, have broad latitude to deduct these losses against past taxes paid. Another explanation is that prospective pass-through entrepreneurs may have similar potential earnings in wage employment and entrepreneurship. Because pass-through profits are taxed under the personal income tax schedule, these entrepreneurs would face the same tax rate regardless of whether they enter entrepreneurship, and changes in this tax rate would not affect their decisions. As we are unable to observe the tax returns of entrepreneurs and their businesses, we cannot distinguish between these two stories.

Figure 2 shows local projection coefficient estimates from Equation 3. Panel A shows results using all variation in net-of-tax rates. We estimate positive net-of-tax elasticities for both corporate and personal income taxes at positive horizons, with slightly larger estimates for corporate taxes. However, both plots show pre-trends: effects of corporate taxes are nearly as strong at the negative five-year horizon as they are at the two-year horizon, and effects of personal taxes at the negative four-year horizon are also similar in magnitude to effects two years post-reform. The presence of pre-trends casts doubt on the parallel trends assumption needed to interpret the post-period estimates as causal effects. Panel B shows two-stage least squares estimates extracting variation from our selected reforms. These results are consistent with our main event study findings: they show larger effects of corporate taxes with no apparent pre-trend, with statistically insignificant and slightly negatively-signed estimates for personal taxes. We estimate a business entry elasticity with respect to net-of-corporate tax of 2.7 five years after a reform takes place, and can statistically reject an elasticity less than 0.5.

**Spillovers.** Next, we examine the effects of corporate tax reforms on business entry in economically connected states. This exercise addresses a longstanding argument in the public economics literature that subnational tax reforms may not discourage economic activity but merely shift it across jurisdictional borders (see e.g., Goolsbee and Maydew, 2000; Wilson, 2009; Giroud and Rauh, 2019). It also addresses an econometric concern: spillovers from treated to control states would violate the parallel trends assumption under which the event study coefficients from Equation 1 can be interpreted as causal effects (Baker, Larcker and Wang, 2022).

Figure 2: Elasticities of business entry with respect to net-of-tax rates: local projections estimates




*Notes:* This figure plots estimates of and 95 percent confidence intervals for the coefficient $\beta_h$, for values of $h$ between $-5$ and 5, from Equation 3. The outcome is log new firms. Standard errors are clustered at the state level and the regression is estimated weighting by each county's 1978 total number of firms.

We test for corporate tax reform spillovers by examining firm entry in connected states. Let $\mathcal{C}_s$ be the set of states connected to treated state $s$. We estimate Equation 1 redefining the treatment as an indicator for being connected to a state enacting a selected tax reform. Formally, we have $\text{Treat}^{\text{spillover}}_{s(i),r} = \max_{s' \in \mathcal{C}_{s(i)}} \text{Treat}_{s',r}$. We further exclude from both the treatment and control groups any state which itself or whose connections had unstable tax rates in the eleven-year window around the reform date. For each state $s$, we construct two sets of connected states $\mathcal{C}_s$: one comprising all geographic neighbors and the other consisting of the five states receiving the most outmigrants. Apart from redefining treatment, both specifications are unchanged from our main stacked event study specification.

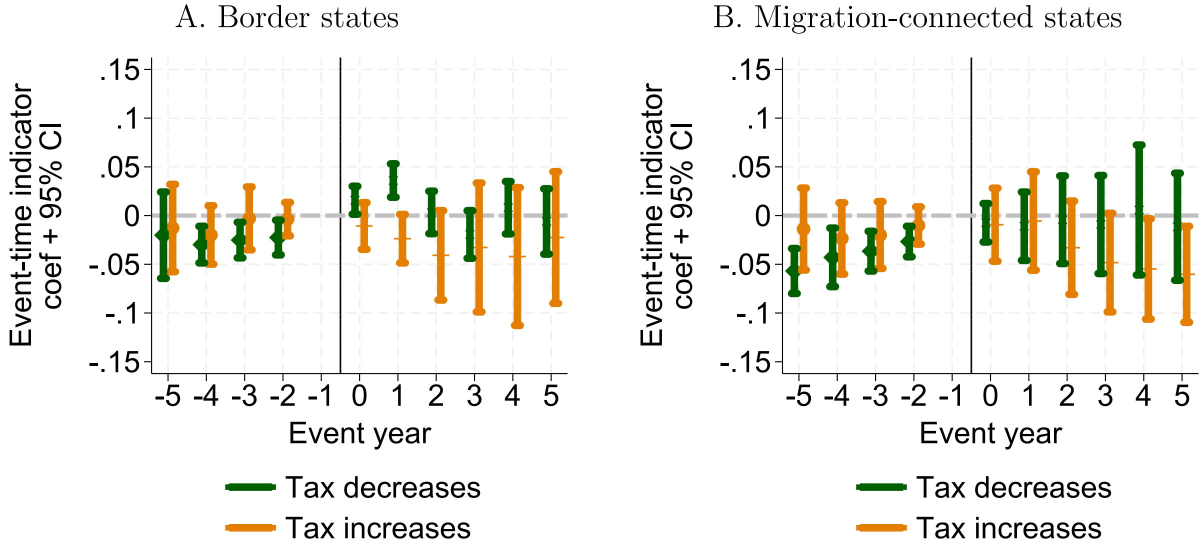
Figure 3 shows that effects of tax decreases on firm entry in both border- and migration-connected states are generally centered around zero. Effects of tax increases on firm entry in both border- and migration-connected states appear slightly negative, which is the opposite of what we would observe if entrepreneurs fled to lower-tax areas. Though imprecise, our estimates are consistent with the idea that corporate taxes do not solely rearrange the spatial distribution of business entry.[10]

**Growth potential.** We measure effects of corporate tax reforms on the quality of entrants using the Guzman and Stern (2020) Entrepreneurial Quality Index. Figure 4 shows estimates of the effect of selected corporate tax reforms on EQI using our main event study approach.[11] The effects of corporate tax decreases are negative, consistent with occupational choice models (e.g., Gentry and Hubbard, 2000; Humphries, 2017) in which higher post-tax returns to entrepreneurship cause agents with lower latent productivity or skill to enter. Corporate tax decreases reduce the average predicted success probability of new firms by 0.012 percentage points, or 0.25 standard deviations, five years after a reform. Effects for corporate tax increases are not statistically significant, and post-period coefficients are generally centered at zero. Together with our firm entry findings, this suggests that state corporate tax cuts play a strong role in shaping entrepreneurial activity, and that tax hikes have smaller effects. Although tax cuts strongly spur firm entry, they have the simultaneous effect of lowering the average quality of these new entrants. The tension between these forces leaves the overall welfare impact of tax cuts on entrepreneurship ambiguous.

[10] An additional concern could be that states shift tax rates in response to one another (Agrawal, Hoyt and Wilson, 2022; DellaVigna and Kim, 2022; de Paula, Rasul and Souza, 2024). Figure A.2 partly assuages this concern by showing that corporate tax rates in migration-connected states do not witness meaningful shifts in corporate tax rates around the reforms we study. However, point estimates of responses to corporate tax increases in neighboring states are positive and economically, though not statistically, significant. Positive effects of neighbors' tax reforms on states' own tax rates could explain why our main event study estimates of the effects of corporate tax hikes are smaller than the effects of corporate tax cuts.

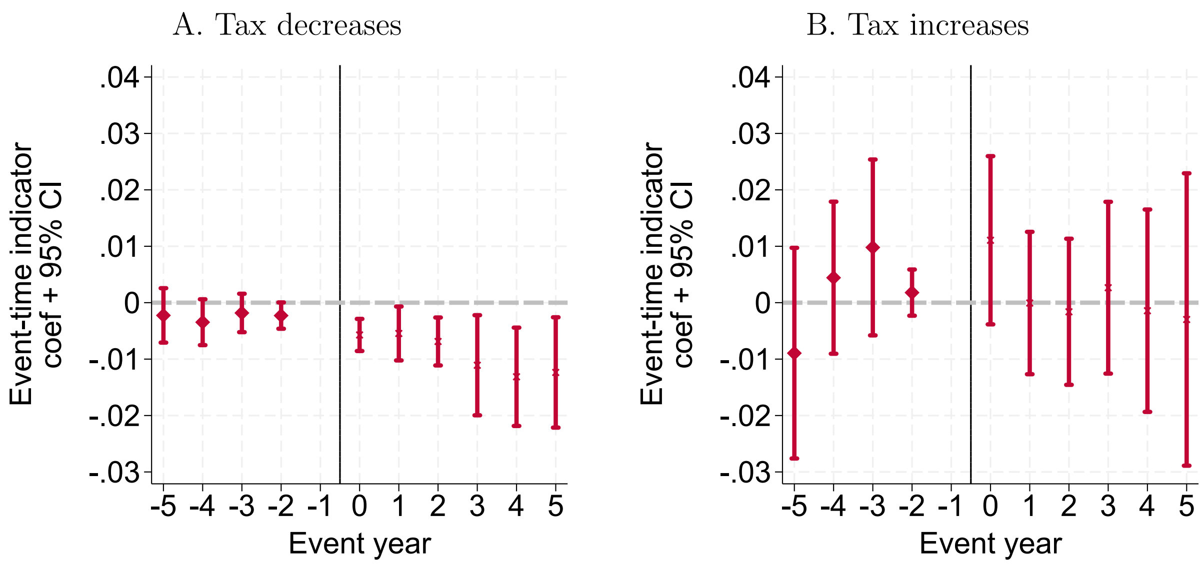
[11] Difference-in-differences estimates for personal tax reforms in the first row of Table A.3 show that personal tax reforms do not significantly affect entrants' quality.

Figure 3: Effects of state corporate tax reforms on business entry in connected states




*Notes:* This figure shows how firm entry evolves around connected states' corporate income tax decreases and increases. The treatment indicator equals one if a state has a connected state (border state or top 5 migration outflow recipient) with a selected reform, and if tax rates are stable in the state and all other non-treated connected states. Event study coefficients are estimated using Equation 1. Standard errors are clustered at the state level and the regression is estimated weighting by each county's 1978 total number of firms.

Figure 4: Effects of state corporate tax reforms on firm growth potential




*Notes:* This figure plots estimates of event study coefficients from Equation 1. The outcome is the Entrepreneurial Quality Index provided by Guzman and Stern (2020) (see section 2). Standard errors are clustered at the state level and the regression is estimated weighting by each county's 1978 total number of firms.

**Employment.** Motivated by perennial interest in questions around effects of taxes on workers (Suárez Serrato and Zidar, 2016; Fuest, Peichl and Siegloch, 2018; Swonder and Vergara, 2024), Table A.1 panel B shows effects of our selected tax reforms on employment. For both new and incumbent firms, employment responds strongly to corporate tax decreases but is not significantly affected by corporate tax hikes or personal tax reforms. Point estimates suggest that new firm employment responds more strongly to corporate tax decreases than incumbent firm employment, though we cannot reject equality of these coefficients.

## 3.3 Robustness and extensions

We conduct multiple tests to assess the consequences of our conservative reform selection criteria and show the results in the online appendix. These include event study estimates around all reforms which shifted state tax rates by at least 0.5 percentage points (Figure A.3), as well as results which impose each component of our reform selection algorithm in isolation (Figure A.4). Figure A.3 suggests that reform selection reduces pronounced pre-trends around corporate tax decreases and personal tax increases, reinforcing the parallel trends assumption we rely on for identification. Both figures confirm the intuition outlined in subsection 3.1 that focusing on reforms resembling ideal experiments strengthens the effects we measure. Differences between our preferred and alternative estimates are likely driven by adjustment frictions, anticipation effects, effect size non-linearities, and reform instability.

Table A.2 and Table A.3 show that our firm entry and quality results are robust to a host of additional tests. Despite correlation between changes in corporate rates and changes in individual rates (Table A.1), adding the counterpart tax rate as a control does not affect our results. The results are broadly robust to controlling for other features of the tax code and policy parameters.[12] Our effects are somewhat attenuated when restricting the sample by excluding states with gross receipts taxes and when excluding reforms that coincided with national recessions. Imposing Romer and Romer (2010) narrative exogeneity into our reform selection does not meaningfully change the effects we measure for corporate tax cuts. Finally, our results are robust to alternative weighting, fixed effects, and the use of state-level (rather than county-level) data, or restricting to 1988–2016 (years for which EQI data are available). Table A.4 confirms our findings hold at the sector level. Corporate tax cuts have large and significant impacts. We cannot generally reject equality in effect sizes across sectors. Personal tax reforms continue to have little effect on business entry, with the exception of one professional and business services subsector.

[12] These include apportionment rules, unemployment insurance taxes (Guo and Wallskog, 2024), sales tax rates, financial assistance measures, investment and job creation incentives, net operating loss provisions, investment and R&D tax credits, and county-level tax measures from Baker, Janas and Kueng (2025).

## 3.4 Discussion

Despite data and methodological differences, our work is compatible with evidence from similar studies. Like Fairlie, Fossen, Johnston and Lyu (2025), we find that corporate taxation influences employer firm entry, despite only 8 out of 25 corporate reforms occurring between 2001 and 2015, the period of overlap between our two studies. Similarly, Papke (1987) and Curtis and Decker (2018) find that increasing tax rates reduces new manufacturing establishments and that corporate taxes have negative impacts on new firm employment, respectively. Our results suggesting that corporate tax rates affect the growth potential composition of entrants are consistent with Fazio, Guzman and Stern (2020) who find that R&D tax credits impact business quality.

Results reported in Suárez Serrato and Zidar (2016) and Giroud and Rauh (2019) (henceforth SSZ and GR, respectively) help contextualize our corporate tax effects on business entry. GR report a ten-year establishment elasticity of 1.2, while SSZ's results imply a ten-year establishment elasticity with respect to the statutory corporate net-of-tax rate of approximately 2.04.[13] GR measure effects on incumbent multi-unit firm establishments only, while SSZ estimates cover establishments of both new and incumbent firms. If all new firms were single establishments, the SSZ elasticity would approximately equal the average of the GR elasticity and the elasticity of firm entry we measure, weighting by the share of new establishments belonging to incumbent and new firms, respectively.[14] We calculate that the average new firm share of establishment entry from 1978 to 2021 is 0.68. Using this weight to average our five-year elasticity estimate of 2.7 in Figure 2 with the GR elasticity of 1.2 arrives at an establishment elasticity of 2.22, similar to the elasticity of 2.04 implied by SSZ.

Our finding that personal tax changes do not affect business entry is also consistent with Curtis and Decker (2018), as well as Zidar (2019) who shows that changes in the top personal income tax rate do not generate more economic activity. Similarly, Fairlie, Fossen, Johnston and Lyu (2025) find no role for tax increases in deterring employer firm entry, though they find a modest positive effect of individual tax cuts. These findings can be reconciled with ours by eliminating our reform selection algorithm: our local projections estimates using all tax variation also report a positive effect of individual taxation on employer firm entry.

[13]SSZ report an elasticity of 4.07 with respect to the *apportioned* corporate tax rate, and report in Table 1 that apportionment weights on payroll, property, and sales are 22.7, 22.8, and 54.5 percent, respectively, in their sample. Assuming conservatively that, on average, businesses have 10 percent of their sales in the same state as their payroll and property, we divide SSZ's estimate of 4.07 by $1/(0.227 + 0.228 + (0.545 \times 0.1)) \approx 2$ to arrive at an elasticity of 2.04. We thank Owen Zidar for drawing this to our attention.

[14]This will not hold exactly for at least two other reasons. First, GR's elasticity is measured for *multi-state* incumbent firms, which might differ from incumbent firms operating in only a single state. Second, GR and SSZ's key outcomes are stocks of establishments, meaning that they measure establishment entry effects net of exit, whereas we measure effects on gross firm entry.

# 4 Conclusion

This paper estimates the effects of state corporate and personal income tax reforms on business entry and growth potential of entrants. We obtain these estimates from event study regressions around reforms identified by a novel algorithm identifying changes to top marginal tax rates which are large, persistent, and whose effects are unconfounded by large, potentially countervailing shifts in the federal tax environment. Our key finding is that business entry responds to corporate tax changes but appears unaffected by personal income tax changes. We find little evidence that effects of corporate tax reforms reflect spatial reallocation of entrepreneurship, suggesting that higher corporate taxes impose meaningful burdens on would-be entrepreneurs. However, lower corporate taxes induce business entry among firms with lower predicted growth potential.

Our study therefore has mixed implications for policy debates. Our corporate tax reform findings suggest that policymakers may spur business and job creation by cutting corporate taxes, but understanding whether this is desirable depends in part on whether they positively affect other agents, for instance, through innovation or job creation (Haltiwanger, Jarmin and Miranda, 2013; Scheuer, 2014; Klenow and Li, 2021; Kleven, 2025). Our finding that tax cuts reduce entrants' average predicted growth potential speaks to this question, suggesting that the marginal firms induced to enter are not "star entrepreneurs" who leave important footprints on the economy. Bell, Chetty, Jaravel, Petkova and Van Reenen (2019) make a related observation in an analysis of prospective inventors' career choices, showing that cutting top income tax rates is theoretically unlikely to increase aggregate quality-weighted innovation. We leave future work to weigh the normative implications of our findings.

In addition, we hope to spur research on an often overlooked "extensive margin" of business investment. While a large literature discusses how taxes affect businesses, empirical studies of major US tax reforms typically restrict attention to incumbent firms.[15] Our findings suggest that the exclusion of business entry from these analyses may be an economically significant oversight. Ignoring new firms may lead scholars to miss an important margin by which corporate taxes affect business investment.

Recent work highlights numerous factors influencing entrepreneurship, from cognitive skills (Levine and Rubinstein, 2017) and age (Azoulay, Jones, Kim and Miranda, 2020) to financial asset returns (Chodorow-Reich, Nenov, Santos and Simsek, 2023) and social

[15]For example, Kennedy, Dobridge, Landefeld and Mortenson (2022) restrict their sample to firms with at least 50 employees and $1 million in sales in the three years before the 2017 tax cuts; Chodorow-Reich, Smith, Zidar and Zwick (2024) drop firms with insufficient history to permit measurement of tax policy shock variables around 2017; and Giroud and Rauh (2019) consider the establishment location decisions of multi-state firms.

insurance design (Hombert, Schoar, Sraer and Thesmar, 2020). Collectively, these findings paint a nuanced landscape in which many factors determine who becomes an entrepreneur and how their ventures fare. Our work provides evidence that corporate income taxation also plays a meaningful role in shaping this entrepreneurial environment.

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# A Online Appendix

Table A.1: Effects of state tax reforms on other outcomes: stacked difference-in-differences

| | Corporate Tax Decrease (1) | Corporate Tax Increase (2) | Individual Tax Decrease (3) | Individual Tax Increase (4) |
|---|---|---|---|---|
| *Panel A. Tax measures* | | | | |
| State tax rate | -2.761*** | 2.199*** | -1.969*** | 1.909*** |
| | (0.484) | (0.604) | (0.269) | (0.490) |
| Other tax rate | -0.093 | 0.012 | -0.945 | 0.435 |
| | (0.239) | (0.285) | (1.079) | (0.293) |
| Log net-of-tax rate | 0.019 | -0.022*** | 0.019*** | -0.020*** |
| | (0.013) | (0.006) | (0.003) | (0.005) |
| *Panel B. Employment outcomes* | | | | |
| Log new firm employment | 0.099*** | -0.014 | -0.001 | -0.023 |
| | (0.028) | (0.051) | (0.021) | (0.025) |
| Log incumbent firm employment | 0.067*** | -0.017 | 0.013 | -0.028 |
| | (0.012) | (0.025) | (0.011) | (0.018) |
| Log total employment | 0.066*** | -0.017 | 0.013 | -0.027 |
| | (0.012) | (0.025) | (0.011) | (0.018) |

*Notes:* This table shows stacked differences-in-differences (Equation 2) estimates of the effects of corporate and personal income tax reforms on additional outcomes. Observations are at the county level. See section 2 for data sources and subsection 3.3 for a discussion of findings. Other tax rate is individual tax for corporate reforms, and vice versa. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.2: Stacked difference-in-differences firm entry robustness

| | Corporate Tax Decrease (1) | Corporate Tax Increase (2) | Individual Tax Decrease (3) | Individual Tax Increase (4) |
|---|---|---|---|---|
| Main specification | 0.087$^{***}$ | -0.020 | 0.008 | 0.001 |
| | (0.032) | (0.046) | (0.018) | (0.024) |
| | *7 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| Controlling for other (personal or | 0.087$^{***}$ | -0.020 | 0.005 | 0.003 |
| corporate) tax rate | (0.031) | (0.045) | (0.024) | (0.024) |
| | *7 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| Controlling for apportionment | 0.071$^{**}$ | -0.005 | 0.011 | 0.011 |
| weights | (0.030) | (0.051) | (0.042) | (0.026) |
| | *5 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| Controlling for UI tax rates and base | 0.070$^{**}$ | -0.018 | 0.008 | 0.003 |
| | (0.029) | (0.043) | (0.034) | (0.021) |
| | *7 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| Controlling for comprehensive set | 0.080$^{***}$ | -0.011 | 0.031 | -0.023 |
| of state tax code features | (0.018) | (0.041) | (0.055) | (0.016) |
| | *5 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| Controlling for Baker, Janas and Kueng (2025) | 0.127$^{***}$ | -0.034$^{*}$ | -0.001 | 0.014 |
| county-level tax measures | (0.013) | (0.019) | (0.030) | (0.026) |
| | *5 reforms* | *5 reforms* | *4 reforms* | *7 reforms* |
| Excluding gross receipt tax states | 0.019 | -0.070$^{*}$ | 0.014 | 0.003 |
| | (0.017) | (0.042) | (0.018) | (0.024) |
| | *6 reforms* | *17 reforms* | *4 reforms* | *11 reforms* |
| Adding Giroud and Rauh (2019) exogeneity | 0.078$^{***}$ | -0.047 | 0.010 | -0.032 |
| on top of main restrictions | (0.013) | (0.036) | (0.014) | (0.031) |
| | *1 reform* | *10 reforms* | *1 reform* | *5 reforms* |
| Excluding recession-year tax reforms | 0.032 | 0.001 | -0.004 | 0.036$^{**}$ |
| | (0.024) | (0.054) | (0.021) | (0.015) |
| | *5 reforms* | *13 reforms* | *3 reforms* | *5 reforms* |
| No weighting | 0.120$^{**}$ | -0.045 | 0.012 | -0.019 |
| | (0.050) | (0.044) | (0.012) | (0.026) |
| | *7 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| Income quintile-by-year fixed effects | 0.089$^{***}$ | -0.009 | 0.018 | -0.006 |
| | (0.031) | (0.041) | (0.019) | (0.023) |
| | *7 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| No controls | 0.094$^{***}$ | -0.027 | 0.015 | 0.003 |
| | (0.032) | (0.047) | (0.022) | (0.025) |
| | *7 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| Estimated at state level | 0.104$^{***}$ | -0.022 | 0.032 | -0.026 |
| | (0.038) | (0.046) | (0.025) | (0.022) |
| | *7 reforms* | *18 reforms* | *4 reforms* | *12 reforms* |
| Only 1988–2016 data | 0.108$^{***}$ | 0.005 | -0.004 | 0.007 |
| | (0.021) | (0.043) | (0.022) | (0.028) |
| | *7 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |

*Notes:* This table shows stacked differences-in-differences (Equation 2) estimates of the effects of corporate and personal income tax reforms on business entry in alternative specifications. The comprehensive set of state tax code features includes apportionment weights and throwback rules; an indicator for whether state tax throwback rules are "throwouts;" the sales tax rate; the unemployment insurance tax rates and base wages; state and local property tax rate indexes; eighteen measures of whether states and localities offer financial assistance; fifteen measures of whether states offer investment or job creation incentives; net operating loss carryforward and carryback windows; indicators for depreciation regimes, including bonus depreciation; the R&D tax credit; and the investment tax credit. These measures come from Suárez Serrato and Zidar (2018) and Giroud and Rauh (2019). For state-level specifications, we take state population and real GDP per capita data from Federal Reserve Economic Data (FRED) and Agersnap and Zidar (2021), respectively. See section 2 for additional data sources. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.3: Stacked difference-in-differences Entrepreneurial Quality Index robustness

| | Corporate Tax Decrease (1) | Corporate Tax Increase (2) | Individual Tax Decrease (3) | Individual Tax Increase (4) |
|---|---|---|---|---|
| Main specification | -0.007** | 0.004 | 0.004*** | -0.005 |
| | (0.003) | (0.014) | (0.001) | (0.004) |
| | *7 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |
| Controlling for other (personal or corporate) tax rate | -0.006*** | 0.004 | 0.004** | -0.007 |
| | (0.002) | (0.013) | (0.002) | (0.004) |
| | *7 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |
| Controlling for apportionment weights | -0.006* | 0.007 | 0.006*** | -0.006 |
| | (0.003) | (0.015) | (0.002) | (0.004) |
| | *5 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |
| Controlling for UI tax rates and base | -0.008** | 0.004 | 0.003 | -0.006 |
| | (0.003) | (0.014) | (0.002) | (0.005) |
| | *7 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |
| Controlling for comprehensive set of state tax code features | -0.008** | 0.004 | 0.003 | -0.002 |
| | (0.003) | (0.014) | (0.002) | (0.004) |
| | *7 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |
| Controlling for Baker, Janas and Kueng (2025) county-level tax measures | -0.007** | -0.004 | 0.002 | 0.005* |
| | (0.004) | (0.004) | (0.003) | (0.003) |
| | *5 reforms* | *5 reforms* | *4 reforms* | *7 reforms* |
| Excluding gross receipt tax states | -0.011* | 0.020 | 0.004 | -0.004 |
| | (0.006) | (0.020) | (0.002) | (0.004) |
| | *6 reforms* | *11 reforms* | *4 reforms* | *9 reforms* |
| Adding Giroud and Rauh (2019) exogeneity on top of main restrictions | 0.005* | 0.010 | 0.005* | 0.002 |
| | (0.003) | (0.024) | (0.003) | (0.002) |
| | *1 reform* | *6 reforms* | *1 reform* | *3 reforms* |
| Excluding recession-year tax reforms | -0.004 | -0.011** | 0.004** | -0.009 |
| | (0.004) | (0.005) | (0.002) | (0.006) |
| | *5 reforms* | *8 reforms* | *3 reforms* | *3 reforms* |
| No weighting | -0.003*** | 0.005 | 0.001 | 0.003** |
| | (0.001) | (0.007) | (0.001) | (0.001) |
| | *7 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |
| Income quintile-by-year fixed effects | -0.007** | 0.005 | 0.003*** | -0.005 |
| | (0.003) | (0.013) | (0.001) | (0.004) |
| | *7 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |
| No controls | -0.007** | 0.004 | 0.003** | -0.006 |
| | (0.003) | (0.014) | (0.001) | (0.004) |
| | *7 reforms* | *12 reforms* | *4 reforms* | *9 reforms* |

*Notes:* This table shows stacked differences-in-differences (Equation 2) estimates of the effects of corporate and personal income tax reforms on the Guzman and Stern (2020) entrepreneurial quality index in alternative specifications. See Table A.2 notes for details. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A.4: Industry heterogeneity in stacked difference-in-differences firm entry effects

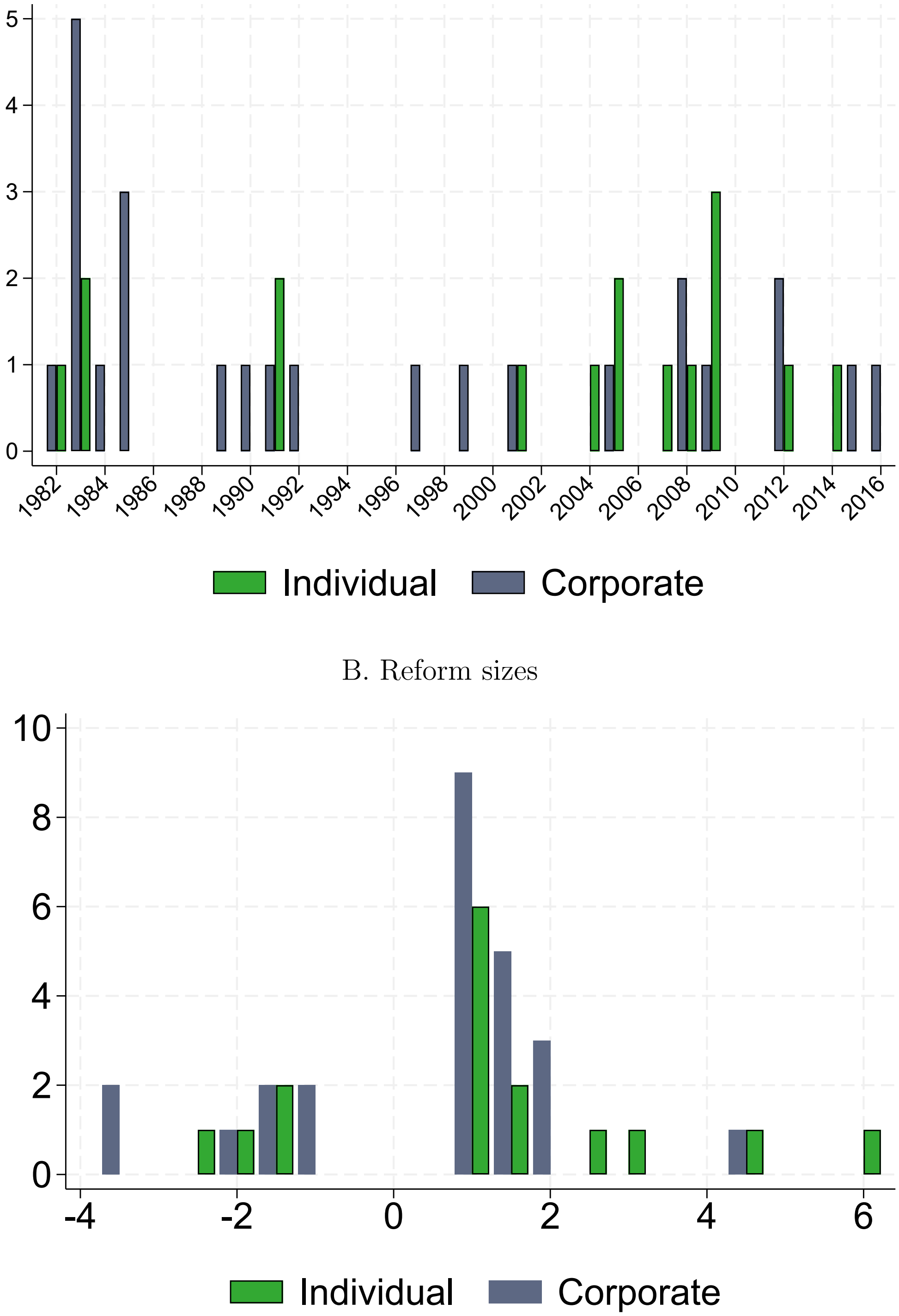
| | Corporate Tax Decrease (1) | Corporate Tax Increase (2) | Individual Tax Decrease (3) | Individual Tax Increase (4) |
|---|---|---|---|---|
| Construction | 0.220*** | 0.035 | 0.115** | -0.025 |
| (NAICS 23) | (0.080) | (0.122) | (0.052) | (0.078) |
| Manufacturing | 0.045** | 0.059 | -0.089 | 0.019 |
| (NAICS 31-33) | (0.020) | (0.052) | (0.108) | (0.032) |
| Wholesale Trade | 0.201*** | -0.042 | -0.073** | 0.031 |
| (NAICS 42) | (0.036) | (0.041) | (0.032) | (0.020) |
| Retail Trade | 0.043 | 0.025 | -0.026 | 0.002 |
| (NAICS 44-45) | (0.034) | (0.057) | (0.039) | (0.020) |
| Transport & Warehousing | 0.134*** | 0.012 | 0.096** | 0.033 |
| (NAICS 48-49) | (0.033) | (0.089) | (0.037) | (0.049) |
| Information | -0.006 | 0.073 | 0.067 | 0.089** |
| (NAICS 51) | (0.052) | (0.053) | (0.168) | (0.040) |
| Finance and Insurance | 0.178*** | -0.007 | -0.056 | -0.001 |
| (NAICS 52) | (0.058) | (0.054) | (0.061) | (0.049) |
| Real Estate, Rental & Leasing | 0.155*** | 0.022 | 0.028 | -0.014 |
| (NAICS 53) | (0.050) | (0.064) | (0.040) | (0.049) |
| Prof, Sci, & Tech Svcs | 0.102*** | -0.020 | 0.041** | 0.021 |
| (NAICS 54) | (0.033) | (0.042) | (0.019) | (0.026) |
| Admin & Support & Wst Mgt | 0.043** | 0.004 | 0.050*** | -0.096*** |
| (NAICS 56) | (0.020) | (0.047) | (0.019) | (0.028) |
| Educational Services | -0.039 | -0.094** | 0.051 | -0.000 |
| (NAICS 61) | (0.039) | (0.046) | (0.055) | (0.031) |
| Health Care | 0.040*** | 0.026 | -0.118*** | -0.022 |
| (NAICS 62) | (0.013) | (0.037) | (0.044) | (0.021) |
| Arts, Ent, & Recreation | 0.067*** | 0.033 | -0.013 | 0.013 |
| (NAICS 71) | (0.025) | (0.035) | (0.024) | (0.034) |
| Accommodation & Food Svcs | 0.073*** | -0.020 | -0.002 | 0.012 |
| (NAICS 72) | (0.023) | (0.035) | (0.036) | (0.018) |
| Other Services | 0.013 | 0.007 | -0.024 | 0.007 |
| (NAICS 81) | (0.030) | (0.050) | (0.036) | (0.018) |

*Notes:* This table shows stacked differences-in-differences (Equation 2) estimates of the effects of corporate and personal income tax reforms on business entry across industries. Observations are at the county level. The BDS does not publish county-by-industry-by-firm age tables. We impute counts of new firms by industry by taking new firms$_t$ = total firms$_t$ − total firms$_{t-1}$ + firm deaths$_t$. We verify that, when aggregated to the county or state by industry levels, this measure matches the variation in true aggregates extremely well (correlation coefficient>0.99). We set values to missing if the imputation gives a negative estimate, and drop 4 industries where the imputed measure and the true aggregate are persistently off in levels. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure A.1: Selected state tax reforms for stacked event study regressions

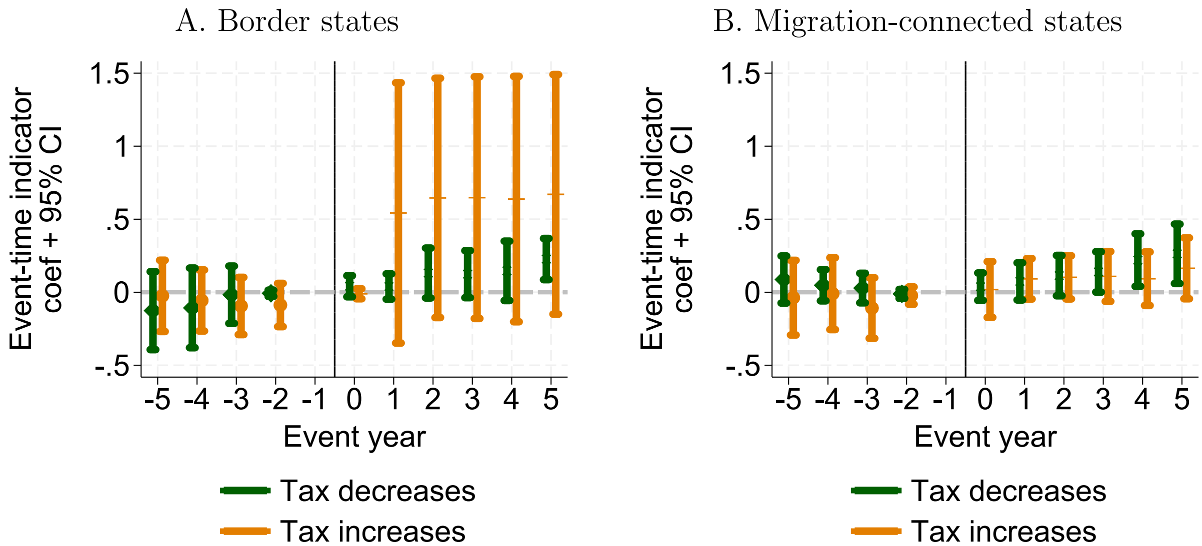
A. Reform dates

B. Reform sizes



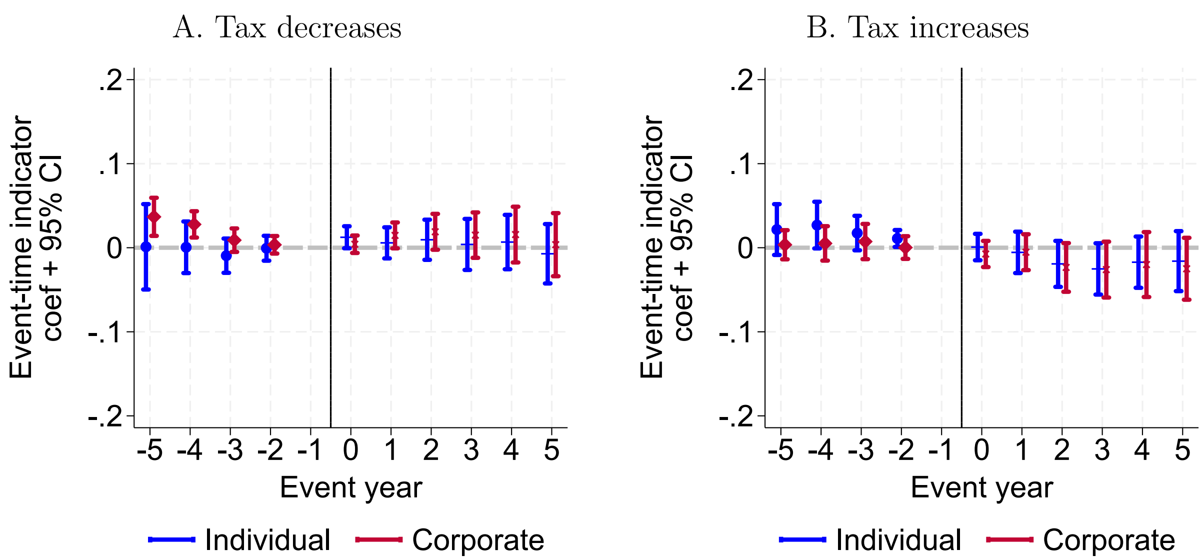

*Notes*: This figure shows the distribution of reform dates and sizes (changes in state tax rates) for state tax reforms which shifted state tax rates by at least one percentage point; had stable tax rates in the symmetric ten-year window around the reform year; and did not coincide with large opposite-signed federal tax reforms, as discussed in section 3.

Figure A.2: Effects of state corporate tax reforms on state corporate tax rates in connected states




*Notes:* This figure shows how state corporate tax rates evolve around connected states' corporate income tax decreases and increases. The treatment indicator equals one if a state has a connected state (border state or top 5 migration outflow recipient) with a selected reform, and if tax rates are stable in the state and all other non-treated connected states.

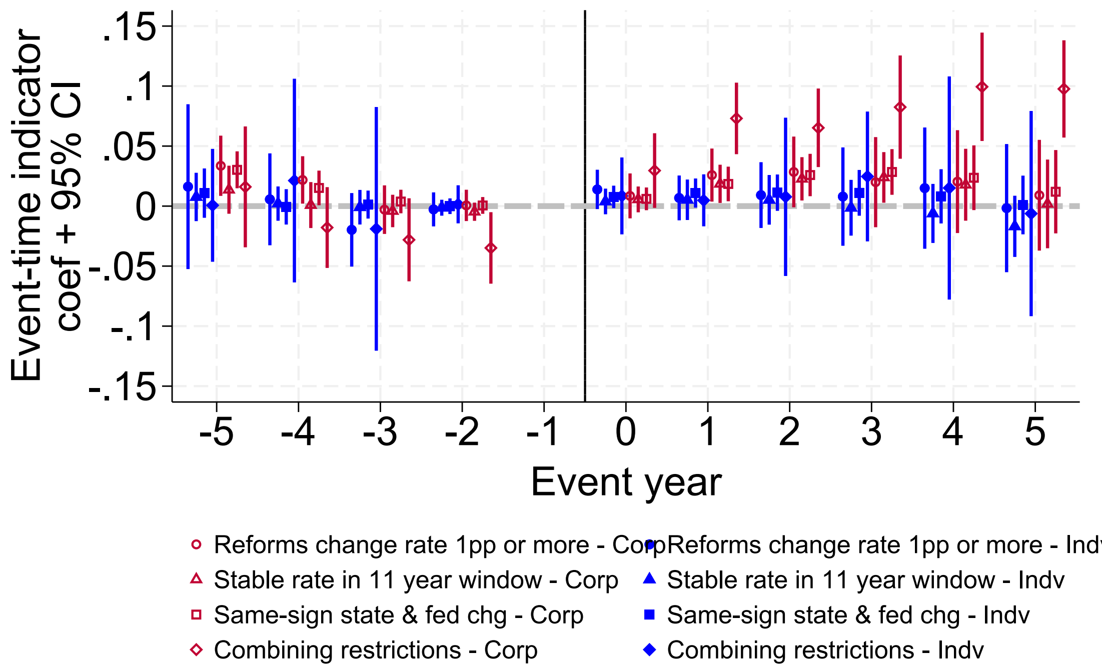
Figure A.3: Effects of all state tax reforms shifting rates by more than 0.5 percentage points



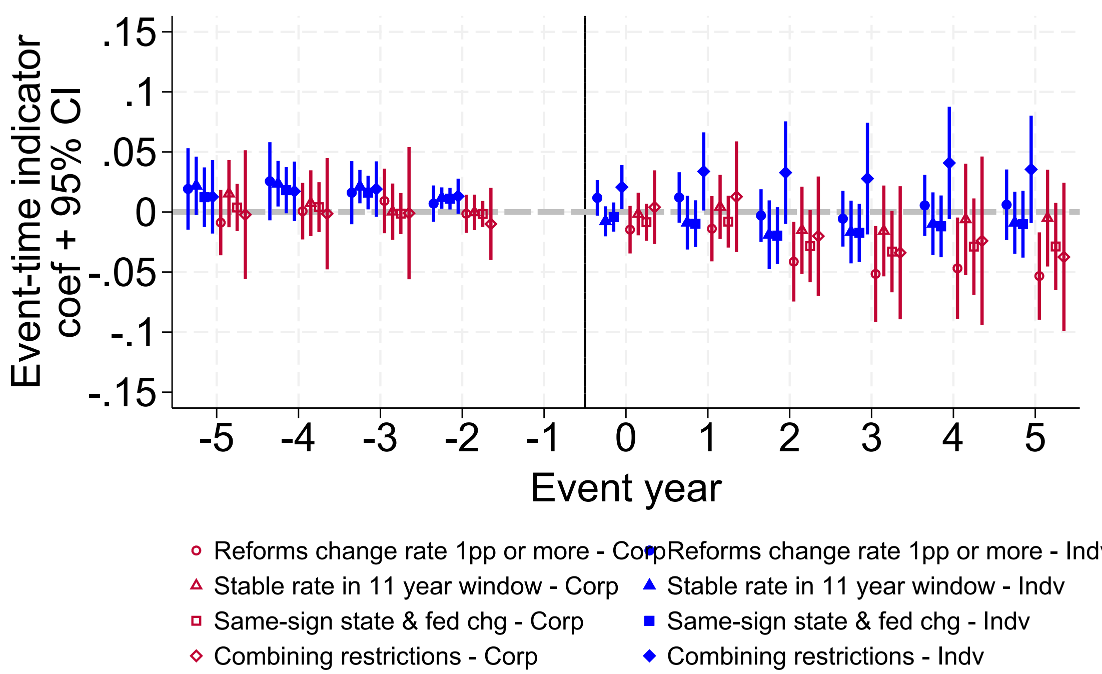

*Notes:* This figure plots estimates of event study coefficients $\beta_k$ for event-time from $-5$ to 5. The outcome is log new firms. The regression equation is Equation 1 in the text. Event-time indicators are defined around all tax reforms shifting tax rates greater than 0.5 percentage points, and control states are any states not experiencing 0.5pp state tax rate changes in the eleven-year window around the reform date. Standard errors are clustered at the state level and the regression is estimated weighting by each county's 1978 total number of firms.

Figure A.4: Stacked event study robustness: components of reform selection algorithm

A. Tax decreases




B. Tax increases




*Notes:* This figure shows how each individual component of our reform selection algorithm, discussed in section 3, affects our event study estimates for firm entry. The outcome is log new firms. "Reforms change rate 1pp or more" estimates event study results around all state corporate or personal income tax reforms which shifted state rates by at least one percentage point. "Stable rate in 10 year window" imposes that state tax rates did not change by one percentage point or more in the five years preceding or following the reform year. "Same-sign state & fed chg" impose the restriction that reforms resulted in same-signed changes to the combined state-federal tax rate and that tax changes did not immediately follow the Tax Reform Act of 1986. "Combining restrictions" are our main estimates. The control groups in each of the regressions are constructed in the same way: they consist of all states which did not experience a reform shifting state taxes one percentage point or more in the eleven-year window around the reform date.