

Corporate Taxes and Employment: Evidence from United States Panel Data, 2012–2025

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ABSTRACT

Do corporate tax cuts raise employment? By analyzing a panel of all 50 states and the District of Columbia from 2012–2025, this paper studies whether reductions in corporate income tax rates are associated with higher employment. The analysis uses state-level corporate tax reforms and estimates employment elasticities using fixed-effects regressions that account for time-invariant state factors, nationwide annual trends, and differences in local economic conditions. Across specifications, the estimated employment elasticity with respect to corporate taxation is statistically significant and negative, ranging between -0.1 and -0.3 , implying that lower corporate tax rates are associated with higher employment. A representative 0.5-percentage-point tax cut is associated with roughly 3,600 to 5,500 additional jobs in the average state, depending on the existing federal tax regime. A distributed lag model shows no evidence of pre-trends and shows that employment responses occur gradually over the two years following the tax reform. While these results are more modest than those reported in previous studies, they provide evidence that corporate tax cuts are associated with economically meaningful increases in employment.

1. INTRODUCTION

Of all the ways a government raises money, few draw as much passion and disagreement as the tax on corporate profits, because nobody can quite agree on who actually pays it. That disagreement is one of the oldest core debates in public economics. Advocates for raising the tax tell a simple, satisfying story: tax the windfall, tax the rich, fund the schools and the roads. Opponents tell an equally compelling one about protecting the small business owners who employ the middle class. Both positions rest on an assumption about where the money actually lands. If the dollar pulled from corporate profit stays with shareholders and executives, raising the tax costs the public almost nothing. However, opponents argue that raising the tax on businesses means they will have less money to spend, which translates to fewer new jobs, fewer raises, or even firms moving states for more favorable tax rates, directly impacting the public.

Classical theory takes that second possibility seriously: higher corporate taxes shrink the after-tax return on investment, and because labor and capital move together, some of that burden can land on workers

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through lower wages or thinner hiring (Auerbach 2002; Hassett and Hubbard 2002; Auerbach and Hines 2002; Gravelle and Smetters 2006). Plenty remain skeptical. To many advocates of higher corporate taxation, it feels like a stretch that a dollar taken from corporate profit ever comes out of an ordinary paycheck, since shareholders are there to absorb the hit. What's striking is that the fight doesn't divide neatly by party. Republican Speaker of the House Paul Ryan built much of his career on the claim that a lower corporate rate means more hiring (Ryan 2017), and Democratic Sen. Ron Wyden spent years pushing his own bill to cut the federal rate (Wyden 2011); Republican Sen. Bob Corker, meanwhile, broke with his party to vote against the 2017 Tax Cuts and Jobs Act over the debt, the same worry that drove Democratic opposition (Kaplan and Rapoport 2017). With deficits high and the future of the 2017 law still contested in Washington, those arguments about investment, growth, and inequality are being relitigated with no more evidence than the first time around. A debate this persistent, one that splits people with otherwise similar goals, won't be settled by ideology. The real question was never whether corporate taxes touch workers, but by how much. That is an empirical question, and this paper takes it up directly: how responsive is employment to corporate taxation, and when a government cuts the rate, do more people actually end up with jobs?

The difficulty is that national data can barely answer this question, because the federal rate rarely moves. It sat at 35% from 1993 to 2017, and then the Tax Cuts and Jobs Act cut it to 21%. That reform arrived alongside the longest expansion on record, a trade war with China, and unemployment near a 50-year low, which is exactly the problem: a single national cut is one treatment observation with nothing clean to compare it against. Supporters credited it with lifting investment and, eventually, wages; critics saw mostly lost revenue and a surge in share buybacks. The evidence since has not resolved the fight, because national data cannot provide a proper counterfactual.

A clean way to settle the issue would be an experiment: randomly assign some states to cut their rate, hold others fixed, and watch employment unfold. That would guarantee treated and untreated states differed only in tax policy, but governments don't set taxes by lottery. State policy comes close enough to be useful: over the last 15 years, some states changed their rates at different times and by different amounts while most held steady, creating the treatment and comparison groups a panel method needs. The states function here as a laboratory, supplying the policy variation a national time series lacks. Furthermore, because the reform wave of the past decade is concentrated in a short window, treated and untreated states can be compared under broadly similar national conditions. The goal isn't to compare high-tax states with low-tax states, which differ in countless ways beyond taxes, but to watch how employment moves when one state changes its rate and others don't.

The empirical literature hasn't settled the question, and most of it looks at wages rather than jobs, on the logic that a tax that workers partly bear should show up as lower pay (Harberger 1962; Fullerton and Metcalf 2002; Auerbach 2006). The direct evidence follows suit: Suárez Serrato and Zidar show that U.S. state corporate taxes move wages and income (2016), and Fuest, Peichl, and Siegloch find German workers absorb part of the burden through lower pay (2018). In neither case is employment the outcome.

There is even less empirical literature on employment itself. The closest paper to this one is that of Giroud and Rauh (2019), who use establishment-level Census microdata to estimate how state corporate taxes affect employment. Their short-run semi-elasticities of -0.4 to -0.5 imply that roughly half of the response reflects firms relocating across state lines rather than shedding jobs outright. We share an approach: both papers exploit actual state-level tax changes rather than national reforms, and both treat employment rather than wages as the outcome. The difference is scope. Their establishment-level data can separate relocation from destruction, whereas I aggregate BLS payroll data to capture the net effect. Ljungqvist and Smolyansky (2014) use a related contiguous-border-county design that excludes much of the country by construction, and Zidar (2019) studies employment through federal changes that hit local labor markets unevenly, a design that assumes exposure is effectively exogenous and doesn't examine state rate changes directly. Whether these local and establishment-level estimates generalize to a broader, more recent statewide panel remains open.

I use an annual panel of all 50 states and the District of Columbia, and across specifications, the relationship comes out consistently negative. Two-way fixed-effects specifications imply elasticities near -0.3 ; adding controls for personal income tax rates and lagged state gross domestic product (GDP), the most conservative cut of the data, brings that to about -0.1 . I lean on the conservative estimate throughout. Even there, a representative 0.5-percentage-point cut, roughly the average reform in the sample, is associated with about 5,500 additional jobs in the average state. The elasticity remains negative.

The size of that effect is the paper's first result; the second is about timing. A distributed-lag specification finds no sign that reforming states were on a different employment path beforehand, and it shows the effect emerging gradually rather than all at once, modest in the year of the change and then roughly tripling over the next two years. That pattern—no unusual movement before the reform followed by an effect that grows steadily after it—is exactly what the identification strategy is built to detect.

Together, the evidence points to a real, if not fully causal, relationship between corporate taxation and employment: consistently negative and cumulative rather than immediate. The rest of the paper proceeds as follows. Section 2 describes the variation in state corporate tax policy and the data. Section 3 outlines the empirical framework, both the baseline fixed-effects specification and the distributed-lag approach. Section 4 presents results: contemporaneous effects, economic magnitude, robustness checks, and dynamics. Section 5 concludes.

2. STATE CORPORATE TAX VARIATION AND DATA

2.1 Patterns in State Corporate Tax Changes

Why might corporate tax changes affect employment? Standard economic models suggest several channels. Lower corporate tax rates increase the after-tax return to investment, potentially encouraging firms to expand productive capacity and hire additional workers. Tax reductions may also increase

internally available funds, thus reducing financing constraints for firms that depend on retained earnings. Finally, tax policy can influence the geographic allocation of economic activity by affecting where firms choose to locate new investment. Whether these mechanisms are quantitatively important is ultimately an empirical question, and one that motivates the analysis that follows.

State corporate tax rates vary substantially across jurisdictions. Figure 1 maps the rates in force in 2025. Six states—Nevada, Ohio, South Dakota, Texas, Washington, and Wyoming—levy no corporate income tax, while several others impose rates above 9%. New Jersey remains among the highest-tax states, while Minnesota and Illinois are also near the upper end of the distribution. A firm deciding where to locate, therefore, faces meaningful differences in statutory tax burdens across state lines.

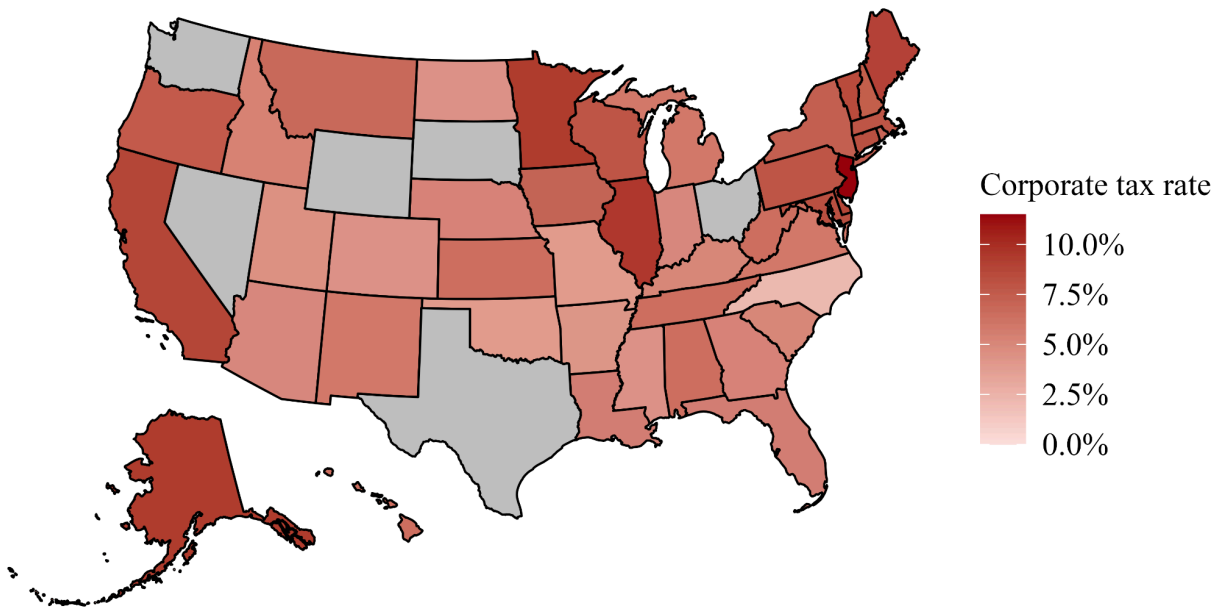


Figure 1. State Corporate Tax Rates, 2025: The figure displays state statutory corporate income tax rates in 2025. States with no corporate income tax are shown in gray.

This cross-sectional snapshot, however, is only part of the story. The estimates in this paper are not identified by permanent differences across states. California and Wyoming differ in many ways beyond tax policy, making direct comparisons of tax levels difficult to interpret. Instead, the analysis uses changes in state corporate tax rates over time.

Figure 2 maps cumulative changes in state corporate tax rates between 2012 and 2023. While most states left their statutory rates unchanged, the states that did reform their tax codes overwhelmingly moved toward lower corporate tax rates. The most prominent example is North Carolina, which reduced its corporate tax rate from 6.9% in 2013 to 2.5% by 2019 and has legislated a complete phase-out by 2030. Similarly, Iowa began the decade with the highest corporate tax rate in the country and subsequently

enacted a series of reductions. Appendix Figure A1 shows that these reforms were part of a broader set of substantial corporate tax cuts enacted across states during the sample period. By contrast, New Jersey posted the largest increase, raising its top corporate rate in 2018 from 9% to 11.5%, where it has remained ever since. New York was the only other state to end the sample with a higher state corporate tax rate than it began with. These reforms were politically contentious, with supporters arguing that lower corporate tax rates would improve economic competitiveness, attract investment, and create jobs, while opponents contended that resulting revenue losses would outweigh any potential economic gains (Walczak 2021; McHugh 2023).

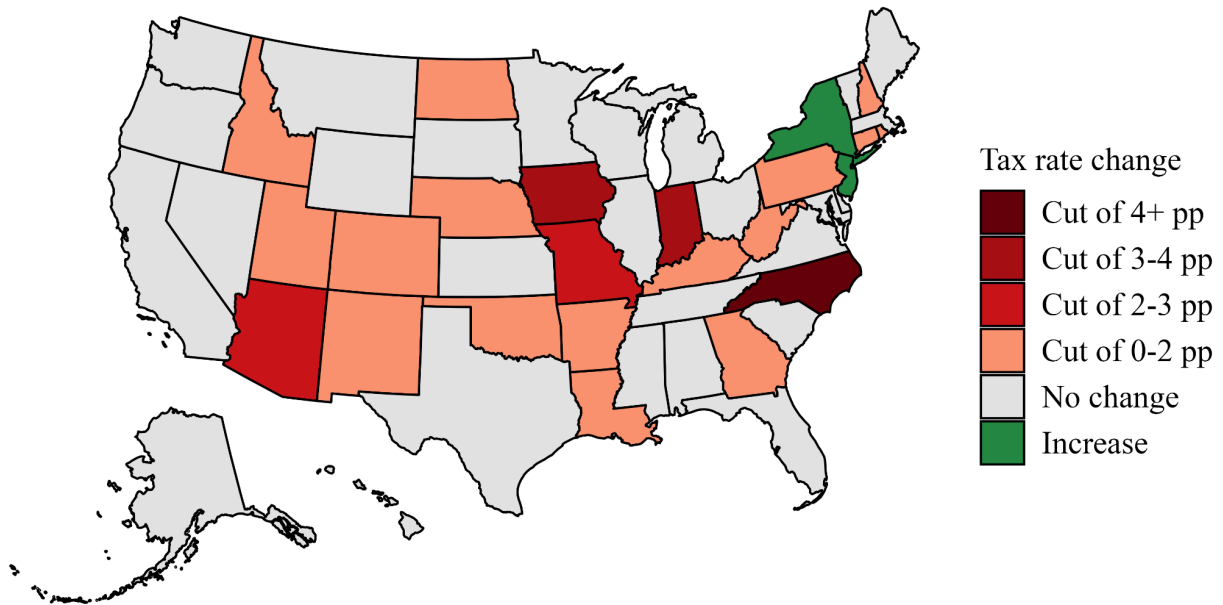


Figure 2. Change in State Corporate Tax Rates, 2012–2023: The figure displays cumulative changes in state corporate tax rates between 2012 and 2023. States are grouped into percentage-point bins.

Figure 3 provides a closer look at the underlying annual variation in state corporate tax rates. Across the sample, there are 561 state-year transitions between 2013 and 2023. Of those transitions, 494 (88.1%) exhibit no change in the corporate tax rate. The remaining 67 transitions exhibit state tax changes, which provide the identifying variation used throughout the paper.

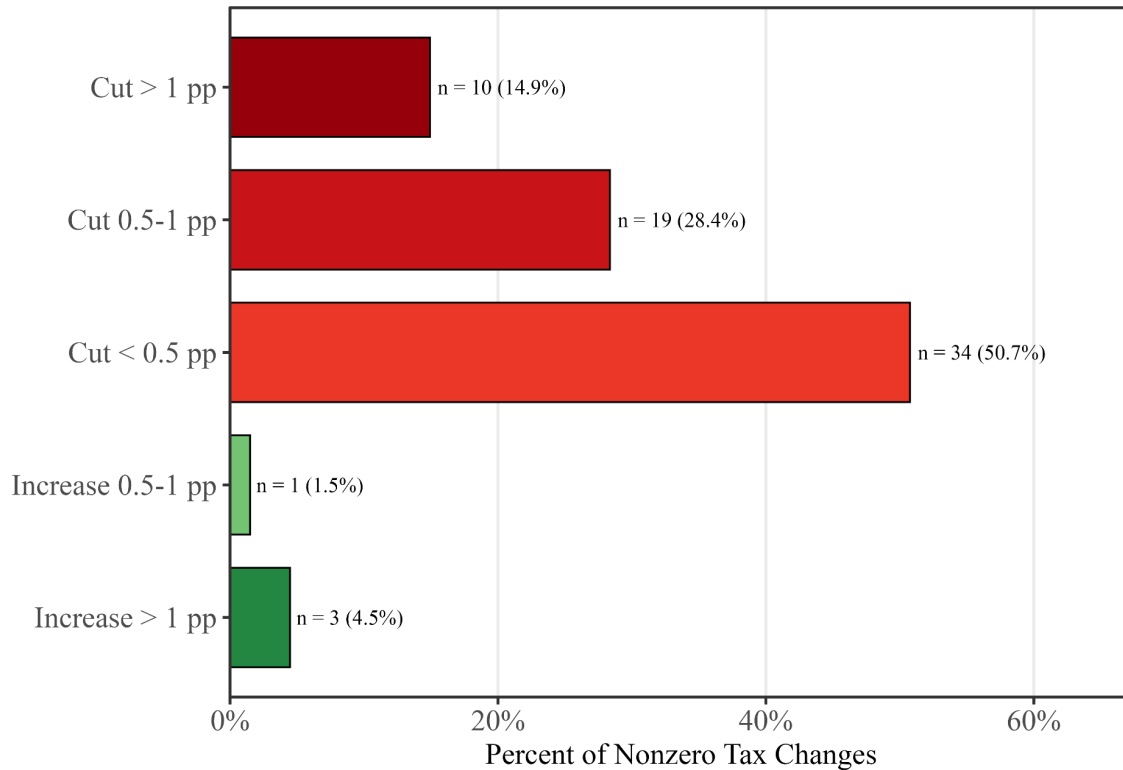


Figure 3. Distribution of Annual Corporate Tax Changes: The figure reports the distribution of nonzero annual state corporate tax changes. It includes 561 state-year observations; 494 (88.1%) exhibit no tax change. The remaining 67 observations provide identifying variation. State years with no tax change are excluded from the bars and reported in Table 1.

Taken together, Figures 2 and 3 illustrate the source of identifying variation in the empirical analysis. Figure 2 shows which states enacted corporate tax reforms, while Figure 3 shows how frequently those reforms occurred. In the ideal experiment described above, some states would receive treatment while others would not. Here, states that change their corporate tax rates serve as the treated units, while states that leave their rates unchanged serve as control units.

Put differently, the analysis is built around a relatively small number of observable policy experiments. This feature is a limitation in the sense that identification depends on a modest number of reforms. At the same time, it is an advantage because individual policy changes can be studied directly. Rather than inferring tax exposure from national reforms or comparing permanently different states, the analysis follows employment outcomes around actual state tax changes.

Figure 3 also shows that annual corporate tax reforms overwhelmingly took the form of tax cuts rather than tax increases. Although the reforms were infrequent, they were economically meaningful. Among

state-year observations in which a corporate tax change occurs, the average annual change was approximately a 0.5-percentage-point tax cut. Given an average state corporate tax rate level of roughly 6%, the typical reform represents a substantial change relative to the initial tax burden. Several states implemented cumulative reductions considerably larger than this benchmark. The policy changes studied here are therefore large enough to plausibly influence firm hiring and investment decisions rather than merely reflecting administrative adjustments.

This pattern is useful for two reasons. First, it provides a substantial number of comparison observations in which tax policy remains unchanged. Second, it limits the number of treatment episodes that must be interpreted, making it easier to study how employment evolves around identifiable policy changes. While the setting is not a randomized experiment, it contains many of the ingredients researchers would want in one: a group of states enacting policy changes, a larger control group that is not, and variation in timing across treatment episodes.

2.2 Data and Measurement

The analysis uses an annual panel of all 50 states and the District of Columbia spanning 2012 through 2025. The primary estimation sample covers 2012 through 2023 and contains 612 state-year observations. Data from 2024 and 2025 are used only in the distributed-lag specifications, allowing future tax reforms to serve as placebo treatments when testing for pre-trends and enabling dynamic analysis of employment responses to tax changes.

State corporate and personal income tax rates are obtained from the Institute on Taxation and Economic Policy's historical state tax series. Federal corporate tax rates are drawn from the Tax Policy Center. Payroll employment data are obtained from the Bureau of Labor Statistics, while state GDP data come from the Bureau of Economic Analysis through Federal Reserve Economic Data.

The primary explanatory variable is the total corporate tax rate, created by adding the statutory state corporate tax rate to the federal corporate tax rate in effect during a given year. The combined measure is used because firms make hiring and investment decisions in response to the total corporate tax burden they face rather than its individual state and federal components. Although state tax reforms provide most of the identifying variation used in the analysis, the combined tax rate more closely reflects the after-tax incentives relevant to firm decision-making. An analogous measure is constructed for personal income taxation.

Table 1 reports summary statistics for the primary variables used in the empirical analysis. Average payroll employment is approximately 2.8 million workers, although the distribution is highly dispersed across states. State corporate tax rates average roughly 6% and range from zero to 12%, while the total corporate tax rate ranges from 21% to 47% over the sample period, reflecting the reduction in the federal corporate tax rate from 35% to 21% at the beginning of 2018. The preferred empirical specifications account for this nationwide policy change, as described in the following section.

Variable	Mean	SD	Min	Median	Max	N
Payroll Employment (thousands)	2844.62	3105.91	274.00	1904.50	17988.40	612.00
Log Payroll Employment	7.48	0.99	5.61	7.55	9.80	612.00
GDP (\$ millions)	403834.63	522177.05	29325.60	238580.30	3827007.30	612.00
Log GDP	12.35	1.04	10.29	12.38	15.16	612.00
State Corporate Tax Rate (pp)	6.29	2.85	0.00	6.50	12.00	612.00
Total Corporate Tax Rate (pp)	34.29	7.70	21.00	34.00	47.00	612.00
State Personal Income Tax Rate (pp)	5.35	3.18	0.00	5.75	13.30	612.00
Total Personal Income Tax Rate (pp)	43.26	3.52	35.00	43.50	52.90	612.00
Annual State Corporate Tax Change (pp)	-0.06	0.30	-2.25	0.00	2.50	561.00
Annual State Corporate Tax Change (Conditional on Change, pp)	-0.46	0.77	-2.25	-0.47	2.50	67.00

Table 1. Summary Statistics: Payroll employment is measured in thousands of workers. GDP is measured in millions of current dollars. Tax rates are reported in percentage points.

The dependent variable is total nonfarm payroll employment, a broad measure of aggregate employment that directly captures the labor market outcome that this paper seeks to analyze. Throughout the paper, employment and tax variables enter the regressions in logarithmic form, allowing coefficients to be interpreted as elasticities. An elasticity describes relationships in percentage terms rather than in the original units of measurement. A coefficient of -0.1 , for example, implies that a 1% increase in the corporate tax rate is associated with approximately a 0.1% reduction in payroll employment.

The descriptive evidence establishes that substantial cross-state differences in corporate taxation persist throughout the sample period. Additionally, the identifying variation used in the analysis comes from a relatively small set of tax reforms concentrated among states that reduced their rates. The next section describes how that variation is used to estimate the relationship between corporate taxation and employment.

3. EMPIRICAL FRAMEWORK

3.1 Contemporary Effects

Figures 2 and 3 suggest a natural comparison between states that change their corporate tax rates and states that do not. The empirical framework asks whether employment evolves differently in reforming states relative to otherwise similar states that leave their tax policies unchanged. A simple comparison of employment levels across states would be difficult to interpret because states differ in many ways beyond tax policy. California and Wyoming, for instance, have different industrial structures, populations, labor markets, and political institutions. Those differences may influence both tax policy and employment outcomes, making cross-sectional comparisons potentially misleading.

To address this concern, this paper's analysis focuses on changes within states over time. The key question is whether employment changes differently after a state changes its corporate tax rate relative to states that do not enact a policy change during the same period. The empirical strategy, therefore, compares employment changes within states before and after tax reforms while controlling for persistent differences across states and nationwide shocks common to all states.

Let $\tau_{s,t}^S$ denote the statutory state corporate income tax rate in state s and year t , and let τ_t^F denote the federal corporate income tax rate in year t . Firms face a total statutory corporate tax rate $\tau_{s,t}^C$ given by:

$$\tau_{s,t}^C = \tau_{s,t}^S + \tau_t^F \quad (1)$$

Although changes in state corporate tax rates generate most of the identifying variation in the data, firms make hiring and investment decisions based on the combined federal and state corporate tax burden they face. Accordingly, the baseline analysis uses the total corporate tax rate as the primary explanatory variable. Specifications using only the state corporate tax rate are reported later as a benchmark for assessing robustness and economic magnitude. The baseline specification is:

$$\ln(\text{Emp}_{st}) = \beta \ln(\tau_{s,t}^C) + X'_{st} \delta + \alpha_s + \gamma_t + \varepsilon_{st} \quad (2)$$

where Emp_{st} denotes total nonfarm payroll employment in state s and year t ; $\tau_{s,t}^C$ denotes the total corporate tax rate; X_{st} incorporates control variables; α_s represents state fixed effects; γ_t represents year fixed effects; and ε_{st} is an error term.

State fixed effects eliminate permanent differences across states, such as geography, long-term institutional characteristics, or industrial compositional factors. Year fixed effects account for annual trends common in all states, including the reduction in the federal corporate tax rate beginning in 2018. As a result, the coefficient of interest β , the elasticity of employment with respect to the corporate tax rate, is identified from within-state changes in corporate tax rates over time rather than from permanent differences in tax levels across states.

One issue with two-way fixed effects under staggered treatment timing is that states cutting taxes in earlier years are effectively compared against states cutting taxes later, even though the early reformers are themselves already treated (Sun and Abraham 2011). There are two features of the research design that mitigate these concerns. First, Figure 3 shows that nearly all the tax reforms in the sample are tax cuts, so the contemporaneous comparisons at least involve treatments moving in the same direction rather than increases and cuts offsetting one another's effects. Appendix Table A3 excludes all four states that increased tax at any point during the sample, and the elasticity estimates are essentially unchanged. Second, the subsequent section's distributed lag results test for and find no significant differential pre-trends between states. That said, this paper's elasticity estimates are still best interpreted as a weighted average of several pairwise comparisons rather than one single ideal experimental tax cut. Re-estimating the results with more robust estimators that account for staggered reforms would be an appropriate extension, though beyond the scope of this paper (e.g., the estimator suggested by Callaway and Sant'Anna 2021).

Table 2 reports a sequence of specifications of Equation (2) that add increasingly more control variables; thus, the specifications reported in Table 2 differ only in the contents of X_{st} and the inclusion of fixed effects. Column (1) includes no controls or fixed effects. Column (2) adds state fixed effects. Column (3) adds year fixed effects. Column (4) augments the baseline specification with lagged log state GDP to account for local economic conditions. Column (5), the most conservative specification, includes the logged total personal income tax rate to distinguish between corporate tax policy and other components of the tax system. This progression allows the estimated relationship between taxation and employment to be evaluated under increasingly restrictive controls for state-specific macroeconomic factors.

Because employment is likely highly persistent within states, the errors in Equation (2) are unlikely to be independent across years. As a result, previous years' effects are possibly still in place. Therefore, all standard errors are clustered by states, allowing for serial correlation within states over time (Bertrand, Duflo, and Mullainathan 2004). The lagged log state GDP control further absorbs persistent local economic conditions when estimating elasticities, and the Section 3.2 distributed-lag specification models the persistence of the employment response directly.

3.2 Dynamic Effects

The baseline specification estimates the average contemporaneous relationship between corporate taxation and employment. However, firms may not fully adjust hiring immediately following a tax

reform. Hiring plans, investment decisions, and business expansion may occur gradually, implying that the employment response to changes in corporate tax changes may take several years. Likewise, if employment begins changing before a tax reform is implemented, the estimated contemporaneous relationship in Equation (2) may reflect pre-existing trends rather than the causal effect of tax policy.

To examine these dynamics, the analysis augments Equation (2) by adding two leads and two lags of the total corporate rate. The distributed-lag specification is:

$$\ln(Emp_{st}) = \sum_{k=-2}^2 \beta_k \ln(\tau_{s,t-k}^c) + X'_{st} \delta + \alpha_s + \gamma_t + \varepsilon_{st} \quad (3)$$

where negative values of k correspond to lead terms, $k=0$ is the contemporaneous corporate tax rate, and positive values correspond to lag terms. The summation therefore estimates a separate coefficient for each lead and lag of the corporate tax rate.

The lead coefficients provide a direct test of the identifying assumption underlying the baseline specification. If future corporate tax changes predict current employment outcomes, this would suggest that reforming states were already following different employment trends than control states before the tax changes were implemented. In contrast, lead coefficients that are statistically indistinguishable from zero provide evidence that there were not pre-trends confounding the empirical design.

The lag coefficients measure how employment evolves following a corporate tax change. Rather than assuming that firms adjust employment immediately, the distributed-lag specification allows employment responses to evolve over time.

Table 4 reports the estimated lead and lag coefficients, while Figure 4 plots the corresponding point estimates and 95% confidence intervals to illustrate the timing and persistence of the estimated employment response.

4. RESULTS

4.1 Contemporary Employment Elasticities

Table 2 reports estimates of the relationship between corporate taxation and payroll employment. The specifications progressively add state fixed effects, year fixed effects, controls for local economic conditions via lagged state GDP, and controls for other tax-related factors via the total personal income tax rate. Rather than emphasizing a single estimate, I interpret the sequence of specifications as providing a plausible range of employment elasticities. Column (5), which includes both personal income tax

controls and lagged state GDP, serves as a conservative benchmark because these controls absorb variation associated with broader economic conditions and therefore produce the smallest estimated employment response.

	No FE	State FE	State + Year FE	+ Lag GDP	+ Log Personal Tax + GDP
Log Total Corporate Tax Rate	-0.163	-0.112***	-0.302**	-0.138**	-0.104*
	(0.293)	(0.017)	(0.115)	(0.056)	(0.060)
Lagged Log GDP				0.523***	0.522***
				(0.051)	(0.050)
Log Total Personal Tax Rate					-0.203
					(0.134)
Observations	612	612	612	561	561
State FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes

Table 2. Corporate Tax Elasticities: Total Corporate Tax Rate: Standard errors clustered by state are in parentheses. Symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

The raw relationship between corporate taxation and employment is negative but statistically imprecise. Column (1) reports an elasticity of -0.163 , but the estimate is not statistically distinguishable from zero. This result is unsurprising. States differ in many ways unrelated to tax policy, and a simple comparison across states is unlikely to isolate the effect of corporate taxation on employment.

Adding state fixed effects in Column (2) removes permanent differences across states and substantially increases precision. The estimated elasticity becomes -0.112 and is statistically significant at the 1% level. However, this specification still combines state-level tax changes with national shocks that affect all states simultaneously. Most importantly, the 2018 federal corporate tax reduction coincides with a period of strong national labor-market performance, making it difficult to distinguish the effects of tax policy from broader macroeconomic conditions.

Column (3) addresses this concern by adding year fixed effects. Once common national shocks are removed, the estimated elasticity increases to -0.302 and remains statistically significant. Because identification now comes entirely from differences in state tax changes over time, this specification

compares employment growth in reforming states to employment growth in otherwise similar states that leave their tax policies unchanged.

Columns (4) and (5) introduce additional controls for lagged state GDP and personal income tax rates. The estimated elasticity falls to -0.138 in Column (4) and -0.104 in Column (5) but remains statistically significant throughout. The reduction in magnitude is informative. State economic performance is correlated with both employment growth and tax policy, so controlling for lagged GDP removes an important source of potential omitted-variable bias that may influence both employment and tax policy. Critically, I control for lagged GDP rather than contemporaneous GDP because contemporaneous GDP may partially lie on the causal pathway linking corporate taxation to employment. If lower taxes stimulate investment and economic activity, controlling for contemporaneous GDP may remove part of the mechanism through which tax policy affects labor demand. For this reason, I control for lagged GDP as a more conservative control for underlying economic conditions while reducing the risk of removing some of the tax policy's effects. Taken together, I view the coefficient of -0.104 in Column (5) as a conservative lower bound rather than necessarily the most accurate estimate of the full employment response.

Taken together, Table 2 produces a stable qualitative result. Every specification that includes both state and year fixed effects yields a negative and statistically significant elasticity. The estimates span a range from approximately -0.10 to -0.30 , with the conservative benchmark at the lower end of that interval. Although the precise magnitude depends on specification, the evidence consistently suggests that lower corporate tax rates are associated with higher payroll employment.

4.2 From Elasticities to Employment

While Table 2 reports estimated employment elasticities, Table 3 translates the estimated elasticities into implied employment effects associated with a representative 0.5-percentage-point reduction in the state corporate tax rate. This benchmark is not arbitrary. As shown in the last row of Table 1, the average annual reform among states that changed their corporate tax rates was approximately 0.5 percentage points, making it a useful illustration of the economic significance of the estimates.

Statistic	Total Tax (35% Federal)	Total Tax (21% Federal)	State Tax Only
Starting tax rate (%)	41.3	27.3	6.3
Tax rate after 0.5 pp cut (%)	40.8	26.8	5.8
Log tax change	-0.012	-0.018	-0.083
Elasticity used	-0.104	-0.104	-0.030
Implied employment effect (%)	0.127	0.192	0.249
Implied jobs, average state	3,601	5,465	7,087

Table 3. Implied Employment Effects of a 0.5 Percentage-Point State Corporate Tax Cut:

Calculations use the exact unrounded elasticities from Column (5) of Table 2 and Table A1. Implied jobs are calculated using the Table 1 mean payroll employment of 2,844,620 workers. Tax rates are reported in percentage points. Payroll employment is measured in thousands of workers.

The first two columns of Table 3 are based on the conservative benchmark elasticity with respect to the total corporate tax rate reported in Table 2, Column (5) of -0.104. The first column applies this elasticity to the pre-2018 federal corporate tax regime, during which firms faced a 35% federal corporate tax rate. Combined with the average 6.3% state corporate tax rate reported in Table 1, the representative firm's total statutory corporate tax rate was approximately 41.3%. A 0.5-percentage-point reduction in the state corporate tax rate therefore reduces the total corporate tax rate from 41.3% to 40.8%, implying a log tax change of -0.012, or a roughly 1.2% tax cut. Applying the elasticity from Table 2, Column (5) implies a resulting employment increase of 0.127%. Based on the Table 1 average payroll employment of 2,844,620 employees in the average state, this corresponds to approximately 3,601 additional jobs for the average state.

Where do these estimates fit within the previous literature? Giroud and Rauh (2019) report a conservative employment semi-elasticity with respect to the corporate tax rate of -0.4. Unlike the elasticities estimated in this paper, which measure the percentage change in employment in response to a 1% proportional change in the corporate tax rate, a semi-elasticity measures the percentage change in employment in response to a one-percentage-point change in the corporate tax rate. This distinction makes the semi-elasticity relatively straightforward to interpret. Under the most conservative semi-elasticity reported by Giroud and Rauh (2019), a 0.5-percentage-point tax cut would increase employment by roughly 0.2%, which would correspond to approximately 5,689 jobs for the average state. As a result, while this paper still finds a significant boost to employment in response to a tax cut, the effects are somewhat smaller than those found in previous literature.

The second column in Table 3 repeats the same calculation under the post-2018 federal corporate tax regime, during which firms faced a reduced 21% federal corporate tax rate. Combined with the average 6.3% state corporate tax rate reported in Table 1, the representative firm's total statutory corporate tax rate was approximately 27.3%. The same 0.5-percentage-point reduction therefore lowers the total corporate tax rate from 27.3% to 26.8%, corresponding to a larger proportional reduction in the total corporate tax of roughly 1.8%. Thus, applying the same elasticity reported in Table 2, Column (5) implies a larger employment increase of 0.192%. Applied to the Table 1 average payroll employment, the 0.5-percentage-point tax cut corresponds to approximately 5,465 additional jobs in the average state. Thus, the results from this paper under the post-TCJA regime are quite similar to the effects predicted by the most conservative semi-elasticity reported by Giroud and Rauh (2019).

These implied employment gains are economically meaningful. Even under the conservative benchmark reported in Table 2, Column (5), a typical corporate tax cut is associated with several thousand additional jobs for the average state, an important justification driving many policymakers' advocacy of lower

corporate rates. One limitation with the state-level employment data is that it is not possible to distinguish between jobs created and jobs moving across state lines. Giroud and Rauh (2019) attribute roughly half of the overall employment response to relocation using employee-level data, thereby suggesting that this paper's estimates have a smaller net national effect. At the same time, the estimated effects are more modest than those reported in the existing literature. This difference is not unexpected: the limited previous literature estimates employment responses using research designs intending to strengthen causal inference, such as businesses located near state borders or firms within a state experiencing differential exposure to tax reforms. By contrast, this paper estimates the average effect of corporate tax changes on total statewide payroll employment using the full panel of all 50 states and the District of Columbia. As a result, this paper's estimates likely reflect economy-wide employment responses rather than outcomes for selected firms or locations. Although more conservative in magnitude than parts of the existing literature, these estimates suggest that typical corporate tax cuts nevertheless are associated with economically meaningful increases in employment.

4.3 State Corporate Taxes vs. Total Corporate Taxes

The baseline analysis focuses on the total corporate tax rate because firms respond to the total tax burden imposed by both federal and state governments. This measure is economically appealing because firms ultimately make hiring and investment decisions based on their total corporate tax burden. At the same time, one may argue that the identifying variation in this paper comes primarily from state corporate tax reforms, since year fixed effects remove nationwide annual shocks such as the 2018 federal corporate tax reduction. However, adding state and federal rates and taking the natural logarithm has a mechanical implication for interpreting the elasticities. After TCJA cut the federal rate from 35% to 21% in 2018, the same state rate cut represents a larger proportional change in the total rate. The analysis addresses this concern in three ways. First, year fixed effects largely absorb the effects of the federal change. Second, Table 3 reports implied employment effects separately under pre-TCJA and post-TCJA regimes rather than combining them. Third, this section re-estimates every specification using the state rate as the independent variable of interest to examine whether the main results are sensitive to measuring corporate taxation using only state corporate tax rates.

Appendix Table A1 re-estimates the baseline specification from Equation (2) after replacing the total corporate tax rate $\tau_{s,t}^C$ with the statutory state tax rate $\tau_{s,t}^S$:

$$\ln(Emp_{st}) = \beta \ln(\tau_{s,t}^S) + X'_{st} \delta + \alpha_s + \gamma_t + \varepsilon_{st}, \quad (4)$$

where all control variables, fixed effects, estimation procedures, and samples remain identical to those used to estimate Equation (2). As a result, each column of Appendix Table A1 corresponds directly to the same column of Table 2; the only difference between the two regression specifications is the corporate tax

variable used. The estimated elasticities remain negative throughout Appendix Table A1 and follow nearly the same progression observed in Table 2. As additional controls are introduced, the estimated elasticity declines in magnitude while remaining statistically significant once both state and year fixed effects are incorporated. The conservative benchmark state tax elasticity with full controls in Appendix Table A1, column (5) is -0.03 , compared with the analogous total tax elasticity of -0.104 in Table 2, Column (5).

The state-tax elasticity estimates are smaller in magnitude than the combined-tax estimates, but the difference should not be interpreted as evidence that state corporate tax changes have smaller employment effects, because the two specifications estimate different economic elasticities. As displayed in Table 1, the average state corporate tax rate is 6.3%, whereas the average total corporate tax rate ranges from 27.3% (after TCJA) to 41.3% (before TCJA). Thus, a 1% reduction in total corporate tax measure requires a significantly larger reduction in state corporate tax rates than a one percent reduction in the state corporate tax rate itself. Thus, comparing elasticities is less informative than comparing the employment effects associated with a given tax cut.

Table 3, Column (3) therefore applies the conservative benchmark elasticity reported in Appendix Table A1, Column (5) to the same representative 0.5-percentage-point reduction in the state corporate tax rate considered in Section 4.2. Relative to the average 6.3% state corporate tax rate reported in Table 1, this reform represents a log tax change of -0.083 , corresponding to a roughly 8.3% reduction in the state corporate tax rate. By comparison, the same policy corresponds to only a 1.2% reduction in the firm's total corporate tax burden before 2018 and a 1.8% reduction after 2018. Applying the state-tax elasticity therefore implies an employment increase of 0.249%, corresponding to approximately 7,087 additional jobs for the average state. The larger implied employment effect reflects the larger proportional reduction in the state corporate tax rate rather than stronger underlying employment responses.

Taken together, Appendix Table A1 and Table 3, Column (3) demonstrate that the paper's conclusion that tax cuts boost employment is robust to using only state corporate tax rates for identification.

4.4 Additional Robustness Checks

The baseline results suggest a negative relationship between corporate taxation and employment. A remaining concern is whether the findings are driven by a particular subset of states or by specific modeling choices. Appendix Tables A2 and A3 therefore report several additional robustness exercises.

Appendix Table A2 re-estimates Equation (2) by excluding the six states with no corporate income tax at all: Nevada, Ohio, South Dakota, Texas, Washington, and Wyoming. These states could skew the results—some, like Texas and Washington, have unusually large, diversified economies that behave differently for reasons unrelated to tax policy. But excluding them barely changes anything: the conservative benchmark elasticity moves only from -0.104 to -0.124 , and the broader pattern—negative, statistically significant effects—holds across specifications, with several estimates growing somewhat

larger. The sample size differs between Table 2 and Appendix Tables A1 and A2 because states with a zero corporate tax cannot be included when the tax variable is expressed in logarithms.

Appendix Table A3 re-estimates Equation (2) by excluding the four states that increased corporate tax rates during the sample period: Florida, Illinois, New Jersey, and New York. As shown in Figure 3, tax increases account for only four of the 67 reforms in the sample; roughly 94% of the identifying variation comes from tax cuts. Therefore, one concern is that this small group of increases may disproportionately influence the baseline estimates. Restricting the sample to tax-cut states produces results very similar to the baseline findings. The conservative benchmark elasticity is -0.127 , again close to the baseline estimate of -0.104 . The qualitative conclusions are therefore unchanged.

Taken together, the robustness exercises suggest that the baseline findings are not driven by a handful of unusual states or by a small number of tax increases. Across alternative samples, the estimated employment elasticities remain close to the conservative benchmark and consistently retain the expected negative sign. The stability of the estimates across these exercises increases confidence that the relationship documented in Table 2 reflects a broader pattern in the data rather than a particular modeling choice or subset of observations.

4.5 Dynamic Effects

The contemporaneous estimates in Table 2 establish a negative relationship between corporate taxation and employment, but they do not reveal how quickly employment responds to policy changes. Figure 4 and Table 4 address this question using the distributed-lag specification introduced in Section 3.

The most important result concerns pre-trends. Table 4 shows that the lead-two coefficient is approximately 0.011 and the lead-one coefficient is -0.065 , with neither estimate statistically distinguishable from zero. A joint statistical test fails to reject the null hypothesis that the lead coefficients are jointly equal to zero ($p = 0.303$). States that eventually change their tax rates, therefore, do not appear to be on systematically different employment trajectories before reform. This finding supports the identification strategy and reduces concerns that tax changes simply occur in states already experiencing unusual employment growth.

	Distributed-Lag Model
Lead 2: Log Total Corporate Tax Rate	0.011 (0.072)
Lead 1: Log Total Corporate Tax Rate	-0.065 (0.043)
Current: Log Total Corporate Tax Rate	-0.053** (0.026)
Lag 1: Log Total Corporate Tax Rate	-0.077*** (0.025)
Lag 2: Log Total Corporate Tax Rate	-0.179** (0.081)
Observations	510
State FE	Yes
Year FE	Yes

Table 4. Distributed-Lag Estimates: Total Corporate Tax Rate: Standard errors are clustered by state in parentheses. Dynamic specifications use the 2012–2025 sample to preserve observations required for lead and lag terms. Symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. A joint test fails to reject the null that the lead coefficients equal zero ($p = 0.303$), providing no evidence of pre-trends. A joint test rejects the null that the lag coefficients equal zero ($p = 0.003$), indicating that employment responses persist after implementation.

The post-treatment coefficients tell a different story. The contemporaneous coefficient is -0.053 , indicating that employment begins responding during the year of the reform. One year later, the estimated effect grows to -0.077 , and after two years it reaches -0.179 . Figure 4 visualizes the gradual adjustment more clearly: the two-year effect is more than three times as large as the contemporaneous estimate. The dynamic pattern is statistically meaningful as well as economically intuitive. A joint test rejects the null hypothesis that the post-treatment coefficients are jointly equal to zero ($p = 0.003$), and the cumulative post-treatment effect is also statistically significant.

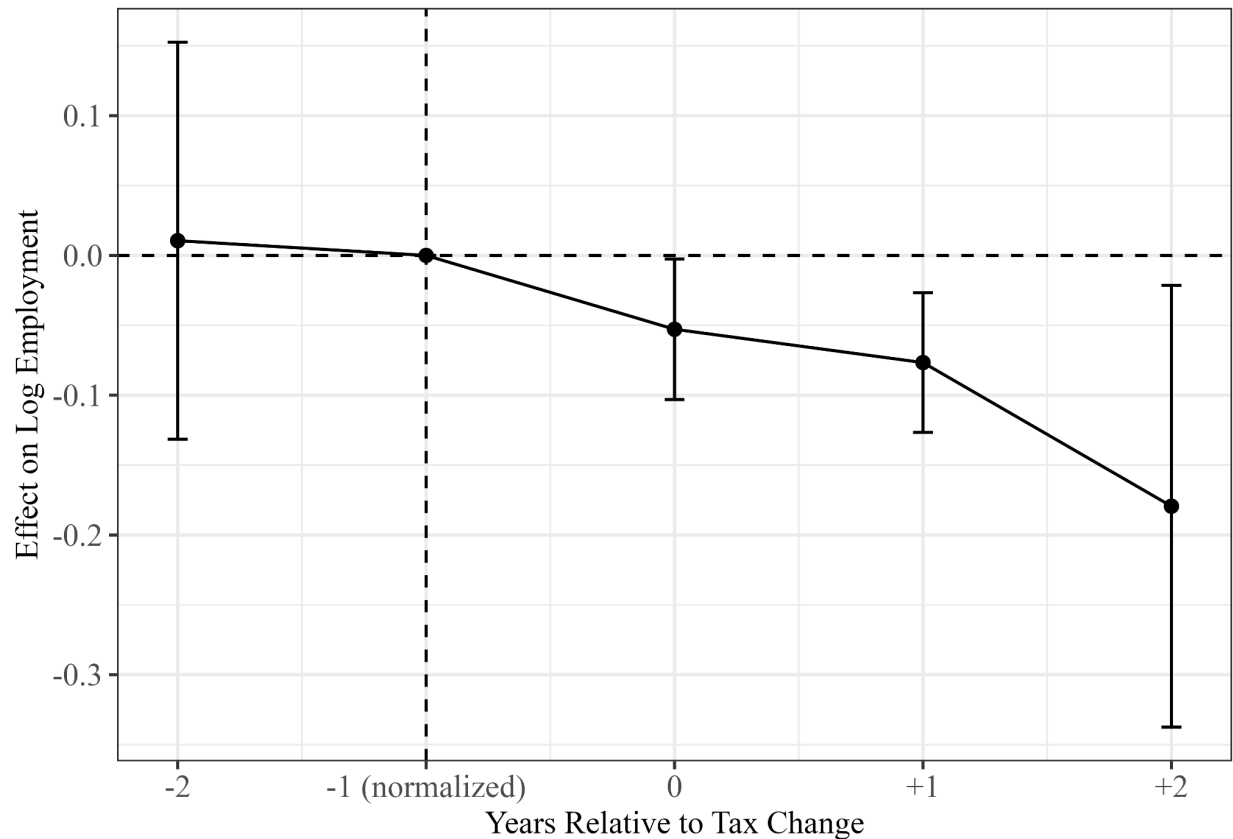


Figure 4. Dynamic Relationship Between Total Corporate Taxes and Payroll Employment: The figure plots distributed-lag coefficients from the total-tax specification. Year -1 is normalized to zero. Error bars denote 95% confidence intervals. Standard errors are clustered by state. Pre-trend $p = 0.303$. Joint significance of lagged effects $p = 0.003$.

There are a few reasons the effect on employment might keep growing after a tax cut. First, existing companies may hire more people, but not right away. It takes time to rework investment plans, expand operations, and get new equipment or space in place before those new jobs actually open up. Second, firms may initially hesitate to hire due to uncertainty about future tax policy. If a tax cut occurs one year, firms may wait until they believe its persistence before dramatically increasing hiring. Finally, new firms could enter the market, which would likely take time to appear in the data as capital may be fixed in the short run; it takes time to break leases or build new factories. The existing payroll employment outcome variable does not allow distinguishing between these various explanations, although subsequent research would benefit from exploring this decomposition further.

The dynamic estimates also help explain the range of elasticities reported in Table 2. Contemporaneous specifications capture only part of the employment response, while longer-run estimates incorporate adjustments that continue after implementation. The distributed-lag results therefore suggest that full employment effects of corporate tax changes may emerge gradually over several years.

Taken together, the dynamic results strengthen the interpretation of the baseline estimates. The absence of detectable pre-trends supports the identifying assumptions underlying the fixed-effects regressions, while the gradual emergence of post-treatment effects is consistent with plausible economic mechanisms linking corporate taxation to labor demand. Rather than appearing before tax reforms occur, the employment response emerges only after policy changes are implemented and grows over time. While omitted factors or statistical limitations could drive results, the potential theoretical economic mechanisms at play at least remain plausible.

5. CONCLUSION

Corporate taxation remains one of the most debated issues in public finance because estimating its effect is challenging. Jurisdictions that change their tax policies typically differ from those that don't, making it hard to separate the tax from other concurrent factors. This paper uses state corporate tax reforms between 2012 and 2025 to isolate that variation, exploiting a period dense with identifiable reforms enacted under broadly similar national conditions—close to the setting a researcher would design if the choice were theirs.

The central finding is that corporate taxation and employment move in opposite directions, and the result holds across every specification that credibly identifies it. Models with both state and year fixed effects yield employment elasticities between -0.1 and -0.3 . The most conservative benchmark, which additionally controls personal income tax rates and lagged state GDP, still yields an elasticity of -0.104 , statistically significant and negative. In concrete terms, a 0.5-percentage-point cut in the corporate rate, which is roughly the average reform in the sample, corresponds to about 3,600 additional jobs in the average state in the pre-TCJA era and 5,500 in the post-TCJA era. The magnitude shifts with specification; the negative sign does not.

The dynamic evidence sharpens the interpretation rather than merely supporting it. Reforming states were on the same employment trajectory as non-reforming states right up to the year of reform: the lead coefficients are jointly indistinguishable from zero, so the results are not being driven by states that were already pulling ahead before they touched their tax code. After a reform, the response builds: the effect two years out is more than three times the contemporaneous effect, consistent with firms revising investment and hiring gradually as capacity adjusts rather than reacting all at once. A pattern this specific—flat before, accumulating after—is exactly what a genuine employment response looks like and exactly what a spurious correlation does not.

These findings speak to one important margin of corporate taxation: lower corporate taxes are linked to higher employment, but they also reduce revenue and may force offsetting changes in spending or other taxes; a full welfare accounting would weigh the employment gains documented here against those fiscal costs. That accounting is beyond this paper's scope. What this paper does establish is that the employment

margin is real and quantitatively meaningful, and that it emerges over several years rather than immediately, which means analyses that look only at the year of a reform will understate the actual effect of corporate tax policy on job counts.

The clearest limitation is that employment is measured at the state level, so the estimates capture the net effect without revealing who is behind it. They cannot say which industries, regions, or workers drive the response, and there is good reason to expect it is uneven: standard models imply that capital-intensive, geographically mobile sectors should respond more strongly to tax policy than sectors tied to local demand, and the gains may fall differently across younger, lower-skilled, or tradable-sector workers than across the rest of the workforce. Resolving that heterogeneity is the natural next step and would clarify not just how large these effects are but who ultimately captures them. What the present evidence already makes clear is that corporate taxation reaches beyond firms and shareholders to the labor market itself: across a decade of state-specific reforms, states that cut their corporate rates saw stronger employment growth, with effects that built gradually and persisted over time.

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APPENDIX

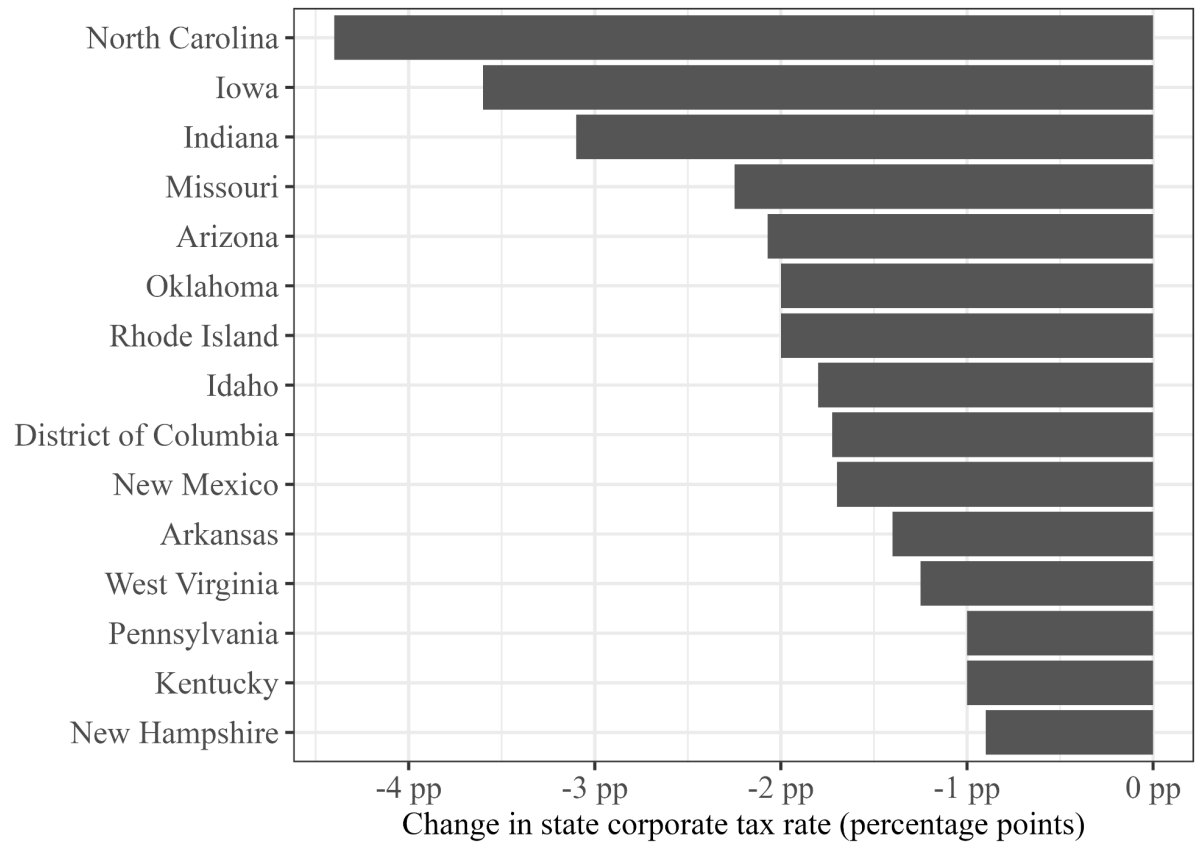


Figure A1. Largest State Corporate Tax Cuts, 2012–2023: The figure reports the 15 states experiencing the largest cumulative reductions in corporate tax rates between 2012 and 2023.

	No FE	State FE	State + FE	Year + Lag GDP	+ Log State Personal Tax + GDP
Log State Corporate Tax Rate	-0.325 (0.495)	-0.164*** (0.034)	-0.056** (0.025)	-0.034** (0.013)	-0.030** (0.013)
Lagged Log GDP				0.541*** (0.062)	0.579*** (0.050)
Log State Personal Tax Rate					-0.022 (0.016)
Observations	540	540	540	495	451
State FE	No	Yes	Yes	Yes	Yes
Year FE	No	No	Yes	Yes	Yes

Table A1. Corporate Tax Elasticities: State Corporate Tax Rate: Standard errors clustered by state are in parentheses. States with zero state corporate or personal tax rates are omitted from log specifications requiring those variables. Symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	No FE	State FE	State + FE	Year + Lag GDP	+ Personal Tax + GDP	Log Tax
Log Total Corporate Tax Rate	-0.176 (0.161)	-0.110*** (0.018)	-0.375*** (0.128)	-0.173*** (0.064)	-0.124* (0.069)	
Lagged Log GDP				0.533*** (0.061)	0.532*** (0.059)	
Log Total Personal Tax Rate					-0.223 (0.134)	
Observations	540	540	540	495	495	
State FE	No	Yes	Yes	Yes	Yes	
Year FE	No	No	Yes	Yes	Yes	

Table A2. Robustness: Total Corporate Tax Rate, Excluding Zero State-Tax States: Standard errors clustered by state are in parentheses. Sample excludes states that ever had a zero corporate tax rate between 2012 and 2023. Symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	No FE	State FE	State + FE	Year + Lag GDP	+ Personal + GDP	Log Tax
Log Total Corporate Tax Rate	-0.251 (0.304)	-0.111*** (0.018)	-0.316** (0.135)	-0.159** (0.064)	-0.127* (0.066)	
Lagged Log GDP				0.521*** (0.053)	0.520*** (0.052)	
Log Total Personal Tax Rate					-0.236 (0.148)	
Observations	564	564	564	517	517	
State FE	No	Yes	Yes	Yes	Yes	
Year FE	No	No	Yes	Yes	Yes	

Table A3. Robustness: Total Corporate Tax Rate, Excluding Tax-Increase States: Standard errors clustered by state are in parentheses. Sample excludes all observations from states that enacted at least one state corporate tax increase during 2012–2023. Symbols *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively. The following states are removed entirely from the sample, and the years in parentheses indicate when the increase occurred: Florida (2022: +1.97 pp); Illinois (2018: +1.75 pp); New Jersey (2018: +2.50 pp); New York (2021: +0.75 pp).