

**Finance and Economics Discussion Series
Divisions of Research & Statistics and Monetary Affairs
Federal Reserve Board, Washington, D.C.**

**To Cut or Not to Cut? On the Impact of Corporate Taxes on
Employment and Income**

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2016-006

Please cite this paper as:

Ljungqvist, Alexander, and Michael Smolyansky (2016). "To Cut or Not to Cut? On the Impact of Corporate Taxes on Employment and Income," Finance and Economics Discussion Series 2016-006. Washington: Board of Governors of the Federal Reserve System, <http://dx.doi.org/10.17016/FEDS.2016.006>.

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To Cut or Not to Cut?
On the Impact of Corporate Taxes on Employment and Income * †

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December 11, 2015

* We are grateful to Robert Chirinko, Stefano Giglio, Boyan Jovanovic, David Merriman, Holger Mueller, Thomas Philippon, Robert Shimer, Lisa De Simone, David Thesmar, Owen Zidar, and seminar participants at Sveriges Riksbank, the NYU Macro Lunch, the 2014 UNC Tax Symposium, and the 2015 National Tax Association meetings for helpful comments.

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To Cut or Not to Cut?

On the Impact of Corporate Taxes on Employment and Income

Abstract

Do corporate tax increases destroy jobs? And do corporate tax cuts boost employment? Answering these questions has proved empirically challenging. We propose an identification strategy that exploits variation in corporate income tax rates across U.S. states. Comparing contiguous counties straddling state borders over the period 1970 to 2010, we find that increases in corporate tax rates lead to significant reductions in employment and income. We find little evidence that corporate tax cuts boost economic activity, unless implemented during recessions when they lead to significant increases in employment and income. Our spatial-discontinuity approach permits a causal interpretation of these findings by both establishing a plausible counterfactual and overcoming biases resulting from the fact that tax changes are often prompted by changes in economic conditions.

Key words: Fiscal policy, Corporate taxation, Economic stimulus, Economic growth, Employment, Regional economies.

JEL classification: H25, E62.

Recent controversies surrounding tax competition, corporate tax inversions, and the apparent reluctance to repatriate corporate profits earned abroad are testament to the internationally high rates at which U.S. corporate profits are taxed. In response, policymakers are currently debating various proposals aimed at reforming U.S. corporate taxation. To inform this debate, we examine empirically how corporate taxes affect employment and income.

Measuring the economic consequences of changes in corporate taxes is challenging for two reasons. First, changes in tax policy are unlikely to be random and instead are themselves influenced by prevailing economic conditions and other factors. For example, in recessions, governments may cut taxes hoping to boost growth; in booms, they may raise taxes to achieve desired distributional outcomes. As a result, observed correlations between changes in tax policy and ex post economic outcomes such as changes in employment and income will, to some unknown extent, reflect unobserved or omitted variation in economic conditions. Compounding this problem is the fact that macroeconomic time-series are relatively short and so typically cover few policy changes. This means that disentangling the effects of a given tax change from the confounding effects of other events that coincide with it is difficult.

The second empirical challenge is that we do not observe counterfactual outcomes. That is, even if a tax change was truly random, we do not know how employment and income would have evolved had the tax change not occurred. The absence of a counterfactual means that it is impossible to measure the impact of changes in tax policy without imposing further assumptions on the behavior of the economic variables of interest.

We propose an empirical strategy designed to address these identification challenges. Our strategy has three elements. The first is to focus on individual U.S. states rather than the federal level. In contrast to federal corporate tax rates, which change only rarely,¹ changes in state-level corporate taxes are plentiful: we count 271 changes in state corporate income tax rates since

¹ Since 1969, there have been only three substantive changes in the top federal corporate income tax rate: in 1979 (from 48% to 46%), in 1986 (from 46% to 34%), and in 1993 (from 34% to 35%).

1969. Conveniently, these tax changes are staggered in time across states such that in any given year, some states are “treated” with a tax change while others are not. Because the tax changes occur at different times in different states, we can use a difference-in-differences approach. This deals with the problem of confounding variation in economic conditions in a way that would not be possible were we to focus on changes in federal tax rates.

The second element of our empirical strategy is designed to minimize the effects of such confounds further – even those that may be difficult to observe or measure. To this end, we exploit a spatial policy discontinuity when forming control groups. Because a state’s tax jurisdiction stops at its border, its immediate neighbors share similar economic conditions but have discretely different tax policies. This makes neighboring states a plausible source of counterfactuals and so we draw our controls from a treated state’s neighbors.

To see why this is helpful, consider the example of Arizona, which in 1998 cut its top corporate income tax rate from 9% to 8%. The identification challenge is to estimate how much of the observed change in economic activity can be attributed to the tax cut and how much would have happened anyway. We might look to Utah to provide an estimate of this counterfactual. Utah did not change tax rates in 1998, but being a neighbor arguably shares similar economic conditions and so similar trends. Arizona’s increases in employment and income over and above those in Utah then provide an estimate of the effect of the tax cut on economic activity.

The third element of our empirical strategy refines this diff-in-diff approach further. To get as close to a plausible counterfactual as possible, our tests compare not neighboring *states* (as in our stylized example) but contiguous *counties* located either side of a state border. This deals with the possibility that states vary their tax rates for reasons that correlate with unobserved changes in economic conditions, which would violate the parallel-trends assumption required for identification. By comparing economic outcomes in groups of neighboring counties straddling a state border, we can eliminate (or at least reduce) the biasing effects of unobserved local

variation in economic conditions that might correlate with the tax change.

Our sample of corporate income tax changes consists of 140 tax increases and 131 tax cuts in 45 states (including DC) affecting a total of 3,390 border county-years going back to 1969. Using these tax changes, we find that the effect of corporate tax changes is asymmetric: tax increases hurt employment and income in treated counties whereas tax cuts have little effect.

Our point estimates suggest that all else equal, a one percentage-point increase in the top marginal corporate income tax rate reduces employment by between 0.3% and 0.5% and income by between 0.3% and 0.6%, measured relative to neighboring counties on the other side of the state border. These estimates are remarkably stable: they remain essentially unchanged regardless of local characteristics such as the flexibility of local labor markets, income levels, population density, or the prevalence of small businesses in a county. They are also stable across the business cycle and little changed when we control for localized industry-level shocks by comparing employment and income in bordering counties within the same industry.

While tax increases are uniformly harmful, tax cuts have, in general, no significant effect on either employment or income. The one exception is when tax cuts are implemented during a recession. In this case, tax cuts lead to sizeable increases in both employment and income. This finding suggests that corporate tax cuts, when used counter-cyclically, can be an effective policy tool if government desires to stimulate employment and income during economic downturns.

The identifying assumption that is central to a causal interpretation of the effects of corporate taxes on economic activity is that contiguous border counties share similar unobservable local economic conditions and trends.² This seems plausible, given that counties “trade” goods, services, and labor with each other across state borders and have similar exposures to external shocks to economic activity, such as extreme weather or changes in federal spending priorities.

But the fact that the economies of border counties are plausibly economically integrated also

² Examples of prior studies implementing the border-county methodology in other contexts are Holmes (1998), Huang (2008), Dube, Lester, and Reich (2010), Hagedorn et al. (2015), and Heider and Ljungqvist (2015).

raises an identification concern: tax changes on one side of the border could trigger changes in the behavior of either firms or households across the border. For example, since a tax increase reduces income, this could spill over to control counties if affected households spend less money not just at home but also in neighboring counties. This would attenuate the estimated treatment effects, as the tax increase would hurt both the treated and the control county. Changes in corporate behavior, on the other hand, could amplify the estimated effects. For example, firms located in treated counties might shift operations to a control county to avoid a tax increase. This would cause economic activity to rise in control counties while falling in treated counties.

Whether such spillovers are likely to be present, and if so, whether they attenuate or amplify our estimates, can be tested. We find no evidence of firms shifting operations across state borders in response to state-level tax changes. This suggests that firms face fixed adjustment costs that are sufficiently large to militate against a move. In turn, this implies that our estimates are not amplified. When we consider households' spending decisions, we similarly find no evidence that our baseline estimates of the effects of tax changes are attenuated by cross-border spillovers. This absence of spillovers suggests that bordering counties provide an appropriate counterfactual for how economic conditions would have evolved in treated counties had they not been subject to tax changes, supporting a causal interpretation of our estimates.

Our paper contributes to the literature on the effects of government spending and tax policies on output and growth, reviewed in the next section, by measuring the sensitivity of employment and income to changes in corporate tax rates. We present the novel finding that the effects of tax changes are asymmetric: while tax increases are consistently harmful, tax cuts fail to stimulate growth in income and employment, except in recessions. One interpretation of the latter finding is that policymakers seeking to bolster economic growth through reform of the corporate tax code may need to consider tax cuts of a magnitude greater than that observed in our data.

The strength of our empirical approach is its internal validity: as long as treated border

counties and their neighbors share similar trends in employment and income absent a tax change, we can interpret our findings as being caused by the tax changes.

More challenging is the question of external validity: how might employment and income respond if the federal government changed corporate income taxes? Extrapolating from our state-level estimates to the federal level would require a structural model of the macro economy. Such a model would need to capture interactions and feedback effects among various economic conditions and policies that are effectively held constant in our local comparison of border-county pairs. For example, a federal tax change may prompt the Federal Reserve to change monetary policy with knock-on effects on long-term interest rates, inflation expectations, and exchange rates. While interesting, developing such a model is beyond the scope of this paper.

1. Related Literature

As Romer and Romer (2010) note, “The literature examining the effects of changes in the level of taxes on output is relatively small.” To put our contribution in context, we briefly discuss prior empirical studies that focus on overcoming the endogeneity of tax policy and economic outcomes. These fall into two broad camps: those using what has come to be called the narrative approach and those using structural vector autoregressions (SVARs).

Romer and Romer (2010) are the leading proponents of the narrative approach. They propose to address identification and endogeneity concerns by searching the narrative record of Presidential speeches and Congressional reports since World War II for changes in federal tax law deemed to be “more exogenous.”³ These include tax increases to deal with an inherited budget deficit and tax cuts aimed at boosting long-term growth. They stand in contrast to fiscal measures apparently influenced by current or anticipated economic events, such as those intended to counteract the immediate effects of a recession. Romer and Romer estimate that

³ This builds on the authors’ earlier work examining monetary policy in Romer and Romer (1989). The narrative approach has also been applied to investigating the effects of government spending shocks. Specifically, Ramey and Shapiro (1998) and Ramey (2011) use news reports to identify changes in government spending that are arguably unrelated to prevailing economic conditions, such as military build-ups. See also Barro and Redlick (2011).

changes in total federal taxes on all categories of income have a large effect on output. A tax increase (cut) equal to 1% of GDP, for example, reduces (raises) output by nearly 3%.

The narrative approach is not universally popular. Leeper (1997), Bernanke and Mihov (1998), and Sims (2010), among others, raise the concern that the policy changes thus identified may not truly be exogenous. For example, even though inherited budget deficits reflect previous tax and spending decisions, the government purchases that those budget deficits financed may have lingering or delayed consequences.

An alternative approach to evaluating the impact of taxes on aggregate economic activity is to use SVARs. Identification assumes either timing lags in the policy responses, as in Blanchard and Perotti (2002), or sign restrictions motivated by economic theory, as in Mountford and Uhlig (2009). However, SVAR estimates tend to be sensitive to even modest changes in identifying assumptions and risk being biased due to omitted variables.⁴

Mertens and Ravn (2013) combine the narrative and SVAR approaches by using Romer and Romer's (2010) narrative tax changes as instruments in an SVAR framework. Their main finding is that a one-percentage-point cut in their measure of the corporate income tax rate (the ratio of aggregate receipts of federal taxes on corporate profits to aggregate corporate profits) raises real GDP by between 0.4% and 0.6% but has no immediate effect on employment or hours worked.

Both the narrative and SVAR approaches focus on the macroeconomic level by analyzing aggregate time series data. One shortcoming of this focus is that it lacks a counterfactual: with nothing to compare the realized output change to, it is impossible to know how output might have changed in the absence of the tax change. In contrast, the advantage of our diff-in-diff framework is that realized output changes can be compared to a plausible counterfactual. This allows us to control for contemporaneous variation in otherwise unobservable economic conditions and so helps isolate the change in output that is due to the tax change alone.

⁴ For a general overview of the identification challenges associated with SVARs, see Stock and Watson (2001).

While we are the first to study the effects of corporate taxes on employment and income using state-level variation, our approach relates to other studies investigating the effects of state tax policies. In contemporaneous work, Suarez Serrato and Zidar (2015) rely on state-level corporate tax changes to structurally estimate a spatial equilibrium model of firm and worker location choices. Earlier work has studied the effects of state-level personal (as opposed to corporate) income taxes on economic activity. Helms (1985) and Mofidi and Stone (1990) conclude that higher personal income taxes are detrimental when the extra tax revenue is used to fund transfer payments (rather than, say, improve the state's infrastructure), though neither study attempts to isolate the causal effects of taxes on economic activity.⁵

Most prior work looks not at changes in tax *rates* but at changes in tax revenue scaled by GDP or income. While understandable where better data are not available, using tax revenue to proxy for tax rates seems problematic: tax revenue is a function of economic activity even in the absence of any change in tax rates. For this reason, we focus on changes in tax rates.

Methodologically, our paper belongs to a growing literature that uses difference-in-difference or instrumental-variables estimators to study the effects of fiscal policy at the state level. Recent examples include Shoag (2015), Fishback and Kachanovskaya (2010), Chodorow-Reich et al. (2012), Clemens and Miran (2012), and Nakamura and Steinsson (2014). The focus in these papers is on the real economic effects of government spending. Our focus on the real economic effects of corporate taxes thus adds a new and important dimension to this emerging literature.

2. Empirical Strategy

Our aim is to measure the causal effects of corporate income taxes on employment and income. To recap, the identification challenges are twofold. First, tax changes likely reflect unobserved changes in economic conditions. A naïve regression of ex post economic outcomes

⁵ Kneller, Bleaney, and Gemmell (1999) reach a similar conclusion using cross-country data from a panel of 22 OECD countries. Other papers that study the effects of corporate taxes using cross-country correlations include Lee and Gordon (2005) and Djankov et al. (2010).

on changes in tax policy thus likely results in biased estimates, since the same omitted variables that trigger the tax change may also affect economic conditions. Second, even if a particular tax change truly was random, we do not observe how the economy would have performed in the absence of the tax change. Some of the outcome “response” may have happened anyway and so may not be a result of the tax change.

The following example illustrates the problem. The 1986 Tax Reform Act cut the federal corporate income tax rate from 46% to 34%. Around this time, real GDP rose by 4.2% in 1985, 3.5% in 1986, and 3.5% in 1987; the unemployment rate was 7% in 1985, 6.6% in 1986, 5.7% in 1987. Whether any of these changes in output or employment were caused by the tax cut is hard to say. First, they could have been driven by something else changing in 1986, such as changes in inflation, productivity growth, world trade, etc. That is, we cannot rule out the presence of omitted variables. Second, we do not know how the U.S. economy would have fared absent the Tax Reform Act. That is, we have no benchmark against which to measure the observed changes in economic performance: we lack a counterfactual.

Our empirical strategy seeks to overcome these challenges by using variation in state corporate income tax rates that are staggered across U.S. states and time (which provides a set of plausible counterfactuals) and by narrowing in on contiguous counties straddling a state border (which allows us to hold unobserved variation in economic conditions constant). Our strategy is thus essentially a standard difference-in-differences approach that exploits a spatial discontinuity in tax policy for identification purposes.

The focus on border counties rather than adjacent states has two main advantages. First, while state tax changes are likely to be endogenous with respect to the state in question, they are plausibly exogenous from the perspective of an individual county.⁶ Second, economic conditions

⁶ This identifying assumption is standard in studies implementing a cross-border discontinuity research design. In principle, large or otherwise economically important counties may have more influence over policy decisions than others. As we will show, our results are robust to excluding such counties from the estimation.

are likely to be more homogeneous in a sufficiently narrow geographic region either side of the state border than between one state and another. The more similar are the economic conditions, the easier it to difference away the confounding effects of unobserved local economic conditions.

To illustrate these advantages, consider the possibility that tax rates change for reasons that correlate with the business cycle. For example, when its economic conditions deteriorate, a state raises corporate taxes (to balance its budget) and – independently – employment and income decline. This would violate the parallel-trends assumption required for identification in a difference-in-differences test such as ours, in the sense that treated and control state would be on diverging trends *regardless of treatment*. The control state would then not provide a plausible counterfactual. Instead of parallel trends at the state level, we assume parallel trends in pairs of contiguous border counties. This assumption is plausible as long as border counties experience similar variation in local economic conditions; that is, as long as conditions, unlike tax policy, vary smoothly across state borders. If so, we can use counties on one side of the border to establish a counterfactual outcome for treated counties on the other side.

The main remaining identification concern, given our empirical approach, is that tax changes on one side of the border could trigger changes in the behavior of either firms or households on the other side of the border such that the economic trend in the control counties changes. This would violate the parallel-trends assumption. We investigate this possibility at length in Section 5.8. In short, we find no evidence of such cross-border spillovers in our setting.

2.1 Regression Specifications

Our empirical models examine the effects of changes in states' corporate income tax rates on employment and pre-tax wage income in a border county using a difference-in-differences regression specification of the following general form:

$$\Delta Y_{c,s,t} = \lambda \Delta T_{s,t} + \delta \Delta X_{c,s,t} + \alpha_{g,t} + \varepsilon_{c,s,t} \quad (1)$$

where c , s , and t index counties, states, and time respectively; Δ is the first-difference operator;

$Y_{c,s,t}$ is the outcome variable of interest; $\Delta T_{s,t}$ measures the magnitude of a change in state corporate tax rates; $X_{c,s,t}$ is a set of control variables; $\alpha_{g,t}$ are border-county-group/year fixed effects, with subscript g referencing a group of contiguous border counties as detailed in the next section; and $\varepsilon_{c,s,t}$ is the usual error term. To allow for asymmetry in the effect of tax changes, we also estimate a version of equation (1) that distinguishes between tax increases and tax cuts:

$$\Delta Y_{c,s,t} = \beta \Delta T_{s,t}^+ + \gamma \Delta T_{s,t}^- + \delta \Delta X_{c,s,t} + \alpha_{g,t} + \varepsilon_{c,s,t} \quad (2)$$

where $\Delta T_{s,t}^+$ and $\Delta T_{s,t}^-$ measure the magnitudes of tax increases and tax cuts, respectively.

First-differencing removes unobserved county- and state-specific fixed effects in the corresponding levels equation and, unlike a levels specification with county fixed effects, can easily accommodate repeated treatments (i.e., a county experiencing a sequence of tax increases or tax cuts over its time in the panel), treatment reversals (i.e., a tax increase followed some time later by a tax cut, or vice versa), and asymmetry in the response to tax cuts and tax increases.

The inclusion of $\alpha_{g,t}$ in equations (1) and (2) is critical, as it ensures that the effect of tax changes on treated counties is always measured relative to bordering control counties that do not experience a tax change. In this sense, $\alpha_{g,t}$ absorbs any confounding local economic shocks that are otherwise unobservable and so aids a causal interpretation of our results.

Tax changes are coded such that they are effective from the beginning of the calendar year, while outcome variables are measured as of the end of the calendar year. This coding convention ensures that outcome variables can plausibly be affected by the tax changes.⁷

To evaluate the dynamics of the responses of employment and income to corporate tax changes, we estimate the following dynamic specification,

$$\Delta Y_{c,s,t} = \sum_{i=-3}^2 \beta_i \Delta T_{s,t-i}^+ + \sum_{i=-3}^2 \gamma_i \Delta T_{s,t-i}^- + \delta \Delta X_{c,s,t} + \alpha_{g,t} + \varepsilon_{c,s,t} \quad (3)$$

⁷ In a small minority of instances, tax changes come into effect midway through the calendar year, rather than at the beginning. Nevertheless, the tax change occurs in the relevant calendar year and is coded as such.

in a six-year window around the tax change. This specification tests the parallel-trends assumption required for identification by comparing outcome variables in treated and control counties over a three-year period before a tax change. By symmetry, we estimate the impact responses to tax changes over the next three years (the year of the tax change, $t=0$, and the next two years) to allow sufficient time for potential reversals or delayed responses to occur.

2.2 Treatments and Controls

We estimate equations (1) – (3) in a sample that includes only years in which one of the counties in a contiguous border-county group experiences a tax change. Years without a tax change in a contiguous border-county group are excluded from the estimation sample, as they do not contribute to the identification of the effect of tax changes and so are superfluous. Over time, a treated county can itself become a control county if sufficient time has elapsed since it last experienced a tax change.⁸ Appendix A provides further details of our coding choices.

There are 3,132 counties in the U.S.⁹ Of these, 943 are “treated” in the sense that they lie on the border of a state that changes corporate taxes between 1970 and 2010. Since a state can change corporate taxes multiple times, we have a total of 3,390 treated county-years. These are matched to 3,650 control county-years covering 978 distinct counties located on the border of an adjoining state. (We have more controls than treated because the average treated county borders more than one control county.) All results are robust to dropping counties covering large geographic areas, common in the western part of the U.S., to address the concern that large counties do not adequately control for “local” economic conditions.

2.3 Standard Errors

Standard errors are clustered at the state level to allow for serial correlation in a county over

⁸ To be eligible to serve as a control, a county must not itself have been subject to a corporate income tax change for at least four years. This ensures that a county has “recovered” from the effects of a tax change (if any) and so has regained its trend growth rates in employment and income. Our results are not sensitive to the choice of cutoff.

⁹ This includes county-equivalents (such as Louisiana’s parishes) except in Virginia, whose independent cities the Bureau of Economic Analysis combines with the nearest county.

time and, importantly, for arbitrary correlations of the error term across counties belonging to the same state in any given year as well as over time. We validate the standard errors using randomly generated “pseudo” tax changes that did not in fact happen and so cannot have any effect on economic activity.¹⁰ The simulations show that the probability of finding results resembling ours when in fact corporate tax changes have no effect on economic activity (the simulated null) is very small. For our variables of interest, the simulated data falsely reject the true simulated null at the 1% level exactly 1% of the time, suggesting that our tests have perfect size. Moreover, in none of the simulations do we find coefficients as large as those found in the real data.

3. State Corporate Income Taxes

Most states tax corporate activities within their borders,¹¹ usually by taxing profits.¹² A firm’s state of incorporation (often, Delaware) is irrelevant for state tax purposes. Instead, firms are taxed in every state they have “nexus” with, i.e., where they have sales, employees, or property. This means that a corporate income tax change in state A will affect all taxable corporate economic activity in that state, regardless of where the firms active in state A are themselves incorporated or headquartered. This makes changes in state corporate income taxes a clean shock to economic activity in the state.¹³

State taxes are a meaningful part of corporations’ overall tax burden. Heider and Ljungqvist (2015) estimate that over the period from 1989 to 2011, state taxes account for 21% (13.7%) of total income taxes paid for the average (median) publicly traded firm in the U.S.

We compile data on state-level corporate income tax rates from several sources. For the years

¹⁰ Following Bertrand, Duflo, and Mullainathan (2004), we randomly generate 1,000 sets of pseudo tax changes. For each iteration, the number of tax increases and tax cuts matches the number actually observed in the sample. We confine these pseudo shocks to those states that experience a corporate income tax change during the sample period.

¹¹ In 1969, eight states did not tax corporate activities (FL, NH, NV, OH, SD, TX, WA, and WY). By 2013, five of these had introduced corporate taxes, leaving only NV, SD, and WY without corporate taxes.

¹² The exceptions, as of 2013, are OH, TX, and WA, which use a gross receipts tax assessed on revenue rather than on income.

¹³ Not all economic activity is carried out inside entities subject to state corporate income tax. Other business forms such as sole proprietors and most LLCs, S-Corps, and partnerships are taxed at the personal (rather than corporate) level. Unobserved heterogeneity in the business-form mix in a treated county will bias us against finding that corporate tax changes affect economic activity.

1969 to 1988, we collect data from the *State Tax Handbook*, published annually by *Commerce Clearing House*. For 1989-2011, we use the information listed in Appendices A and B of Heider and Ljungqvist (2015). For 2012-2013, we use data from the Tax Foundation. Throughout the analysis, we focus on changes in the top statutory marginal tax rate. This approach is appropriate in our context as states either have a flat corporate tax rate structure or tend to charge the top rate on even relatively low levels of income (say, \$25,000).¹⁴

3.1 Variation in State Corporate Income Taxes

There is substantial variation in state corporate income taxes over time. To illustrate general trends, Figure 1 plots the average tax rate over time, along with bands representing the 20th to 80th percentile range. Averaged across the states, tax rates increased from 4.9% in 1969 to a high of 7.2% in 1991 and have since fallen to 6.7%. Only seven states have lower tax rates in 2013 than they did in 1969; 36 have higher tax rates. The bands in Figure 1 show that the inter-state variation in tax rates has narrowed over time, from a five-percentage-point difference between the 20th and 80th percentile in 1969 to a 3.45-point difference in 2013.

The time variation in tax rates is sufficiently large to affect a state's position in tax league tables. For example, in 1969 Indiana, Missouri, and Nebraska charged the lowest rate in the nation (2%), while Minnesota's rate of 11.33% was the highest. By 2013, Indiana, Missouri, and Nebraska had moved into the middle of the pack and Minnesota's rate had fallen to 9.8%, below DC's (9.975%), Pennsylvania's (9.99%), and Iowa's (12%). Colorado's rate of 4.63% is currently the lowest rate among states charging corporate income tax.

We identify 140 tax increases in 45 states (including DC) and 131 tax cuts in 35 states over the 45-year period from 1969 to 2013.¹⁵ In any given year, on average 6.1% of states increase

¹⁴ In 2010, the highest corporate income tax bracket sets in at no more than \$100,000 in 38 of the 44 states (including DC) that impose a corporate income tax.

¹⁵ We ignore a small number of cases involving states that switched from a tax on income to a tax on revenue or vice versa, as the effect of such changes on economic activity is ambiguous ex ante. The most prominent examples are Michigan, Ohio, and Texas.

taxes ($=140/(45 \text{ years} * 51 \text{ states and DC})$) and 5.7% of states cut taxes ($=131/(45 * 51)$). Table 1, Panel A reports summary statistics. The mean tax increase measures 126 basis points, a 20.6% increase from a year earlier. The median tax increase is 100 basis points and the standard deviation is 113 basis points. The mean tax cut measures 65 basis points, a 7.7% reduction from a year earlier. The median tax cut is 48 basis points and the standard deviation is 71 basis points.

Figure 2 shows the distribution of corporate income tax changes across states and time. The pace of tax increases has slowed over time. There were 57 tax increases in the 1970s, 44 in the 1980s, 23 in the 1990s, 13 in the 2000s, and 3 since 2010. Tax cuts follow a hump-shaped pattern, with 12 tax cuts in the 1970s, 34 in the 1980s, 45 in the 1990s, 24 in the 2000s, and 16 since 2010. Figure 2 reveals little geographical clustering for either tax increases or tax cuts.¹⁶

3.2 Why Do State Taxes Change?

Clearly, states do not change tax rates randomly. Table A1 in Appendix B shows that the main political economy determinants of a state's decision to raise or cut its corporate income tax rate are the state's budget balance, changes in its bond ratings, the local unemployment rate, and tax competition with nearby states. These patterns suggest that states that change their corporate tax rates differ on several important dimensions from other states. In turn, this underscores the need to control for variation in local economic conditions as finely as possible if we want to estimate the *causal* effect of corporate tax changes.

While our empirical strategy is designed to do just that, one might still be concerned about the possibility of reverse causality. For example, it could be the case that states that are already experiencing economic downturns and declining tax revenues increase corporate taxes to balance their budgets. This could exacerbate the downturn, even relative to nearby states facing similar

¹⁶ Of the 140 tax increases, 103 are geographically isolated cases. The remaining 37 form the following 15 geographic clusters: KS/NE (1970), NH/VT (1970), IA/MO/TN (1971), ND/MN/WI (1972), DC/VA (1972), NJ/NY/CT (1975), MA/NY (1976), NE/IA/WI (1982), CT/RI (1983), ME/NH (1983), ID/UT/NM (1983), OH/WV (1983), NM/CO/UT (1986), IL/KY (1989) and MO/NE/OK (1990). Similarly, of the 131 tax cuts, 101 are isolated cases. The remaining 30 form the following 14 groups: IL/WI (1984), PA/WV (1985), NY/PA/OH (1987), CO/UT (1988), OH/WV (1988), AZ/CO (1990), CO/NE (1993), ME/NH (1993), NJ/PA (1994), NY/CT (1999), AZ/CO (2000), NY/CT (2000), KY/OH (2005), and NY/VT (2007).

economic conditions. As Meyer (1995) notes, the standard way to address reverse causality concerns in the context of natural experiments such as ours is to perform Granger causality tests. These reveal no evidence of reverse causality in our setting; see Table A2 in Appendix B.

3.3 Coincident State-level Policy Changes

Like every state-level diff-in-diff, our identification strategy invites an important potential concern: corporate income tax changes may systematically coincide with other state-level policy changes that affect economic activity. This would confound our estimates by making it difficult to isolate the effects of corporate taxes as distinct from the effects of other policy programs. To see if this is the case, we collect data on state-level policies that are likely to impact economic activity and so have the potential to confound our estimates of the effects of corporate income tax changes. Specifically, we collect information on the initiation of state incentive programs providing tax credits for either investment, R&D, or job creation and on changes in state taxes on bank profits (which Smolyansky (2014) shows affect credit supply and real economic activity).

According to Table 1, Panel B, very few of the state corporate income tax changes in our sample coincide with the initiation of a state incentive program, or with a change in such a program, so it is unlikely that our results are contaminated by the effects of these programs. The overlap with state taxes on bank profits is more substantial, though as we will show, our results are robust to excluding corporate tax changes that coincide with such state-level policy changes.

State corporate income tax changes may trigger changes in government expenditure, which could in turn affect economic conditions in treated counties independent of the tax changes. In practice, this is unlikely to confound our findings concern because receipts from corporate taxes account for a very small share of state government revenues. For example, corporate taxes account for only around 5% of state government revenues in 2013.¹⁷ A similar point is made by Suarez Serrato and Zidar (2015).

¹⁷ See Figure 1 in <http://www2.census.gov/govs/statetax/2013stcreport.pdf>.

3.4 Tax Apportionment

Firms are not required to compute how much of their profit is earned in each of the states they operate in. Instead, states proxy for the profit a firm earns in a state by multiplying the firm's total taxable income by an apportionment formula.¹⁸ Apportionment formulae differ across states. As of 2012, thirteen states use a single-factor formula based on sales, apportioning a firm's profit to the state based on the ratio of the firm's in-state sales to its total sales. Thirty-three states use a three-factor formula based on a weighted average of the proportions of sales, property, and employees attributable to the state. Of these, 22 states overweight the sales factor.

Given our diff-in-diff setup, this kind of variation in apportionment formulae across states is easily differenced away and so cannot confound our results. But there are two other ways in which apportionment rules can potentially affect our results.

First, apportionment rules can induce heterogeneous treatment effects. To illustrate, note that apportionment implies that a firm earning \$5 million and operating only in, say, New Jersey will pay the same amount of tax there as a multi-state firm with \$5 million in profit apportioned to New Jersey. If the apportionment formula over- or under-apportions true New Jersey profits, multi-state and single-state firms may respond differently to a New Jersey tax change. For example, multi-state firms benefitting from under-apportionment would respond less strongly than single-state firms. If counties in our sample differ systematically in their mix of single- and multi-state firms, heterogeneous treatment effects could result. While data on a county's mix of single- and multi-state firms are unavailable, we use a close proxy, namely a county's small-business employment share, and find our results to be robust. Similarly, one might expect variation of this type across industries, but our results hold even within industry.

Second, it is possible that state tax rate changes coincide with changes in apportionment rules in the state. This could either lessen or amplify the response to tax rate changes, depending on

¹⁸ Note that firms do not apportion their costs among states, which reduces incentives to engage in transfer pricing.

the signs of the changes. For example, a tax increase that coincides with tougher apportionment rules would be expected to result in a larger response than a tax increase that coincides with more generous apportionment rules. In practice, states virtually never change apportionment rules when they change tax rates: as Table 1, Panel B shows, only five of the 140 tax increases and five of the 131 tax cuts in our sample coincide with changes in apportionment rules.

4. Sample and Data

4.1 Outcome Variables

The Regional Economic Accounts of the Bureau of Economic Analysis (BEA) report annual data for various definitions of employment and income for each of the 3,132 counties in the U.S. from 1969 to 2010. For all variable definitions and details of their construction, see Appendix C.

Our baseline employment measure is the number of full-time and part-time jobs in a county scaled by county population.¹⁹ Employment is measured by the location of the place of work, not by the worker's place of residence, and so includes workers commuting in from other counties.

Income is measured as a county's aggregate annual earnings from wage employment (in the form of wages or salaries and including non-wage disbursements), scaled by county population. The BEA measures income *before* tax and excludes income received in the form of dividends, interest, or rent, which helps rule out a mechanical relation between changes in corporate taxes and income. Income is reported either by place of work or by place of residence (the difference being due to commuters) and either with or without government transfers. Our baseline income measure is earnings by place of work excluding government transfers. We consider the impact of commuting and government transfers separately.

The self-employed are not subject to corporate taxation (their income is typically taxed as personal income) and so constitute a useful placebo group whose response to corporate tax changes we analyze separately. Accordingly, our baseline employment and income measures

¹⁹ This measure reflects the extensive margin of employment: whether or not the average county resident has a job. Data on the intensive margin – hours worked conditional on having a job – are not available at the county level.

exclude the self-employed.

4.2 Pre-Trends and Unobserved Local Economic Conditions

Table 2, Panel A provides summary statistics of our dependent variables for all treated and control border counties and for those counties located in the interior of treated and control states, measured the year before a tax change. Employment and income *levels* differ significantly between treated and control border counties in the year before a tax change. Employment in treated counties averages 35% of county population, significantly below the mean of 36.2% in control counties ($p < 0.001$). Per capita income is also significantly lower, by \$423 ($p = 0.021$), in treated border counties, where it averages \$11,271 per county resident (in 2005 dollars).²⁰

For identification purposes, what is more important than differences in levels is differences in growth rates. Identification relies on the parallel-trends assumption, which in our setting means that employment and income should grow at the same rates in treated and control border counties in the absence of a corporate tax change. Panel B of Table 2 tests the parallel-trends assumption by comparing growth rates over the year before a tax change. As Panel B shows, for each variable of interest, the growth rates in treated and control border counties prior to the tax change are virtually identical and never statistically different from each other. This supports the parallel-trends assumption. Moreover, since we find significant differences in employment and income levels, but not in growth rates, it is essential to remove time-invariant heterogeneity in these variables across counties. The first difference of our difference-in-differences approach accomplishes this by removing the county fixed effect from all regression specifications.

Table 2 also shows that there are significant differences in pre-tax-change growth rates between border counties and counties located in a state's interior. This is true for both treated states and their neighbors. For example, in the year before a tax change, employment grows on

²⁰ This may seem low, but recall that we are scaling wage income by total county population. Moreover, this is an equally weighted average. Thus, Manhattan, the richest county in the U.S. in 2010 with roughly 1.6 million residents, receives the same weight as sparsely populated Buffalo County in South Dakota, the poorest county in the U.S. in 2010 with only 1,912 residents.

average by 0.407% and 0.7% in border and interior counties located in treated states, respectively – a difference in trends that is highly significant ($p < 0.001$). Thus, while the parallel-trends assumption holds for bordering counties in adjacent states, it does not necessarily hold for border and interior counties within the same state. This underlines the importance of using border counties, rather than neighboring states, as controls. On average, the distance between border and interior counties in a given state is much greater than the distance between contiguous border counties. The fact that growth rates in employment and income can vary significantly, even within the same state, shows how important it is to control for local economic conditions. By comparing economic outcomes in groups of adjacent counties that straddle a state border, this is precisely the concern that our empirical approach is designed to address.

4.3 Control Variables

Throughout, we control for the following county-level demographic characteristics: the fractions of the county's residents who are aged over 25 and have a college degree; live in a rural area; are white; are aged under 21; are aged over 65; reside in owner-occupied residences; or live in single-mother households.

5. The Effects of Corporate Incomes Tax Changes on Employment and Income

5.1 Employment

Table 3 presents estimates of the effect of corporate tax on employment. Column 1 shows that changes in corporate income taxes have a negative and statistically significant effect on wage employment, controlling both for observed changes in county demographics and for unobserved variation in local economic conditions using contiguous-border-county-group/year fixed effects ($\alpha_{g,t}$ in equation (1)).²¹ Economically, a one percentage-point increase (cut) in the

²¹ Though not shown to conserve space, only one of the demographic controls is significant at the 5% level: the change in the fraction of county residents under the age of 21 is negatively associated with changes in employment. However, the inclusion of demographic controls affects neither the point estimates nor the significance of our results, suggesting that state tax changes do not coincide with material changes in a border county's demographic characteristics that would have an independent bearing on the county's economic outcomes.

corporate tax rate reduces (raises) wage employment in the treated county by 0.241% ($p < 0.001$) all else equal, measured relative to neighboring counties just the other side of the state border whose corporate tax rates remain unchanged.

How important is it to remove unobserved variation in local economic conditions? Column 2 reports a specification that omits the border-county-group/year fixed effects.²² This doubles the estimated tax effect, to -0.481 ($p < 0.001$). Ignoring the effect of unobserved local variation would thus lead to an overestimate of the effect of corporate taxes on wage employment. This implies that the unobserved local variation goes in the same direction as the effect induced by the tax change (e.g., employment-reducing tax increases coincide with times when employment is already falling).

Columns 1 and 2 assume that the effect of corporate tax changes is symmetric. Column 3 relaxes this assumption. This reveals that the negative employment effect is driven entirely by tax increases: while tax cuts do not affect employment, either statistically ($p = 0.614$) or economically (with a point estimate of only 0.065), tax increases are associated with fewer jobs. The effect is significant, both statistically ($p < 0.001$) and economically: increasing the state's corporate tax rate by one percentage-point reduces employment by 0.282% in a treated county.²³

Our baseline employment measure scales the number of jobs by total county population. This means that the estimated effect of corporate tax increases on employment could potentially be driven by an increase in the birth rate or in the number of retirees moving to the county (perhaps because higher corporate tax revenue enables greater spending on schools or on public services attractive to the elderly). To investigate this possibility, column 4 scales employment by a measure of the size of the workforce instead (namely, the number of county residents aged between 20 and 70). The point estimate of -0.288 ($p < 0.001$) is virtually identical to that in

²² The sample size in this column is smaller than in column 1 as we remove duplicated control counties that are paired to more than one treated county (see Appendix A). This has no bearing on the estimates shown in column 2.

²³ The fact that state-level corporate tax cuts have no significant effect on employment recalls Mertens and Ravn's (2013) finding that narratively identified reductions in federal corporate tax revenues do not appear to affect jobs.

column 3, so our results are not confounded by changes in a county's age profile. For the remainder of the paper, we therefore continue to scale by total county population.

Our baseline employment measure includes government employees and so gives the gross effect of a corporate tax change: if a tax increase is used to fund an expansion of government services, this may lead to more government workers being hired, cushioning the reduction in private-sector employment. To isolate how tax changes affect private-sector employment, column 5 excludes government employees. Doing so makes little difference: the coefficient for tax increases of -0.289 ($p=0.002$) is essentially unchanged from our baseline, while the effect of tax cuts remains statistically and economically insignificant. This implies that the negative effect of tax increases is driven entirely by a reduction in private-sector employment; government employment has no offsetting effect on average.

Before we turn to income, we present a simple placebo test. Self-employment income tends to be taxed as personal income rather than corporate income. The self-employed should thus not be directly affected by corporate tax changes, though they could be affected indirectly by changes in aggregate county demand. Column 6 shows that corporate tax changes have no statistically significant effect on the number of self-employed. This finding indirectly validates our results: if corporate tax increases had systematically coincided with negative economic shocks specific to treated border counties, we should have found an effect among the self-employed. It also suggests that a primary channel through which corporate tax affects economic activity, at least in our setting, is through companies' demand for labor. Secondary effects, due to reductions in wage employment reducing aggregate demand to the potential detriment of all county residents, including the self employed, appear to be too modest to measure reliably.

5.2 Income

Table 4 presents estimates of the effect of corporate tax on income. Column 1 shows that changes in corporate income taxes affect per capita wage income negatively, controlling both for

observed changes in county demographics and for unobserved variation in local economic conditions.²⁴ Economically, a one percentage-point increase (cut) in the corporate tax rate reduces (raises) wage income in the treated county by 0.367% ($p < 0.001$), relative to counties just the other side of the state border whose corporate taxes are unchanged.

Column 2 omits the contiguous-border-county-group/year fixed effects, which increases the estimated tax effect to -0.477% ($p = 0.002$). Thus, as in the case of employment, ignoring the effect of unobserved local variation would result in an overestimate of the effect of corporate tax changes on per capita wage income. As before, this implies that the unobserved local variation goes in the same direction as the effect induced by the tax change (e.g., employment-reducing tax increases coincide with times when per capita wage income is already falling).

Column 3 shows that the effect is asymmetric: it is driven entirely by tax increases, just as it was for employment. Economically, a one-point increase in the corporate tax rate reduces per capita wage income by 0.42% ($p < 0.001$). Together with the negative effect of tax increases on employment in Table 3, this negative income response suggests that a primary channel through which tax increases harm economic activity is job losses (the extensive margin of employment). Job losses in turn reduce income levels. This effect is driven by the private sector: excluding income from government employment leaves the point estimate unchanged (see column 4).

In contrast, the effect of tax cuts on income is small (with a point estimate of 0.132 in column 3) and not statistically significant ($p = 0.455$).²⁵ The absence of an income effect indirectly rules out that tax cuts boost hours worked conditional on having a job (something that lack of data prevents us from estimating directly). If they did, we should find an increase in income following tax cuts. We thus conclude that tax cuts affect neither the extensive margin of employment (the

²⁴ None of the demographic controls is significant at the 5% level.

²⁵ A previous version of our paper included farm income in the baseline income measure and found that tax cuts had a large, positive, and statistically significant effect on per-capita income. Further investigation revealed that this finding is erroneous; it is due to an obscure quirk of the BEA data. As Appendix D explains, the BEA adjusts farmers' income for changes in the value of inventories. Farmers' income itself is not sensitive to corporate tax cuts.

number of jobs) nor the intensive margin (hours worked).

Column 5 adds government transfers such as unemployment insurance and welfare benefits to our baseline income measure. This slightly dampens the effect of corporate tax increases. The point estimate declines by about 25%, from -0.42 in column 3 to -0.307 in column 5, though it remains highly statistically significant ($p < 0.001$). The decline suggests that government transfers cushion the adverse effects of corporate tax increases to some extent and so act as automatic stabilizers, though the negative effects are far from totally offset.

Column 6 models per capita wage income by place of residence rather than by place of work (while ignoring government transfers). This should attenuate the effect of corporate tax changes, as residents include people who work for in-state firms (which are subject to the tax change) and people who commute to firms on the other side of the state border (which are not). As expected, the point estimate for tax increases declines to -0.247 , though it remains significant ($p < 0.001$).

Column 7 uses the self-employed as a placebo. Consistent with our earlier finding that the number of self-employed is unaffected by corporate tax changes, we find that neither increases nor cuts in corporate tax have a significant effect on the pre-tax income of the self-employed (who of course are not usually subject to corporate income tax). This reinforces the interpretation that a primary way corporate tax changes affect economic activity is through their effect on companies' demand for labor. Secondary effects, resulting from changes in aggregate demand in the county, appear too modest to affect the self-employed.

5.3 Robustness and Alternative Channels

One potential concern with our empirical strategy is that some counties may be large relative to the state as a whole and so may disproportionately influence state policy. A related concern is that even though treated and control border counties are neighbors, their geographic centers may in some instances be quite far apart. This would undermine the identifying assumption that they share similar local economic conditions (i.e., parallel trends) prior to a tax change.

Table 5 addresses these concerns. Columns 1 and 2 estimate the effects on employment and income in a subset of border counties that account for no more than 10% of a state's population, as measured in the year before a tax change. Removing potentially politically influential counties in this way has no effect on our results; the point estimates are nearly identical to our baseline estimates. Columns 3 and 4 restrict the sample to border-county pairs whose geographic midpoints are no more than 20 miles apart. This too leaves our results unchanged.

Table 6 presents further robustness checks. A natural interpretation of the results in Tables 3 and 4 is that firms reduce their labor demand as some activities cease to be economically viable at the higher tax rate. This would represent a deadweight cost of corporate taxes. But there is also another possibility: firms might be responding to a contemporaneous reduction in credit. To see why, note that banks too pay state taxes, albeit usually on a special schedule that only applies to banks. By taxing banks more, a state reduces the after-tax profit on every loan made. Because each state taxes every bank that lends to borrowers located in the state (regardless of the bank's own location), an increase in state taxes on banks could reduce the supply of credit available in the state. Firms might then be responding to a credit supply shock rather than a corporate tax increase occurring at the same time.

To separately identify a deadweight cost of corporate taxes, we need to hold state bank taxes constant. Fortunately, as Table 1 shows, our sample period contains plenty of corporate income tax changes that do not coincide with changes in state bank taxes. Using this subsample, columns 1 and 2 of Table 6 confirm that corporate tax increases lead to significant drops in employment and income even when they are not accompanied by increases in bank tax rates. The economic magnitudes of the effects are comparable to the overall sample: a one-percentage-point increase in the corporate tax rate results in a 0.332% fall in employment ($p=0.003$) and a 0.274% fall in income ($p=0.006$). This is consistent with corporate income taxes entailing deadweight costs.

In columns 3 and 4, we remove a small number of corporate tax changes that coincide with

the initiation of or a change in a state incentive program rewarding companies for undertaking either more investment or more R&D or for hiring more labor. This has no effect on our results – the point estimates are virtually identical to those in Tables 3 and 4 – suggesting that our findings are not driven by coincident changes in state tax incentives.

Columns 5 and 6 address the concern that treated and control counties may start off with very different tax rates. For example, in 1981 New Mexico increased its corporate tax rate from 5% to 6%. At that time, Texas, which borders New Mexico, had no corporate income tax. It is an open question whether Texas counties on the New Mexico border are good controls in this instance. We address this question by restricting the sample to border county pairs whose tax rates differ by at most 20 basis points two years prior to treatment. This restriction reduces the sample size by over 90%, yet if anything, our results become stronger: the estimated effect of a corporate tax increase on employment of -0.516% ($p=0.021$) is approximately 80% larger than our baseline estimate. In other words, firms' employment decisions appear more sensitive to corporate tax increases when tax conditions were initially very similar in both states than when they were very different. This translates into a more negative effect on income: the point estimate of -0.605% ($p=0.008$) is approximately 70% larger than in our baseline specification.

Columns 7 to 9 refine our identification strategy by controlling for local industry-level shocks. The BEA reports county-level employment and income broken down by industry. For employment, the finest industry breakdown is at the SIC1/NAICS2 level, while for income the finest breakdown is at the SIC2/NAICS3 level.²⁶ These data allow us to compare employment and income in bordering counties *within the same industry*, by including border-county-group/industry/year fixed effects. Doing so controls for time-varying localized shocks to a particular industry within a group of bordering counties. If such shocks were to systematically coincide

²⁶ The BEA's county/industry-level data transition from the SIC to NAICS industry classification systems in 2001. Because the industry breakdowns are too coarse to match SIC codes to NAICS codes using concordance tables, we drop observations for 2001 in our county-industry level analysis.

with corporate tax changes, they could be driving our results.

As columns 7 to 9 show, this is not the case. In column 7, we estimate that a one percentage-point increase in the corporate tax rate reduces employment by 0.184% ($p=0.024$), measured relative to employment in the same industry in bordering counties whose tax rates remain unchanged. This is a little smaller than the baseline estimate of 0.282%, but not significantly so. Tax cuts continue to have no significant effect on employment. Results for income are similar: whether we control for industry-level shocks at the SIC1/NAICS2 level (column 8) or at the finer SIC2/NAICS3 level (column 9), we find that tax increases reduce income by approximately 0.3% ($p=0.003$ and $p=0.019$, respectively), with tax cuts having no effect.

5.4 Impact Response Functions

To see how employment and income respond to corporate tax changes over time, Figure 3 summarizes the results from estimating equation (3) by plotting the implied impact responses for a one-percentage-point tax increase or tax cut for the three years following a corporate tax change becoming effective, along with 95% confidence bands.²⁷ As the figure shows, tax increases significantly reduce employment and income in the year they become effective ($t = 0$), with no subsequent reversion over the next two years.²⁸ Their effects are thus immediate and, over this time horizon, persistent. Corporate tax cuts, on the other hand, have no significant effect on employment or income at any point.

5.5 Business Cycle Effects

Our results so far show that corporate tax increases result in harm to the local economy by reducing employment and income while corporate tax cuts have no significant effect. We next consider how these results vary over the business cycle. Table 7 estimates the effects of increases

²⁷ Consistent with the validity of the parallel-trends assumption presented in Table 2, Panel B, none of the estimated pre-tax-change effects in equation (3) is statistically significant.

²⁸ Specifically, in response to a one-point tax increase, employment falls by 0.247% in the year of the tax increase ($p=0.002$) and by an insignificant 0.093% and 0.07% over the next two years, for a cumulative fall of 0.41% over three years ($p=0.014$). Income falls by 0.376% in the year of the tax increase ($p<0.001$) and by an insignificant 0.032% the next year before rising by an insignificant 0.035%, for a cumulative three-year fall of 0.373% ($p=0.067$).

and cuts in corporate tax rates separately in recessions and expansions, at the national level as dated by the NBER (columns 1 and 2), at the regional level using the BEA's eight economic regions (columns 3 and 4), and at the state level (columns 5 and 6). Regional and state recessions are coded based on whether real personal income has fallen for at least two quarters.

Table 7 reveals a striking pattern: while we saw previously that corporate tax cuts were ineffective on average, when implemented during a recession tax cuts significantly boost both employment and income. Employment increases by around 0.6% for every percentage-point cut in corporate tax rates implemented during a national, regional, or state recession, while income increases by around 1%. During expansions, on the other hand, tax cuts are ineffective.

The effects of corporate tax increases, by contrast, do not vary over the business cycle. Whether they occur during recessions or expansions, corporate tax increases consistently reduce employment and income and they do so by a similar amount.

5.6 Labor Market Flexibility

We next explore whether the effects of corporate tax changes vary with the flexibility of the state's labor markets. If tax increases harm employment and income by reducing firms' demand for labor, it is possible that a given tax increase has a larger (more harmful) effect in states whose unions are too weak to resist job cuts. To investigate this possibility, we classify states based on whether they have a right-to-work law in place and whether they are heavily unionized (i.e., whether their private-sector unionization rate is above or below the national median).²⁹

In either case, as Table 8 shows, tax increases lead to significant reductions in employment and incomes. The magnitudes are larger (more negative) in right-to-work states and less heavily unionized states, but not significantly so. Interestingly, tax cuts have small positive effects where unions are weaker and small negative effects where they are stronger, but none of these point estimates is statistically significant, mirroring our baseline findings. Thus, outside a recession,

²⁹ Right-to-work laws weaken unions by prohibiting closed shops, payments of union dues as a condition of employment, or other ways to discriminate against non-union workers (Ellwood and Fine (1987), Holmes (1998)).

tax cuts do not stimulate employment and income, not even in states with flexible labor markets.

5.7 County Characteristics

Finally, we investigate whether the effects of corporate tax changes vary with county income, population density, or the importance of small businesses to the local economy. For each of these characteristics, we divide counties into two groups based on the prior-year median. For example, we classify counties as “rich” or “poor” based on whether their per capita personal income is above or below the national median in the year prior to the tax change.

Table 9 presents the results. Increases in corporate tax rates uniformly lead to significant declines in employment and income, regardless of county income, population density, or the importance of small businesses (as measured by the share of the total county workforce that they employ). The point estimates are similar to those in our baseline regressions and not significantly different from each other. For tax cuts, all coefficient estimates are statistically insignificant.³⁰

5.8 Cross-Border Spillovers

If the economies of border counties are highly integrated with each other across state lines, our results could be confounded by cross-border spillovers between treated and control counties resulting from changes in the behavior of either firms or households. The direction and confounding effects of such spillovers, were they to occur, would depend on the direction of the tax change in the treated state and potentially on which state has the higher corporate tax rate.

5.8.1 Potential Spillover Effects of Corporate Tax Increases

Consider the case of a corporate tax increase. Our findings suggest that firms in treated counties reduce their demand for labor which in turn lowers county-level income. But it is an empirical question whether firms in control counties on the other side of the state border remain on their pre-treatment trend. They might not, for two reasons.

First, since households in the treated county have lower incomes, they may spend less not

³⁰ There is some indication that corporate tax cuts may raise income in poor counties by 0.428%, though this is imprecisely estimated ($p=0.143$).

just at home but in the wider local economy – including our control counties. This “household spending” channel would attenuate the estimated effects of corporate tax increases: the reduced levels of spending by treated households would spillover to control counties, thus potentially lowering employment and income there as well. Whether this channel is in operation can be tested. The spillover effect should be strongest close to the state border and then dissipate the further away from the border a county is located. If this is true, we expect to see systematic declines in employment and income in control border counties relative to counties located in the interior of the control state that are too far away from the border to be hurt by the spillover.³¹

Second, firms located in treated counties may shift some operations to a control county to mitigate the effects of the tax increase. Such “corporate spillovers” would amplify the estimated treatment effect: economic activity would fall in treated counties while rising in control counties. If so, this would lead us to overstate the effect of corporate tax increases on economic activity. To the extent that transport costs are an issue, corporate spillovers should affect border counties more strongly than counties in the interior of the control state. So a comparison of border and interior counties within a control state can shed light on both household and corporate spillovers. Moreover, corporate relocations may be most likely if the tax increase results in the treated state having a higher corporate tax rate than the neighboring control state, which also is testable.

5.8.2 Potential Spillover Effects of Corporate Tax Cuts

A possible reason why corporate tax cuts (outside recessions) have little impact is that their effects may dissipate across state borders. This would be the case if households increased their spending both at home and in neighboring control counties, which would raise employment and incomes in control counties and thereby attenuate the estimated treatment effects. Theoretically, it is also possible that firms located in control counties react to a tax cut in the treated county by

³¹ Recall that income and employment are measured by the location of work and so allow for the possibility that firms in the treated county hire not only fewer workers who live in the treated county but also fewer workers who commute in from a control county. Spillovers due to cross-border commuting can thus not confound our estimates.

shifting operations across the state line. But as this would amplify the estimated treatment effects, it is unlikely to explain the lack of sensitivity of employment and income to tax cuts.

5.8.3 Testing for Spillover Effects

To test whether firms or households in treated counties respond to corporate tax changes by altering their behavior in control counties across the border, we create a new estimation sample consisting of interior counties in control states which we compare to those border counties in the same control state that neighbor a treated state which increases (columns 1 and 3 of Table 10) or cuts its corporate tax rate (columns 2 and 4).³² The household spending channel implies that interior counties do better than border counties when taxes rise in the neighboring state and fare worse when taxes are cut. The corporate spillover channel implies the opposite.

The results show that there are no significant differences in either employment or income trends between interior and border counties in control states that neighbor tax-changing states. This is true for both tax increases and cuts. We therefore find no evidence that our estimates are systematically biased due to either household spending or corporate spillovers.

Columns 5 and 6 return to our baseline sample and allow the effect of tax changes to depend on whether the control county has a higher or lower tax rate than the treated county once the tax change has come into effect. As discussed, it seems plausible that corporate spillovers, if present, amplify the effect of tax changes more when the control state's tax rate is lower. The data reject this: employment and income decline significantly in response to corporate tax increases whether the tax rate is higher or lower in the control state. Importantly, the coefficients are not significantly different from each other. Thus, firms operating in a treated county do not appear to respond to a tax rise by shifting operations to a neighboring state with a lower tax rate.

Similarly, since the estimated coefficients for tax cuts in all cases remain insignificant, there is no evidence that firms operating in a control county shift operations to a neighboring state that

³² Some smaller states (e.g., Delaware and Rhode Island) and DC have no interior and so are omitted.

has cut its corporate tax rate below that of their home state. Our estimated treatment effects are therefore unlikely to be inflated due to firms moving operations across borders in response to tax changes. This suggests the presence of fixed adjustment costs of sufficient size to prevent firms from moving operations (a finding consistent with Suarez Serrato and Zidar (2015)).

In sum, the evidence suggests that cross-border spillovers from treated to control counties, due to either firms or households, are unlikely to be a concern in our setting. This suggests that control border counties on average remain on their pre-treatment trend even after a tax change in a neighboring state. They hence provide an appropriate set of counterfactuals against which to measure the economic effects of corporate tax changes in adjacent treated counties.

6. Conclusions

Against the background of the ongoing debate on corporate tax reform, we ask how corporate taxes affect employment and wages. Answering this question is empirically challenging. We propose an identification strategy that combines staggered changes in corporate income tax rates across U.S. states with a focus on contiguous counties straddling state borders. The resulting spatial discontinuity design permits a causal interpretation of our results by establishing a counterfactual against which to measure the effects of corporate tax changes and by eliminating omitted-variable biases resulting from the confounding influence of unobserved variation in local economic conditions. Effectively, our empirical approach exploits the fact that economic conditions, in contrast to state tax policy, do not respect state borders.

Our results suggest that increases in corporate tax rates are uniformly harmful for workers while corporate tax cuts are ineffectual in boosting economic activity unless implemented during recessions. The point estimates are fairly large: a one percentage-point increase in the corporate tax rate reduces employment by between 0.3% and 0.5% and income from employment by between 0.3% and 0.6%, all else equal. These estimates vary little over the business cycle and are remarkably stable across counties with different characteristics, such as labor market

flexibility, wealth, population density, or the prevalence of small businesses. Tax cuts, on the other hand, are only effective in recessions, when they raise employment by around 0.6% and income from employment by around 1% for every percentage point cut in the tax rate.

We end with two important caveats. First, a limitation of our empirical strategy is that county-level data on other economic quantities that could be affected by corporate taxes, such as investment and innovation, are unavailable. We can thus not rule out that corporate tax cuts boost the economy through some other channel than firms' demand for labor, nor do we necessarily capture all the ways in which corporate tax increases harm the economy. However, evidence from other studies suggests these effects may be relatively small. Asker, Farre-Mensa, and Ljungqvist (2015) show that stock market listed firms, which in 2010 accounted for 52.8% of aggregate non-residential fixed investment in the U.S., do not change their investment spending in response to changes in state corporate income tax rates. Similarly, Mukherjee, Singh, and Zaldokas (2014) find that corporate tax cuts have no effect on innovation as measured by patent applications filed by stock market listed firms.

Second, our findings should not be interpreted as evidence that a cut in the federal corporate income tax rate would have no beneficial effects (unless implemented in a recession). Some current policy proposals involve tax cuts of a magnitude that lies outside the observed range of tax changes in our data. We therefore hesitate to extrapolate from our findings to the potential effects of comprehensive tax reform at the federal level. We also cannot rule out that federal corporate income tax cuts could benefit the economy in other ways, say by boosting the returns to corporate investment, even if, like state tax changes, they ended up having no discernible effects on employment and wages.

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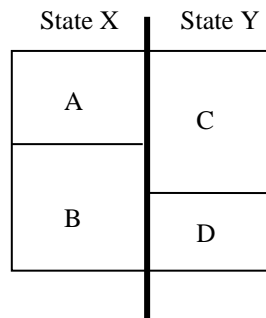
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Appendix A. Treatments and Controls.

Several issues require care when constructing the treatment and control groups. First, a county experiencing a tax shock is potentially a candidate to serve as a control later, when the neighboring county, which initially served as control, itself experiences a tax change. For example, in 1972 Virginia raised its corporate tax rate by one percentage point. In 1991, North Carolina, which borders Virginia, raised its rate by 1.06 percentage points. Although the Virginia counties on the North Carolina border were treated in 1972, arguably a sufficient amount of time has elapsed by 1991 to permit them to serve as controls. On the other hand, if hypothetically North Carolina had raised its rate in 1973, it would be doubtful whether the Virginian counties on its border are good controls, given that they have themselves only recently been subject to a tax shock. Thus, we adopt a conservative approach, requiring control counties not to have been subject to a tax change themselves for at least four years.³³

Second, a treated border county can neighbor multiple contiguous counties in the adjacent state. In the figure below, this is the case for county B in state X, which borders both counties C and D in state Y. We allow both counties C and D to serve as controls for county B when state X changes its tax rate.³⁴ That is, B, C, and D form a “group” of contiguous border counties, with B being treated and C and D serving as controls.



³³ There are 18 state-years in our sample that experience a tax change but have no valid control group based on our criteria. These are hence excluded from the analysis.

³⁴ This approach is standard in studies implementing a border discontinuity research design. See for example Dube, Lester, and Reich (2010).

The third complication is best explained with reference to county C in the above figure. County C is adjacent to both counties A and B. So if state X changes its taxes, county C would be in two separate contiguous-county groups: the control group for county A *and* the control group for county B. In terms of the regression specification, this means that observations for county C are duplicated in instances where state Y serves as a control for state X, such that counties A and C form one group of contiguous-border counties and counties B, C, and D form another. County C appears twice since it belongs to both groups.

The final issue arises if state Y is treated at some later time. In this case, the treated and control groups look quite different. For example, A, B, and C now form a contiguous border-county group, whereas before, when state X was treated, A was only in a group with C.

Appendix B. Why Do State Taxes Change?

Table A1 relates states' tax policies to their economic and political conditions. Columns 1 to 3 report summary statistics of our explanatory variables. (For all variable definitions and details of their construction, see Appendix C.) As expected, economic and political conditions play an important role in states' tax policies. Compared to states that cut taxes, those that raise taxes tend to run budget deficits (averaging 1.8%, compared to surpluses averaging 5.1% among tax-cutting states), are more likely to have suffered a credit-rating downgrade, and have a more unionized private-sector workforce (18.5% versus 15.4%). Growth in gross state product (GSP) and unemployment rates, on the other hand, are statistically similar in tax-cutting and tax-raising states.

Whatever their own political and economic circumstances, states' tax policies do not exist in a vacuum: their neighbors' tax policies also likely play a role. To capture the effects of tax competition among states, we compare each state's corporate tax rate to the rates levied by the states it borders. This reveals an interesting pattern. States that raise tax rates previously charged substantially lower taxes than their neighbors, while states that cut taxes were previously among the highest-taxing states in their region. To illustrate, before raising tax rates, the average state's rate is 2.4 percentage points below the highest rate levied by its neighbors. By contrast, before cutting taxes, the average state's tax rate is only 80 basis points below that of its highest-taxing neighbor. This suggests that states cut taxes when their rates are uncompetitive relative to their neighbors and feel freer to raise taxes when their neighbors levy higher taxes still, all else equal.

Next, we estimate linear-probability models to better understand states' decisions to raise or cut taxes. The models include year and state fixed effects and cluster the standard errors at the state level. The year fixed effects control for macroeconomic shocks common to all states, while the state fixed effects allow us to abstract from time-invariant heterogeneity across different states. Column 5 shows that tax rises are more likely the larger (more negative) the state's budget deficit the year before ($p=0.07$) and less likely if the state's tax rate is already high relative to its

neighbors' rates ($p < 0.001$). None of the other economic conditions has a significant effect, nor do Republican and Democratic governors differ in their propensity to raise taxes. On the other hand, Democratic governors are significantly less likely to *cut* taxes ($p = 0.008$, in column 6). Budget surpluses increase the likelihood of a tax cut ($p = 0.003$), as does higher GSP growth ($p = 0.012$) and a higher unemployment rate ($p = 0.01$). A ratings downgrade, on the other hand, reduces the likelihood of a tax cut by a sizeable amount (4.8 percentage points, $p = 0.013$). Tax competition also has a large economic effect: for every percentage point by which a state's tax rate exceeds that of its neighbors, the likelihood of a tax cut increases by 2.5 percentage points ($p < 0.001$) relative to the unconditional mean of 6.4%.

Columns 7 to 9 model the magnitude (rather than likelihood) of the tax changes, with similar results: the main factors are the state's budget balance, ratings downgrades, unemployment, and tax competition.

Table A2 performs a Granger test for reverse causality by regressing changes in state corporate income tax rates on lagged changes in state-level personal income and employment, including a set of region-year fixed effects for the BEA's eight economic regions to hold constant economic conditions among regional peers. None of the coefficients of interest is even remotely significant, suggesting that reverse causality is unlikely to be a concern in our setting.

Table A1. Determinants of State Corporate Income Tax Changes, 1986-2010.

We estimate the effect of political and economic conditions and of tax competition among states on the probability that a state changes its top marginal corporate income tax rate and the magnitude of any such change. Columns 1 to 3 report summary statistics of the explanatory variables, showing fractions or means (with standard deviations shown in italics underneath the means). Column 4 compares conditions in states that increase taxes to those in states that cut taxes. Columns 5 and 6 model the probability that a state raises or cuts corporate income taxes, using linear probability models. Columns 7 to 9 model the magnitude of the tax changes (measured in percentage points). For variable definitions and details of their construction, see Appendix C. All regression specifications are estimated using least squares with state and year fixed effects (not shown for brevity). The unit of observation in all columns is a state-year. The sample covers 50 states plus the District of Columbia over the period 1986-2010, for a total of 1,275 observations. In columns 5-9, heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Summary statistics			Difference (tax inc. – tax cut) (4)	Probability of ...		Magnitude of ...		
	all obs. (1)	tax increases (2)	tax cuts (3)		tax increase (5)	tax cut (6)	tax change (7)	tax increase (8)	tax cut (9)
Political conditions (in year $t-1$)									
=1 if Democratic governor	0.524	0.535	0.430	0.106	0.002 <i>0.011</i>	-0.033*** <i>0.012</i>	0.018 <i>0.020</i>	0.000 <i>0.016</i>	-0.018* <i>0.011</i>
Economic conditions (in year $t-1$)									
state budget balance	0.025 <i>0.075</i>	0.012 <i>0.080</i>	0.042 <i>0.108</i>	-0.030**			-0.547*** <i>0.199</i>		
state budget deficit	-0.013 <i>0.026</i>	-0.018 <i>0.033</i>			-0.458* <i>0.253</i>			-0.546* <i>0.317</i>	
state budget surplus	0.038 <i>0.064</i>		0.051 <i>0.101</i>			0.412*** <i>0.139</i>			0.348 <i>0.216</i>
=1 if state bond rating downgraded	0.043	0.081	0.008	0.073***	0.008 <i>0.034</i>	-0.048** <i>0.019</i>	0.040 <i>0.045</i>	0.000 <i>0.044</i>	-0.040*** <i>0.013</i>
GSP growth rate	0.063 <i>0.044</i>	0.071 <i>0.048</i>	0.063 <i>0.038</i>	0.008	0.332 <i>0.205</i>	0.429** <i>0.171</i>	0.049 <i>0.416</i>	0.366 <i>0.342</i>	0.367* <i>0.201</i>
state unemployment rate	0.061 <i>0.021</i>	0.066 <i>0.024</i>	0.062 <i>0.023</i>	0.004	0.431 <i>0.526</i>	1.621*** <i>0.630</i>	-1.343 <i>1.080</i>	0.005 <i>0.860</i>	1.286** <i>0.567</i>
state union penetration	0.152 <i>0.073</i>	0.185 <i>0.069</i>	0.154 <i>0.076</i>	0.031***	0.361 <i>0.347</i>	-0.117 <i>0.259</i>	0.123 <i>0.492</i>	0.120 <i>0.452</i>	0.000 <i>0.197</i>
Tax competition (in year $t-1$)									
state's tax rate relative to highest tax rate among its neighboring states	-0.019 <i>0.036</i>	-0.024 <i>0.027</i>	-0.008 <i>0.025</i>	-0.016***	-0.039*** <i>0.006</i>	0.025*** <i>0.006</i>	-0.082*** <i>0.015</i>	-0.057*** <i>0.013</i>	0.025*** <i>0.008</i>
Diagnostics									
R^2					13.2%	14.6%	11.9%	12.1%	10.2%

Table A2. Potential Reverse Causality.

We relate changes in the top marginal rate at which a state taxes corporate income to the lagged state-level growth rate in per capita personal income and employment for the period 1970-2010. All specifications are estimated using OLS in first differences to remove time-invariant state-level heterogeneity in the levels equations. Each specification includes a set of BEA-region/year fixed effects. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a state-year. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively

	Δ tax rate (1)	Δ tax rate (2)	Δ tax rate (3)	Magnitude of tax increase (4)	Magnitude of tax cut (5)
Lagged state-level:					
changes in personal income	0.008 <i>0.007</i>		0.010 <i>0.009</i>	0.005 <i>0.006</i>	-0.005 <i>0.005</i>
changes in employment		0.001 <i>0.009</i>	-0.006 <i>0.013</i>	-0.012 <i>0.009</i>	-0.006 <i>0.009</i>
Region-year fixed effects	x	x	x	x	x
Adjusted R^2	2.4%	2.3%	2.3%	2.4%	0.9%
Number of state-years	1,960	1,960	1,960	1,960	1,960

Appendix C. Variable Definitions.

State-level variables (Table A1)

Democratic governor is an indicator set equal to one if the state is governed by a Democratic governor, and zero otherwise. Data come from the Congressional Quarterly (through 2008) and state election websites (after 2008).

State budget balance equals the difference between a state's *general revenues* and its *general expenditures* scaled by its *general expenditures*. The data come from the U.S. Census Bureau's State & Local Finances database, available at <http://www.census.gov/govs/local>.

State budget deficit equals *state budget balance* if the state runs a budget deficit, and zero otherwise.

State budget surplus equals *state budget balance* if the state runs a budget surplus, and zero otherwise.

State bond rating downgrade is an indicator set equal to one if the state's credit rating is downgraded by either S&P or Moody's.

GSP growth rate is the real annual growth rate in gross state product (GSP) using data obtained from the U.S. Bureau of Economic Analysis.

State unemployment rate is the state unemployment rate, obtained from the U.S. Bureau of Labor Statistics.

State union penetration is the fraction of private-sector employees in a state who belong to a labor union in year t . The data come from Hirsch and Macpherson (2003) as updated on their website, <http://www.unionstats.com>.

Tax competition is measured as the difference between a state's corporate income tax rate and the highest corporate income tax rate levied by any of the neighboring states.

State investment tax credit rate is the rate at which a firm can deduct capital expenditures directly from its state corporate income tax liability (in addition to the usual depreciation deductions against taxable income). Data through 2006 come from Chirinko and Wilson (2008). Data for subsequent years come from tax forms available on state Department of Revenue websites.

State R&D credit rate is the percentage of a firm's R&D expenditures that it can deduct directly from its state corporate income tax liability (in addition to the usual deduction against taxable income). Data through 2006 come from Wilson (2007). Data for subsequent years come from tax forms available on state Department of Revenue websites.

State job creation credit is set equal to one if the state offers a tax credit in return for hiring new workers meeting certain requirements, and zero otherwise. The data come from Appendix C1 in Neumark and Grijalva (2013).

State job creation grants is set equal to one if the state offers grant payments in return for hiring new workers meeting certain requirements, and zero otherwise. The data come from Appendix C1 in Neumark and Grijalva (2013).

State tax on banks captures changes in the rate at which a state taxes financial institutions with nexus to the state. (Both a physical presence in the state and out-of-state lending to borrowers located in the state constitute nexus.) The data come from the *Book of the States* and state codes accessed through Lexis-Nexis.

State per capita personal income is taken from the Bureau of Economic Analysis's State Personal Income and Employment Summary (line code 30).

State employment is taken from the Bureau of Economic Analysis's State Personal Income and Employment Summary (line code 7010).

County-level dependent variables

Employment is measured using data from the Personal Income and Employment Summary from the Bureau of Economic Analysis's (BEA) Regional Economic Accounts. It is constructed as the natural logarithm of the total number of wage and salary jobs (both full time and part time; line code 7020), scaled by either total county population (line code 20) or the number of county residents aged between 20 and 70, times 100. Employment in a county is measured by the location of the place of work, not by the worker's place of residence, and so includes workers commuting in from other counties.

Employment (excluding government) is measured as the total number of wage and salary jobs (line code 7020 in the BEA's Personal Income and Employment Summary) minus employment by government and government enterprises (line code 900 in the BEA's county-industry employment data files), scaled by total county population (line code 20 in the BEA's Personal Income and Employment Summary), times 100.

Self-employment is measured as the natural logarithm of the number of nonfarm self-employed workers in a county (line code 60 in the BEA's county-industry employment data files), scaled by total county population (line code 20 in the BEA's Personal Income and Employment Summary), times 100.

Wage income by place of work is measured using data from the Personal Income and Employment Summary from the BEA's Regional Economic Accounts. It is constructed as the natural logarithm of earnings by place of work (line code 35) minus proprietors' income (line code 70), scaled by total county population (line code 20), times 100.

Wage income by place of work (excluding government) is measured as the natural logarithm of earnings by place of work (line code 35 in the BEA's Personal Income and Employment Summary) minus proprietors' income (PIES line code 70) minus earnings of government employees (line code 900 in the BEA's county-industry income data files), scaled by total county population (PIES line code 20), times 100.

Wage income by place of work (including transfers) is measured using data from the Personal Income and Employment Summary from the BEA's Regional Economic Accounts. It is constructed as the natural logarithm of earnings by place of work (line code 35) minus proprietors' income (line code 70) plus personal current transfer receipts (line code 47), scaled by total county population (line code 20), times 100. Transfers receipts include food stamps, unemployment insurance, and training assistance.

Wage income by place of residence is measured using data from the Personal Income and Employment Summary from the BEA's Regional Economic Accounts. It is constructed as the natural logarithm of earnings by place of work (line code 35) minus proprietors' income (line code 70) plus adjustment for residence (line code 42), scaled by total county population (line code 20), times 100.

Self-employment income by place of work is measured using data from the Personal Income and Employment Summary from the BEA's Regional Economic Accounts. It is constructed as the natural logarithm of nonfarm proprietors' income (line code 72), scaled by total county population (line code 20), times 100.

Farm employment is measured as the natural log of the sum of the number of individuals employed and self-employed in farming (line code 70 in the BEA's county-industry employment data files), scaled by total county population (line code 20 in the BEA's Personal Income and Employment Summary), times 100.

Farm income is measured as the natural log of farm earnings from wage and self-employment (line code 81 in the BEA's county-industry income data files), scaled by total county population (line code 20 in the BEA's Personal Income and Employment Summary), times 100.

Farm income (excluding inventory adjustment) is measured as the natural log of farm earnings (line code 81 in the BEA's county-industry income data files) minus value of inventory change (line code 240 in the BEA's farm income and expenses data files), scaled by total county population (line code 20 in the BEA's Personal Income and Employment Summary), times 100.

County level demographic control variables

County demographic information from the decennial U.S. Census for the years 1970-2010, is obtained from the National Historical Geographic Information System (NHGIS.org). The first differences of the following demographic controls are included in regressions: percentage of persons over 25 with a 4-year college degree or higher qualification; percentage of the population that resides in a rural area; percentage of the population that is white; percentage of the population that is 21 or under; percentage of the population that is 65 or older; percentage of the population residing in owner-occupied residences; and percentage of families with children that are single-mother households. For off-census years we linearly interpolate the relevant numerator and denominator and then divide the two to obtain each of these percentage measures, giving us an average growth rate of each variable over the course of a decade. This approach is standard in the literature.

Conditioning variables

Small counties are counties that account for no more than 10% of a state's population, as measured in the year prior to the tax change.

Close counties are border-county pairs whose geographic midpoints are no more than 20 miles apart. To calculate distances between county pairs, we obtain latitudes and longitudes of county midpoints from the Census Bureau's Gazetteer files and use the great-circle distance formula. This gives the shortest distance between two points on the surface of a sphere (i.e., the earth).

National recessions are years classified as recessions by the NBER (see <http://www.nber.org/cycles.html>).

Regional recessions are defined as a period of at least two quarters of decline in real personal income in a given BEA region. This definition is intended to closely match the NBER's definition of national recessions. Though it is a common misconception that the NBER defines a recession as two consecutive quarters of falling GDP, most recessions do meet this criterion (see http://www.nber.org/cycles/recessions_faq.html). We use personal income growth rather than GSP growth because the former is available at a quarterly frequency while the latter is only available annually. The personal income data come from the Bureau of Economic Analysis's Quarterly State Personal Income data files.

State recessions are defined as a period of at least two quarters of decline in real personal income. While national and regional recessions are plausibly driven by aggregate factors, and so are essentially exogenous from an individual state's perspective, the same is not the case for state recessions. To avoid conditioning on the outcome of a corporate tax change designed to counteract the effects of a state recession, we classify a state corporate income tax change as occurring in a state recession if during the year *prior* to the tax change, the state experienced at least two quarters of negative growth in real personal income. The personal income data come from the Bureau of Economic Analysis's Quarterly State Personal Income data files.

Right-to-work states are states that have a right-to-work statute in place, as detailed in Ellwood and Fine (1987) and updated by Matsa (2010).

States with high unionization are states in which the fraction of non-agricultural wage and salary employees who are union members exceeds the national median in the previous year. The data come from Hirsch and Macpherson (2003) as updated on their website, www.unionstats.com.

States with low unionization are states in which the fraction of non-agricultural wage and salary employees who are union members is below the national median in the previous year. The data come from Hirsch and Macpherson (2003) as updated on their website, www.unionstats.com.

Rich counties are counties in which per capita personal income exceeds the national median among all counties in the previous year. The data come from the Personal Income and Employment Summary from the Bureau of Economic Analysis's Regional Economic Accounts (per capita personal income, line code 30).

Poor counties are counties in which per capita personal income is below the national median among all counties in the previous year. The data come from the Personal Income and Employment Summary from the Bureau of Economic Analysis's Regional Economic (per capita personal income, line code 30).

High-population-density counties are counties in which the population density, measured as population per square mile, exceeds the national median among all counties in the previous year. The data come from the Personal Income and Employment Summary from the Bureau of Economic Analysis's Regional Economic Accounts (county population, line code 20), and the Census Bureau for county land area.

Low-population-density counties are counties in which the population density, measured as population per square mile, is below the national median among all counties in the previous year. The data come from the Personal Income and Employment Summary from the Bureau of Economic Analysis's Regional Economic Accounts (county population, line code 20), and the Census Bureau for county land area.

High "small-business-employment-share" counties are counties in which the share of total county employment attributable to establishments with fewer than ten employees exceeds the national median among all counties in the prior year. The data come from the Census Bureau's County Business Patterns database and are available only from 1974. The CBP provides information on the number of establishments broken down by size categories based on the number of employees. The establishment size categories are 1-4 employees, 5-9 employees, etc. To calculate total employment attributable to establishments with fewer than 10 employees, we follow Adelino, Schoar, and Severino (2015). We first obtain employment for each establishment size category (i.e., 1-4 employees and 5-9 employees) by multiplying the number of establishments by the middle point of that category. For example, the total employment of 1-4 employee establishments is 2.5 multiplied by the number of 1-4 employee establishments. The share of total employment attributable to small establishments is the sum of employment in 1-4 and 5-9 employee establishments, divided by total county employment.

Low "small-business-employment-share" counties are counties in which the share of total county employment attributable to establishments with fewer than ten employees is below the national median among all counties in the prior year. The data come from the Census Bureau's County Business Patterns database.

Appendix D. Farms

Table A3 investigates the effects of corporate tax changes on farm employment and farm income. To establish a point of reference, columns 1 and 2 reproduce our baseline estimates from column 3 of Tables 3 and 4 for wage employment and income, respectively.

Column 3 measures total county employment as wage employment (as in our baseline) plus farm and non-farm self-employment. Column 4 measures total county income from work as wage income (as in our baseline) plus farm and non-farm self-employment income. For tax increases, the effects on total employment and total income are essentially indistinguishable from our baseline estimates. For tax cuts, on the other hand, we find a significantly positive effect on total income (though not on total employment). This contrasts with the baseline. The effect is quite large economically, measuring 2.191% ($p=0.023$) in the average treated county.

To see what is driving this departure from our baseline findings, we next consider farms in isolation. Columns 5 and 6 show that a one-percentage-point corporate tax cut is associated with a 1.294% ($p=0.003$) increase in farm employment,³⁵ and a surprisingly large 8.242% ($p=0.016$) increase in farm income. Further investigation reveals that this large effect is due to a quirk of the BEA data. The BEA defines farm income as receipts minus production expenses plus an adjustment to reflect changes in the value of farm inventories. The inventory term is added so that farm income reflects *current* production. As column 7 shows, the inventory adjustment is crucial. When it is excluded from farm income, the large positive effect disappears and even switches sign ($p=0.434$). Exactly why corporate tax cuts are associated with increased values of farm inventories is unclear. What is clear is that farming is best excluded when estimating the effect of corporate tax changes on employment and income.

³⁵ Since farms employ relatively few workers, this effect is not large enough to raise total county employment, not even, as we show in Table 9, in rural counties.

Table A3. Effect of Corporate Tax Changes on Farm Employment and Income.

We estimate the effect of corporate income taxes on farm employment and income following the methodology in Tables 3 and 4. In columns 1 and 2, we repeat our baseline estimates from column 3 of Tables 3 and 4, respectively, of the effect of corporate tax changes on wage employment and income. In column 3, we include to our baseline employment measure the number of nonfarm self-employed workers and the number of self-employed farmers. In column 4, we include to our baseline income measure the income from nonfarm self-employment and the income of self-employed farmers. Column 5 examines the effect only on farm employment, while column 6 examines the effect only on farm income. Farm income is (approximately) equal to receipts, minus production expenses, plus an adjustment to reflect changes in the value of farm inventories. The BEA adds a “value of inventory adjustment” so that farm income only reflects income from *current* production. Column 7 excludes the inventory adjustment from the farm income measure. In all other respects, the methodology follows that in Tables 3 and 4. All specifications are estimated using OLS in first differences to remove time-invariant county-level heterogeneity in the levels equations and include a set of contiguous-border-county-group/year fixed effects. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a county-year. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

<i>Dep. var.:</i> Change in log	Baseline measures: column 3 from Tables 3 and 4		County total (including farms and self- employed)		Farm employ- ment only (5)	Farm income (6)	Farm income (excl. inventory) (7)
	employ- ment (1)	income (2)	employ- ment (3)	income (4)			
magnitude of tax increase	-0.282*** <i>0.075</i>	-0.420*** <i>0.087</i>	-0.256** <i>0.072</i>	-0.403*** <i>0.199</i>	-0.083 <i>0.174</i>	-0.678 <i>1.505</i>	-1.069 <i>1.924</i>
magnitude of tax cut	0.065 <i>0.128</i>	0.132 <i>0.175</i>	0.081 <i>0.123</i>	2.191** <i>0.934</i>	1.294*** <i>0.415</i>	8.242** <i>3.310</i>	-2.618 <i>3.320</i>
Demographic controls	x	x	x	x	x	x	x
Group-year fixed effects	x	x	x	x	x	x	x
Adjusted R^2	9.1%	20.9%	9.0%	34.5%	43.1%	42.5%	38.1%
Number of county-years	10,366	10,366	10,366	10,366	10,280	9,230	9,262

Figure 1. Average Top Marginal Corporate Income Tax Rate by Year (with 20th/80th percentiles)

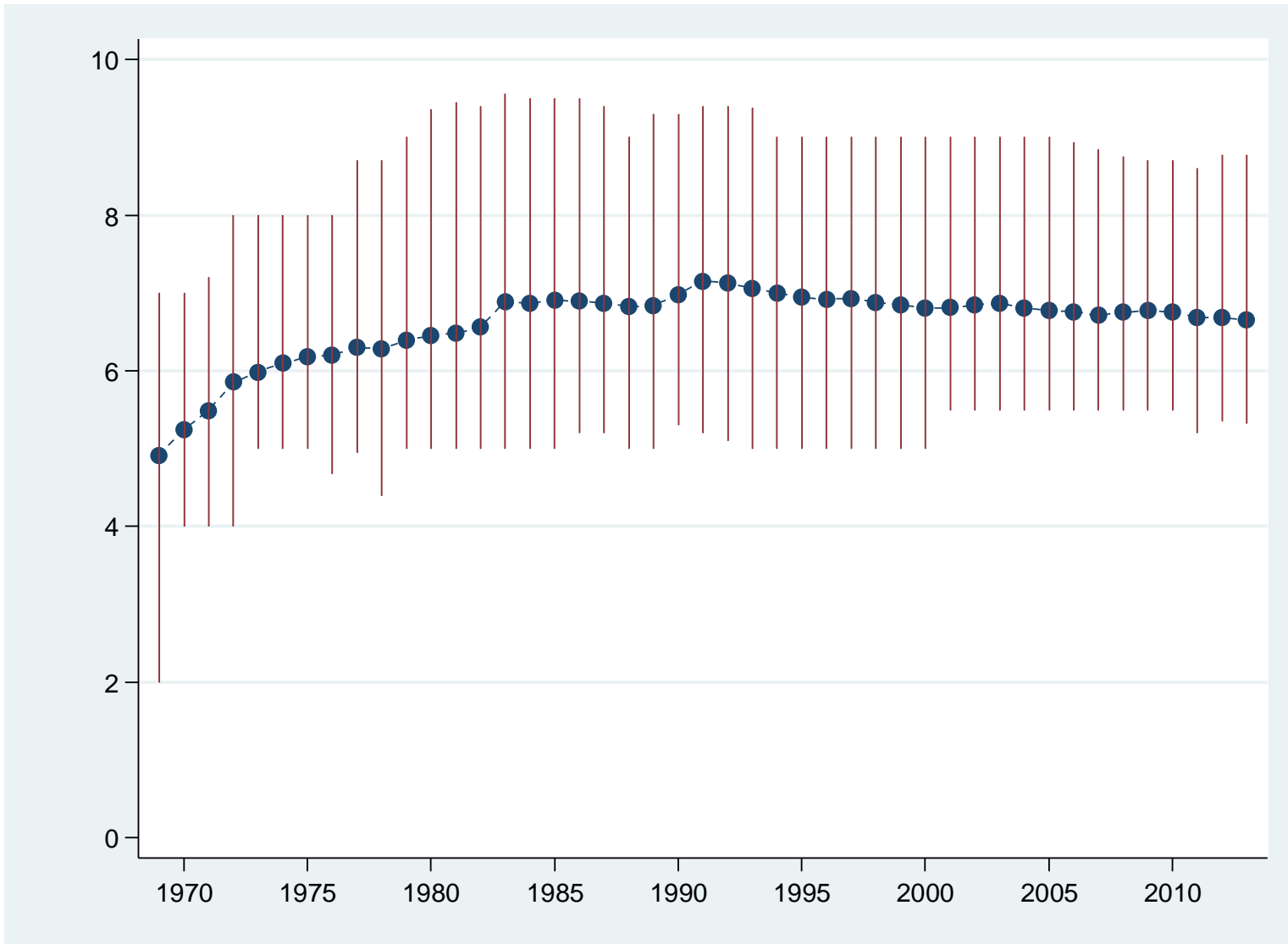


Figure 2. Distribution of Corporate Tax Changes Across States and Time

To make the figure readable, we perturb the magnitudes of the tax changes by at most 20bps in some instances.

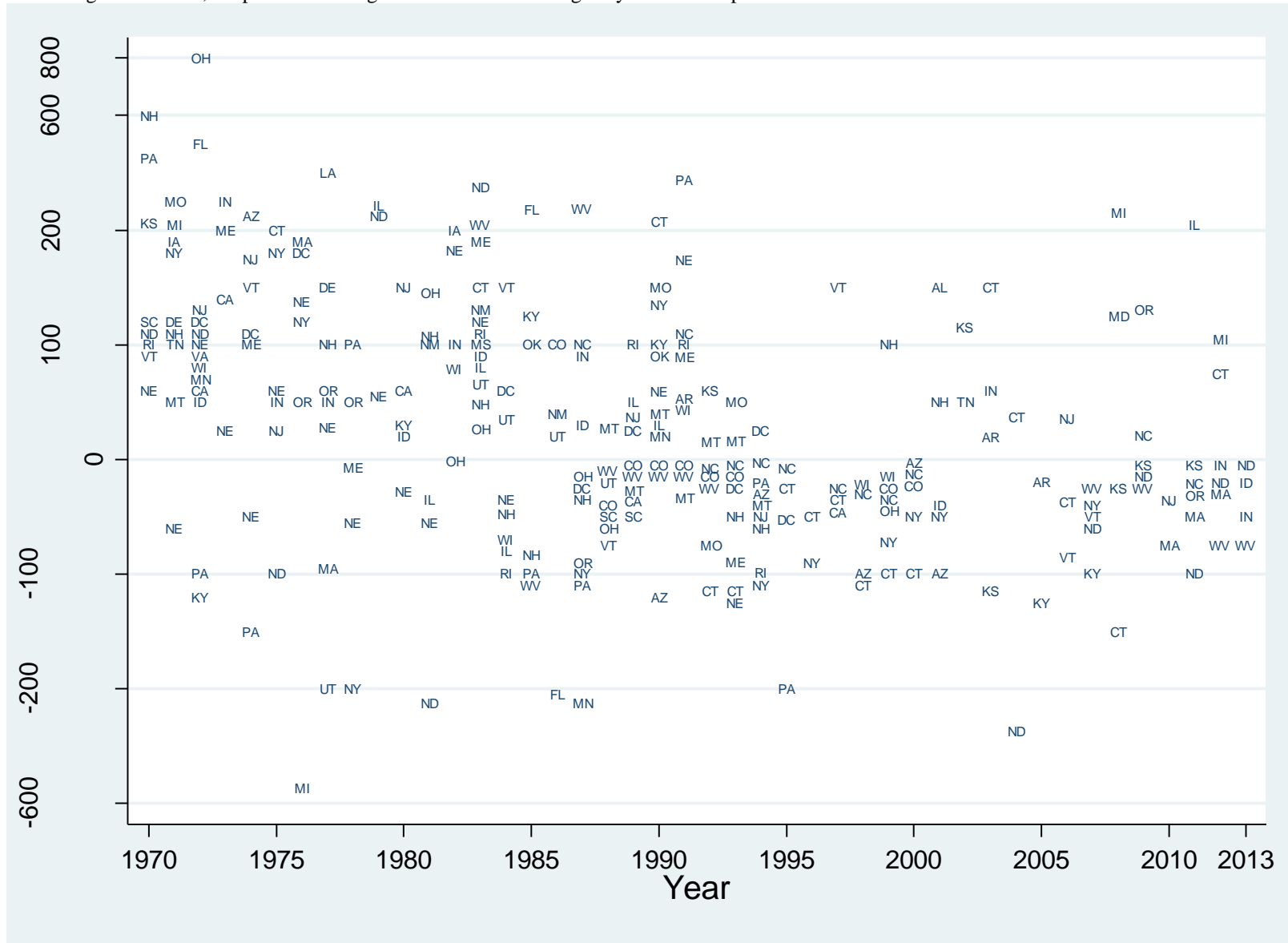


Figure 3. Impact Response Functions

This figure plots the dynamic responses of employment and income to changes in state corporate income tax rates, along with 95% confidence bands, as estimated in equation (3). The figure shows the cumulative effect of a one-percentage-point tax increase or tax cut on employment and income over time, starting from the year of the tax change ($t = 0$). The y-axis measures the cumulative log change (expressed in percent) in our baseline employment and income measures from Tables 3 and 4.

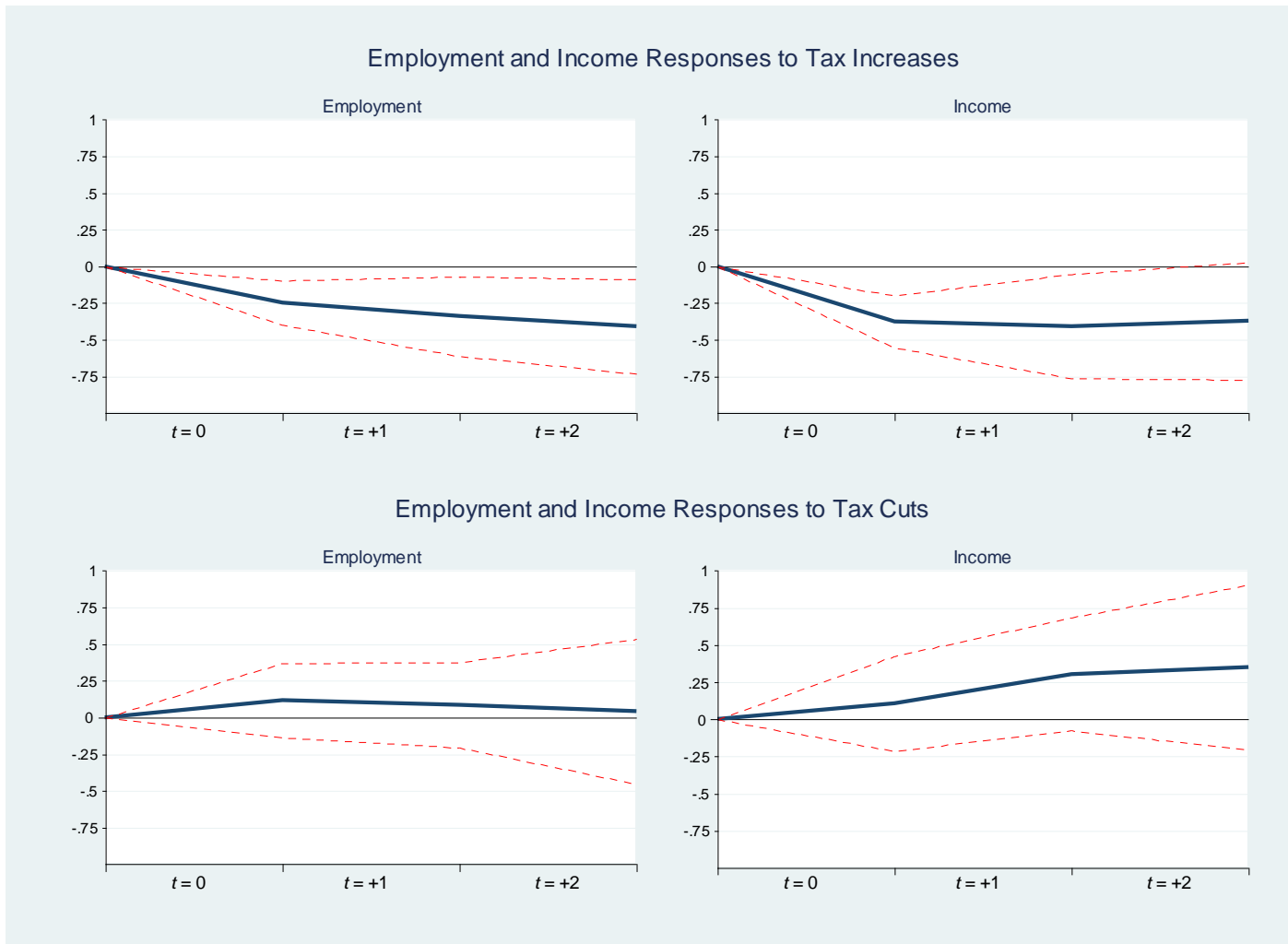


Table 1. Summary Statistics and Coincident State-level Policy Changes.

Panel A reports summary statistics of the 271 changes in state corporate income tax rates that occurred over the period from 1970 to 2013. The data come from several sources. For the years through 1988, we collect data from the *State Tax Handbook*, published annually by *Commerce Clearing House*. For 1989-2011, we use the information listed in Appendices A and B of Heider and Ljungqvist (2015). For 2012-2013, we use data from the Tax Foundation. Throughout the analysis, we focus on changes in the top statutory marginal tax rate. Panel B reports the extent to which increases or cuts in state corporate income taxes coincide with state-level policy changes that could independently affect economic outcomes, confounding the estimated effects of corporate income tax changes. We focus on initiations of and changes in state incentive programs (i.e., tax credits for investment, R&D, or job creation), changes in state taxes on banks, and changes in apportionment rules. Note that data for changes in apportionment rules (which are taken from Appendix D in Bernthal et al. 2012) are not available for the period 1970-1977 or for 2013. For variable definitions and details of their construction, see Appendix C.

	Tax increases	Tax cuts
Panel A. Summary statistics		
Number of tax changes	140	131
Mean tax change	1.26%	-0.65%
Standard deviation	1.13%	0.71%
Minimum tax change	0.16%	-5.45%
Median tax change	1.00%	-0.48%
Maximum tax change	8.00%	-0.02%
Panel B. Coincident state-level policy changes		
Number of tax changes		
... that coincide with		
initiation of state incentive programs		
R&D tax credits	2	6
investment tax credits	0	2
job creation tax credits	3	6
change in state incentive programs		
R&D tax credits	2	6
investment tax credits	1	6
job creation tax credits	0	0
changes in state tax on banks		
increase in tax on banks	97	2
cut in state tax on banks	3	85
changes in apportionment rules	5	5

Table 2. Summary Statistics.

Panel A provides summary statistics for our dependent variables in all treated and control border counties, and those counties located in the interior of treated and control states, measured the year before a tax change. Panel B examines the parallel-trends assumption by reporting changes in our dependent variables the year before a tax change. All values are in real 2005 dollars. For variable definitions, see Appendix C. The final three columns report differences-in-means. To estimate statistical significance, we use *t*-tests. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Border county-years				Interior county-years				Differences in means		
	in treated states		in control states		in treated states		in control states		Border: treated- controls	Treated: border- interior	Control: border- interior
	(N = 3,390)		(N = 3,650)		(N = 7,828)		(N = 18,820)				
	mean	s.d.	mean	s.d.	mean	s.d.	mean	s.d.			
Panel A. Levels (year prior to tax change)											
Employment											
wage employment /county population	0.350	0.123	0.362	0.145	0.343	0.127	0.347	0.142	-0.012***	0.007***	0.015***
wage employment/cnty. pop'n aged 20-70	0.587	0.183	0.606	0.216	0.578	0.198	0.583	0.224	-0.019***	0.009**	0.024***
wage employment excl. govt./cnty. pop'n	0.270	0.111	0.282	0.137	0.264	0.114	0.267	0.127	-0.012***	0.007***	0.015***
self-employment/county population	0.083	0.039	0.080	0.035	0.080	0.038	0.084	0.040	0.003**	0.002***	-0.004***
farm employment/county population	0.056	0.060	0.054	0.058	0.062	0.064	0.056	0.053	0.002*	-0.005***	-0.002
Income (in 2005 dollars)											
wage income/county population	11,271	7,498	11,695	7,838	10,598	6,369	11,096	8,162	-423**	674***	598***
wage income excl. govt./cnty. pop'n	8,536	6,363	8,958	6,846	7,991	5,448	8,358	7,258	-423***	545***	600***
wage income incl. transfers/cnty. pop'n	14,821	8,093	15,166	8,265	13,775	6,847	14,612	8,526	-345*	1,046***	554***
wage income (residence) /cnty. pop'n	12,524	5,623	12,907	5,725	11,873	5,016	12,708	5,302	-384***	651***	199**
self-employed income/cnty. pop'n	1,720	1,077	1,664	897	1,633	822	1,677	918	55**	1,659***	-13
farm income/cnty. pop'n	1,182	2,199	1,198	2,281	1,343	2,353	1,088	1,978	-16	161***	109***
farm income (excl. inventory)/cnty. pop'n	1,140	2,088	1,191	2,292	1,300	2,245	1,094	1,980	-51	161***	98***
Panel B. Log changes x 100 (year prior to tax change)											
Employment											
wage employment /county population	0.407	4.400	0.449	4.729	0.700	4.934	0.520	4.734	-0.042	-0.293***	-0.071
wage employment/cnty. pop'n aged 20-70	0.068	4.432	0.071	4.777	0.300	4.971	0.112	4.787	-0.003	-0.232***	-0.041
wage employment excl. govt./cnty. pop'n	0.350	5.554	0.416	6.012	0.706	6.305	0.558	5.969	-0.066	-0.355***	-0.142
self-employment/county population	1.477	7.985	1.597	7.413	1.152	8.758	1.775	8.476	-0.120	0.324*	-0.178
farm employment/county population	-2.086	5.311	-1.861	5.654	-1.904	5.352	-2.080	5.518	-0.225*	-0.182	0.219**
Income											
wage income/county population	1.797	6.118	1.793	6.459	1.918	6.758	1.994	6.274	0.005	-0.121	-0.201*
wage income excl. govt./cnty. pop'n	1.695	7.848	1.660	8.489	1.791	8.766	1.959	8.106	0.035	-0.096	-0.299**
wage income incl. transfers/cnty. pop'n	2.380	4.670	2.345	5.084	2.528	5.236	2.479	4.855	0.035	-0.148	-0.134
wage income (residence) /cnty. pop'n	1.912	5.022	1.786	5.157	1.944	5.050	2.029	4.729	0.126	-0.032	-0.243***
self-employed income/cnty. pop'n	-0.047	12.430	-0.181	13.037	-0.287	12.830	0.076	12.408	0.134	0.240	-0.257
farm income/cnty. pop'n	-0.717	68.448	-0.704	68.079	-1.819	73.221	-2.862	75.397	-0.013	1.102	2.158
farm income (excl. inventory)/cnty. pop'n	1.250	73.577	-0.801	70.401	-2.106	73.861	-2.944	75.384	2.050	3.356**	2.143

Table 3. Effect of Corporate Tax Changes on Employment.

We estimate the effect of corporate income taxes on employment in a differences-in-differences specification that regresses within-county changes in log employment on changes in the state’s top marginal corporate income tax rate. (Given the log specification, the dependent variable is thus the year-on-year growth rate in employment. It is expressed in percent and measured as of the end of a calendar year.) Our baseline employment measure in columns 1 to 3 is the total number of wage and salary jobs (full-time and part-time) in a county scaled by total county population. In column 4, we instead scale by the number of county residents aged between 20 and 70. Column 5 excludes government employment from our baseline employment measure. Column 6 estimates the effect on the number of nonfarm self-employed workers. Treated counties are those that experience a corporate tax change. The controls are counties located in a neighboring state that have not themselves experienced a state corporate income tax change in the previous four years. The sample of treated and control counties is restricted to border counties such that treated and control counties form contiguous cross-border pairs (or groups) located on opposite sides of a state border. The regressions are estimated in first differences to remove time-invariant county-level heterogeneity in the levels equation. Except in column 2, each regression includes a set of contiguous-border-county-group/year fixed effects to remove unobserved time-varying shocks to local economic conditions. In other words, we estimate the effect of corporate taxes on economic activity after differencing away both unobserved county-level time-invariant characteristics and unobserved common variation in economic conditions that affect groups of contiguous border counties located either side of a state border. All specifications include changes in county-level demographic controls (the fractions of the county’s residents who are aged over 25 and have a college degree; live in a rural area; are white; are aged under 21; are aged over 65; reside in owner-occupied residences; or live in single-mother households). To conserve space, their coefficients are not reported. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a county-year. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	<i>Dep. var.: Change in log employment</i>					
	(1)	(2)	(3)	scaled by population aged 20-70 (4)	excluding govt. employ- ment (5)	self employ- ment only (6)
Δ tax rate	-0.241*** <i>0.065</i>	-0.481*** <i>0.079</i>				
magnitude of tax increase			-0.282*** <i>0.075</i>	-0.288*** <i>0.075</i>	-0.289*** <i>0.086</i>	-0.183 <i>0.140</i>
magnitude of tax cut			0.065 <i>0.128</i>	0.100 <i>0.126</i>	0.008 <i>0.146</i>	0.299 <i>0.349</i>
Demographic controls	x	x	x	x	x	x
Group-year fixed effects	x		x	x	x	x
Adjusted R^2	9.1%	2.7%	9.1%	8.8%	10.0%	16.1%
Number of county-years	10,366	7,040	10,366	10,334	10,366	10,366

Table 4. Effect of Corporate Tax Changes on Income.

We estimate the effect of corporate income taxes on wage and self-employment income in a differences-in-differences specification that regresses within-county changes in log income on changes in the state’s top marginal corporate income tax rate. (Given the log specification, the dependent variable is thus the year-on-year growth rate in income. It is expressed in percent and measured as of the end of a calendar year.) Our baseline income measure in columns 1 to 3 is total wage and salary income (by place of work) in a county scaled by total county population. Column 4 excludes income from government employment from our baseline wage income measure. Column 5 returns to our baseline wage income measure but includes government transfer receipts (e.g., unemployment insurance). Column 6 measures wage income by place of residence rather than by place of work. Column 7 estimates the effect on income from nonfarm self-employment. All income variables are pre-tax. Treated counties are those that experience a corporate tax change. The controls are counties located in a neighboring state that have not themselves experienced a state corporate income tax change in the previous four years. The sample of treated and control counties is restricted to border counties such that treated and control counties form contiguous cross-border pairs (or groups) located on opposite sides of a state border. The regressions are estimated in first differences to remove time-invariant county-level heterogeneity in the levels equation. Except in column 2, each regression includes a set of contiguous-border-county-group/year fixed effects to remove unobserved time-varying shocks to local economic conditions. In other words, we estimate the effect of corporate taxes on economic activity after differencing away both unobserved county-level time-invariant characteristics and unobserved common variation in economic conditions that affect groups of contiguous border counties located either side of a state border. All specifications include changes in county-level demographic controls (the fractions of the county’s residents who are aged over 25 and have a college degree; live in a rural area; are white; are aged under 21; are aged over 65; reside in owner-occupied residences; or live in single-mother households). To conserve space, their coefficients are not reported. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a county-year. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	<i>Dep. var.: Change in log income</i>						
	(1)	(2)	(3)	excluding govt. employ- ment (4)	including transfers (5)	by place of residence (6)	self employ- ment only (7)
Δ tax rate	-0.367*** <i>0.072</i>	-0.477*** <i>0.144</i>					
magnitude of tax increase			-0.420*** <i>0.087</i>	-0.422*** <i>0.098</i>	-0.307*** <i>0.091</i>	-0.247*** <i>0.070</i>	-0.165 <i>0.189</i>
magnitude of tax cut			0.132 <i>0.175</i>	-0.014 <i>0.216</i>	0.088 <i>0.135</i>	0.146 <i>0.141</i>	0.518 <i>0.444</i>
Demographic controls	x	x	x	x	x	x	x
Group-year fixed effects	x		x	x	x	x	x
Adjusted R^2	20.9%	6.8%	20.9%	18.2%	19.1%	41.1%	37.3%
Number of county-years	10,366	7,040	10,366	10,366	10,366	10,366	10,366

Table 5. Robustness: Expectations, Influential Counties, and Far-Apart County Pairs.

To investigate robustness, columns 1 and 2 remove potentially politically influential counties by restricting the estimation sample to counties that account for no more than 10% of a state’s population, as measured in the year prior to a tax change. Columns 3 and 4 remove far-apart border-county pairs, which may not plausibly share parallel trends, by restricting the estimation sample to border county pairs whose geographic midpoints are no more than 20 miles apart. All specifications are estimated using OLS in first differences to remove time-invariant county-level heterogeneity in the levels equations. Each specification includes a set of contiguous-border-county-group/year fixed effects. This controls for unobserved time-varying local economic conditions by estimating the tax effect within groups of contiguous border counties located either side of a state border, such that counties on one side of the border are treated and the contiguous counties on the other side of the border serve as controls. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a county-year. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

<i>Dep. var.:</i> Change in log	“Small” counties only		“Close” counties only	
	employ- ment (1)	income (2)	employ- ment (3)	income (4)
magnitude of tax increase	-0.282*** <i>0.078</i>	-0.432*** <i>0.089</i>	-0.329** <i>0.152</i>	-0.433** <i>0.188</i>
magnitude of tax cut	0.055 <i>0.136</i>	0.126 <i>0.185</i>	0.016 <i>0.376</i>	0.121 <i>0.603</i>
Demographic controls	x	x	x	x
Group-year fixed effects	x	x	x	x
Adjusted R^2	8.0%	19.6%	25.5%	30.9%
Number of county-years	9,580	9,580	1,515	1,515

Table 6. Robustness: Coincident Policy Changes, Tax Differentials, and Industry Effects.

To investigate robustness, columns 1 and 2 drop corporate tax changes that coincide with bank tax changes of the same sign. Columns 3 and 4 drops corporate tax changes that coincide with the initiation of or a change in a tax credit program intended to stimulate either investment, R&D, or job creation. Columns 5 and 6 restrict the sample to border county pairs whose tax rates differ by at most 20 basis points two years prior to treatment. All specifications in columns 1 to 6 include a set of contiguous-border-county-group/year fixed effects to control for unobserved time-varying shocks to local economic conditions. In columns 1 to 6, the unit of analysis is a county-year. Columns 7 to 9 control for industry-level shocks. In columns 7 to 9, the unit of analysis is a county-industry-year. In columns 7 and 8, the industry breakdown is at the SIC1/NAICS2 level. In column 9, the industry breakdown is at the SIC2/NAICS3 level. Columns 7 to 9 include a set of contiguous-border-county-group-*industry*/year fixed effects. These control for unobserved time-varying shocks that affect the same industry in groups of contiguous border counties located on either side of a state border. All regressions are estimated in first differences. In columns 1 to 6, this removes time-invariant county-level heterogeneity in the levels equation, while in columns 7 to 9, first-differencing removes time-invariant county-industry-level heterogeneity in the levels equation. All specifications include changes in county-level demographic controls (the fractions of the county’s residents who are aged over 25 and have a college degree; live in a rural area; are white; are aged under 21; are aged over 65; reside in owner-occupied residences; or live in single-mother households). To conserve space, their coefficients are not reported. For variable definitions and details of their construction, see Appendix C. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

<i>Dep. var.:</i> Change in log	Excluding corp. tax changes coinciding w/ bank tax changes		Excluding corp. tax changes coinciding w/ incentive programs		Tax rates at most 20bp apart		County-industry level w/ group-industry-year fixed effects		
	employ-ment	income	employ-ment	income	employ-ment	income	employ-ment	income	income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
magnitude of tax increase	-0.332*** <i>0.106</i>	-0.274*** <i>0.094</i>	-0.255*** <i>0.076</i>	-0.411*** <i>0.091</i>	-0.516** <i>0.212</i>	-0.605*** <i>0.214</i>	-0.184** <i>0.079</i>	-0.311*** <i>0.100</i>	-0.302** <i>0.125</i>
magnitude of tax cut	-0.038 <i>0.276</i>	0.282 <i>0.282</i>	0.140 <i>0.151</i>	0.178 <i>0.179</i>	-0.201 <i>0.549</i>	-0.385 <i>0.772</i>	-0.022 <i>0.201</i>	-0.054 <i>0.287</i>	-0.121 <i>0.182</i>
Demographic controls	x	x	x	x	x	x	x	x	x
Group-year fixed effects	x	x	x	x	x	x			
Group-industry-year FE:									
SIC1/NAICS2							x	x	
SIC2/NAICS3									x
Adjusted R^2	11.9%	22.1%	10.8%	23.4%	20.2%	19.4%	13.2%	19.7%	26.0%
Number of observations	2,906	2,906	9,397	9,397	860	860	81,723	82,769	267,691

Table 7. Business Cycle Effects.

We examine whether state corporate income tax changes have heterogeneous effects on employment and income depending on whether they are implemented during recessions. Columns 1 and 2 split the sample according to whether a tax change comes into effect during a national recession year, as classified by the NBER. Columns 3 and 4 split the sample according to whether a tax change occurs during a regional recession, based on at least two quarters of negative real personal income growth in the state's BEA region. Columns 5 and 6 split the sample according to whether a tax change occurs during a state recession. To avoid conditioning on the outcome of a state tax change, we classify tax changes as occurring during state recessions if in the year *prior* to the tax change, the state experienced at least two quarters of negative real personal income growth. All specifications are estimated using OLS in first differences to remove time-invariant county-level heterogeneity in the levels equations. Each specification includes a set of contiguous-border-county-group/year fixed effects. This controls for unobserved time-varying local economic conditions by estimating the tax effect within groups of contiguous border counties located either side of a state border, such that counties on one side of the border are treated and the contiguous counties on the other side of the border serve as controls. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a county-year. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

<i>Dep. var.:</i> Change in log	Conditioned on national recession		Conditioned on regional recession		Conditioned on state recession	
	employ- ment (1)	income (2)	employ- ment (3)	income (4)	employ- ment (5)	income (6)
Magnitude of tax increase						
x recession	-0.230*	-0.409**	-0.251**	-0.467***	-0.316***	-0.439***
	<i>0.125</i>	<i>0.167</i>	<i>0.106</i>	<i>0.147</i>	<i>0.100</i>	<i>0.130</i>
x no recession	-0.309***	-0.426***	-0.303***	-0.388***	-0.267***	-0.412***
	<i>0.089</i>	<i>0.092</i>	<i>0.091</i>	<i>0.095</i>	<i>0.090</i>	<i>0.106</i>
Magnitude of tax cut						
x recession	0.639***	1.000	0.597**	1.173*	0.681**	1.166**
	<i>0.213</i>	<i>0.635</i>	<i>0.240</i>	<i>0.603</i>	<i>0.268</i>	<i>0.513</i>
x no recession	-0.081	-0.089	-0.043	-0.079	-0.136	-0.204
	<i>0.149</i>	<i>0.185</i>	<i>0.143</i>	<i>0.167</i>	<i>0.156</i>	<i>0.184</i>
Demographic controls	x	x	x	x	x	x
Group-year fixed effects	x	x	x	x	x	x
Wald test (<i>F</i>):						
Tax increase: recession = no recession	0.27	0.01	0.17	0.25	0.16	0.03
Tax cut: recession = no recession	6.20**	2.18	4.58**	3.50*	5.76**	5.15**
Adjusted <i>R</i> ²	9.1%	20.9%	9.1%	20.9%	9.1%	21.0%
Number of county-years	10,366	10,366	10,366	10,366	10,366	10,366

Table 8. Labor Market Flexibility.

We examine whether state corporate income tax changes have heterogeneous effects on employment and income depending on labor market flexibility. Columns 1 and 2 split the sample according to whether a county is located in a right-to-work state. Columns 3 and 4 split the sample according to whether a county is located in a state whose private-sector unionization rate exceeds the national median in the previous year. All specifications are estimated using OLS in first differences to remove time-invariant county-level heterogeneity in the levels equations. Each specification includes a set of contiguous-border-county-group/year fixed effects. This controls for unobserved time-varying local economic conditions by estimating the tax effect within groups of contiguous border counties located either side of a state border, such that counties on one side of the border are treated and the contiguous counties on the other side of the border serve as controls. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a county-year. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

<i>Dep. var.:</i> Change in log	Conditioned on right-to-work state		Conditioned on unionization rate	
	employ- ment (1)	income (2)	employ- ment (3)	income (4)
Magnitude of tax increase				
x no right-to-work	-0.239*** <i>0.070</i>	-0.352*** <i>0.101</i>		
x right-to-work	-0.361** <i>0.149</i>	-0.546*** <i>0.136</i>		
x high unionization rate			-0.254*** <i>0.067</i>	-0.381*** <i>0.100</i>
x low unionization rate			-0.332** <i>0.160</i>	-0.490*** <i>0.157</i>
Magnitude of tax cut				
x high unionization rate			-0.123 <i>0.226</i>	-0.126 <i>0.245</i>
x low unionization rate			0.187 <i>0.169</i>	0.300 <i>0.218</i>
x no right-to-work	-0.024 <i>0.206</i>	-0.010 <i>0.228</i>		
x right-to-work	0.149 <i>0.199</i>	0.268 <i>0.258</i>		
Demographic controls	x	x	x	x
Group-year fixed effects	x	x	x	x
Wald test (<i>F</i>):				
Tax increase: high = low	0.58	1.36	0.20	0.33
Tax cut: high = low	0.28	0.56	0.98	1.47
Adjusted <i>R</i> ²	9.1%	20.9%	9.1%	20.9%
Number of county-years	10,366	10,366	10,366	10,366

Table 9. Heterogeneous Effects by County Characteristics.

We examine whether state corporate income tax changes have heterogeneous effects on employment and income based on county characteristics. In columns 1 and 2, counties are classified as either “rich” or “poor” based on whether their per capita personal income is above or below the national median among all counties one year earlier. In columns 3 and 4, counties are classified as having either high or low population density based on whether their population per square mile is above or below the national median among all counties one year earlier. In columns 5 and 6, counties are classified as having either high or low “small-business employment share” based on whether the proportion of total county employment attributable to businesses with fewer than ten employees is above or below the national median among all counties one year earlier. In all other respects, the methodology follows that in Tables 3 and 4. For variable definitions and details of their construction, see Appendix C. Standard errors are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

<i>Dep. var.:</i> Change in log	Conditioned on per capita personal income		Conditioned on population density		Conditioned on small-business employment share	
	employ- ment (1)	income (2)	employ- ment (3)	income (4)	employ- ment (5)	income (6)
Magnitude of tax increase						
x rich county	-0.271*** <i>0.088</i>	-0.444*** <i>0.122</i>				
x poor county	-0.297** <i>0.134</i>	-0.386*** <i>0.141</i>				
x high-density county			-0.245*** <i>0.076</i>	-0.385*** <i>0.093</i>		
x low-density county			-0.354** <i>0.141</i>	-0.488*** <i>0.166</i>		
x high small-business shr.					-0.282 <i>0.174</i>	-0.463*** <i>0.172</i>
x low small-business shr.					-0.319** <i>0.130</i>	-0.582*** <i>0.163</i>
Magnitude of tax cut						
x rich county	0.125 <i>0.154</i>	-0.040 <i>0.173</i>				
x poor county	-0.039 <i>0.211</i>	0.428 <i>0.288</i>				
x high-density county			-0.018 <i>0.132</i>	-0.068 <i>0.149</i>		
x low-density county			0.127 <i>0.191</i>	0.284 <i>0.247</i>		
x high small-business shr.					0.172 <i>0.158</i>	0.217 <i>0.216</i>
x low small-business shr.					-0.040 <i>0.120</i>	-0.172 <i>0.195</i>
Demographic controls	x	x	x	x	x	x
Group-year fixed effects	x	x	x	x	x	x
Wald test (<i>F</i>):						
Tax increase: high = low	0.03	0.09	0.50	0.30	0.05	0.24
Tax cut: high = low	0.42	2.82*	0.41	1.83	1.96	1.86
Adjusted <i>R</i> ²	9.0%	20.9%	9.1%	20.9%	18.9%	24.2%
Number of county-years	10,366	10,366	10,366	10,366	8,219	8,219

Table 10. Testing for Cross-Border Spillovers.

Columns 1 through 4 test for cross-border spillovers emanating from changes in either household or firm behavior. For example, following a corporate tax cut, firms may move operations from the control county to the treated county, creating jobs there, and households in the treated county may spend their wage increases not just in their home county but also across the border. In either case, treated and control counties would both be affected by the tax cut. For household spillovers, this would attenuate the estimated treatment effect. For corporate spillovers, it would amplify the estimated treatment effect. Household spillovers in response to tax rises (tax cuts) imply systematic reductions (increases) in economic activity in control border counties relative to counties located in the interior of the control state that are too far away from the border to be affected by the spillover. Corporate spillovers imply the opposite. To test this, columns 1 through 4 compare interior counties in control states to those border counties in the same state neighboring a treated state that increases (columns 1 and 3) or cuts corporate taxes (columns 2 and 4). Columns 5 and 6 return to the baseline estimation sample of Tables 3 and 4 and allow the effect of tax changes to depend on whether the control county has a higher or lower corporate tax rate than the treated county once the tax change has come into effect. This allows us to test whether corporate cross-border spillovers, if they exist, depend on the tax differential: firms operating in a treated county might be more likely to shift operations across the border after a tax rise by if the neighboring state has a lower tax rate, and vice versa for a tax increase. Such behavior would asymmetrically amplify the effect of tax changes; columns 5 and 6 test for such asymmetry. All specifications are estimated using OLS in first differences to remove time-invariant county-level heterogeneity in the levels equations. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a county-year. Standard errors clustered at the state level are shown in italics underneath the coefficient estimates. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Change in log employment		Change in log income		Change in log ...	
	around tax increase in neighboring state	around tax cut in neighboring state	around tax increase in neighboring state	around tax cut in neighboring state	employment	income
	(1)	(2)	(3)	(4)	(5)	(6)
=1 if county lies in state's interior	0.110	-0.217	0.189	-0.266		
	<i>0.126</i>	<i>0.147</i>	<i>0.206</i>	<i>0.191</i>		
magnitude of tax increase						
x control county has lower tax					-0.278***	-0.405***
					<i>0.074</i>	<i>0.094</i>
x control county has higher tax					-0.459*	-0.554**
					<i>0.229</i>	<i>0.224</i>
magnitude of tax cut						
x control county has lower tax					0.011	0.202
					<i>0.215</i>	<i>0.243</i>
x control county has higher tax					0.201	0.104
					<i>0.250</i>	<i>0.288</i>
Demographic controls	x	x	x	x	x	x
State-year fixed effects	x	x	x	x		
County group-year fixed effects					x	x
Wald test (<i>F</i>):						
Tax increase: higher = lower					0.64	0.43
Tax cut: higher = lower					0.25	0.06
Adjusted <i>R</i> ²	17.3%	17.3%	25.1%	21.8%	9.1%	20.9%
Number of county-years	12,176	10,265	12,176	10,265	10,366	10,366