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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.

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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

[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%).

corporate taxes are plentiful: we count 271 changes in state corporate income tax rates since 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

[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).

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 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.

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

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 macroeconomic data, which rely on either the "narrative approach" or structural vector autoregressions (SVARs) for identification, and those using variation in state or regional tax policies.[3]

Romer and Romer (2010) are the leading proponents of the narrative approach to using macro data. 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."[4] These include tax increases to deal with an inherited budget deficit and tax cuts aimed at boosting long-term growth. They stand in

[3] Another camp uses cross-country data. See Kneller, Bleaney, and Gemmell (1999), Lee and Gordon (2005), Djankov et al. (2010), and Hassett and Mathura (2015). Yagan (2015) studies the effect of the 2003 U.S. dividend cut by comparing the response of C-corporations, which are subject to dividend taxation, to S-corporations, which are not.

[4] 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).

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 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%.[5]

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.[6]

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

[5] Zidar (2015) uses the Romer and Romer measure to study the heterogeneous impact of federal personal income taxes.
[6] For a general overview of the identification challenges associated with SVARs, see Stock and Watson (2001).

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.

While we are the first to study the effects of state corporate taxes on employment and income using a border-discontinuity research design, our approach relates to other studies investigating the effects of state corporate tax policies. Goolsbee and Maydew (2001) investigate the effect of changes in state apportionment formulae on employment and find that a reduction in the payroll weight increases employment in manufacturing. Moretti and Wilson (2015) show that higher corporate taxes only induce migration of star scientists if the wage bill enters the state's apportionment formula. 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. They find that approximately a third of corporate tax incidence is borne by workers.[7] Giroud and Rauh (2015) use establishment-level data to study the reaction of large multi-state firms (those with at least 100 employees) to changes in state corporate taxes. They estimate that employment and establishment counts have corporate tax elasticities of -0.4.

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). Subsequent papers in this tradition include Reed and Rogers (2004), who investigates the effect of a large reduction in New Jersey's personal income tax, as well as Reed (2008) and Gale, Krupkin, and Rueben (2015) who present more generalized findings.

Most of this 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

[7] Using data from Germany, Siegloch, Fuest, and Peichl (2015) estimate a 40% incidence rate.

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 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.[8] Second, economic conditions 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

[8] 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.

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:

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

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:

$$\Delta Y_{c,s,t} = \beta \Delta T^{+}_{s,t} + \gamma \Delta T^{-}_{s,t} + \delta \Delta X_{c,s,t} + \alpha_{g,t} + \varepsilon_{c,s,t} \tag{2}$$

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.[9]

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

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

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

[9] 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.

experienced a tax change.[10] Appendix A provides further details of our coding choices.

There are 3,132 counties in the U.S.[11] 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 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.[12] 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.

[10] 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.

[11] 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.

[12] 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.

3. State Corporate Income Taxes

Most states tax corporate activities within their borders,[13] usually by taxing profits.[14] 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.[15]

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 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).[16]

3.1 Variation in State Corporate Income Taxes

There is substantial variation in state corporate income taxes over time. To illustrate general

[13] 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.
[14] The exceptions, as of 2013, are OH, TX, and WA, which use a gross receipts tax assessed on revenue rather than on income.
[15] 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.
[16] 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.

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.[17] In any given year, on average 6.1% of states increase taxes (=140/(45 years*51 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

[17] 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.

since 2010. Figure 2 reveals little geographical clustering for either tax increases or tax cuts.[18]

3.2 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

[18] 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).

account for only around 5% of state government revenues in 2013.[19] A similar point is made by Suarez Serrato and Zidar (2015).

3.3 Anticipation Effects

If firms plan their policies based on the taxes they expect to pay in the future, their observed responses to an actual tax change may not uncover the causal effect of taxes on their behavior. To see why, consider a tax increase that turns out smaller than expected. This may cause corporate policy to change in a way normally associated with a tax *cut* (since the tax rate increased by less than expected). This would in turn confound the interpretation of the observed treatment effect (given that the econometrician does not observe the firm's expectations).

As Hennessy and Strebulaev (2015) note, this identification challenge arises only when the corporate policy in question is subject to adjustment costs, so that the firm must plan ahead in order to reach its desired position over time given its expectations. The extent to which this applies to firms' employment decisions is an empirical question. Besides the absence of adjustment costs, there is one (somewhat obvious) scenario for when anticipation effects do not pose an identification challenge: if policy changes are unanticipated. As Hennessy and Strebulaev (2015) state more formally, a necessary and sufficient condition for correct inference about causal effects is that the policy variable is a Martingale. Translated to our context, this means that state tax rates follow a random walk. In this section, we test this condition.

We first test for a unit root in corporate tax rates in each state in isolation, using augmented Dickey-Fuller tests. As Table 2 shows, we fail to reject the presence of a unit root in each state and in DC, which suggests that corporate tax rates follow a random walk in every state.

We then take into account that some states condition their tax policy on the tax policies of their neighbors (Heider and Ljungqvist 2015), which would give rise to cross-sectional

[19] See Figure 1 in http://www2.census.gov/govs/statetax/2013stcreport.pdf.

dependence in tax rates across states. Standard panel unit root tests such as Breitung's can accommodate such cross-sectional dependence. The null is that all time series in a regional "cluster" contain a unit root. The alternative hypothesis is that one or more of the series are stationary. We define clusters in two ways: either as a state and its contiguous neighbors (giving 49 clusters, including DC's but excluding Alaska and Hawaii) or as states that are located in a given Census region. We fail to reject the null at standard significance levels everywhere except in Connecticut and in Massachusetts and their respective contiguous neighbors. Within Census regions, the null is never rejected at the 5% level; it is rejected at the 10% level in New England.

Overall, these results suggest that changes in state corporate tax rates are largely unanticipated, echoing Barro's (1990) finding that federal taxes follow a random walk.

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.[20] 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

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

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 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 B.

Our baseline employment measure is the number of full-time and part-time jobs in a county scaled by county population.[21] Employment is measured by the location of the place of work, not

[21] 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.

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 exclude the self-employed.

4.2 Pre-Trends and Unobserved Local Economic Conditions

Table 3 provides summary statistics of our dependent variables for treated and control border counties and for their hinterland counties (i.e., counties located in a state's interior that border a border county). Each statistic is measured the year before a tax change.

Panel A shows that 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).[22]

[22] 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.

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 3 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 3 also compares growth rates in border counties and their hinterland counties in the state's interior. In treated states, growth rates in both income and employment are statistically little different in border and hinterland counties. In control states, employment growth is similarly little different (except in the farming sector), but income in control states grows significantly faster in hinterland counties than in border counties, by around a quarter of a percentage point in the year before a tax change.

Thus, while Table 3 suggests that the parallel-trends assumption holds for bordering counties in adjacent states, it does not necessarily hold for border and hinterland counties within the same state. The fact that economic conditions can vary even within a state underlines the importance of focusing the analysis on border counties, rather than on neighboring states, to control for local economic conditions and thereby establish plausible counterfactuals. 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 4 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)).[23] Economically, a one percentage-point increase (cut) in the 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 and instead includes year fixed effects only.[24] This significantly increases the estimated tax effect by 39.4%, to –0.336 (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-

[23] 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.

[24] 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.

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.[25] The asymmetry in the tax sensitivity is marginally statistically significant (p=0.081).

Our baseline employment measure is essentially a form of employment rate: the number of jobs scaled by total county population. The employment rate could fall as corporate taxes increase because the number of jobs falls or because county population increases (or both). Column 4 models the change in the raw number of jobs. The point estimate for tax increases is –0.289 (p<0.001), virtually identical to the baseline estimate of –0.282 in column 3. The point estimate for tax cuts is similarly unchanged. This suggests that county population stays unchanged around tax changes, something we confirm in unreported tests. By implication, local area unemployment rates must increase following a corporate tax increase.[26]

A constant population may hide changes in a county's demographic make-up. Specifically, 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) while workers migrate away from the county. To investigate this possibility, column 5 scales employment by a measure of the size of the workforce (namely, the number of

[25] 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.
[26] We do not model local area unemployment rates directly, for two reasons. First, local unemployment data are only available from 1990. Second, these data come from household surveys, unlike the BEA data on county employment and income we use, which come from administrative payroll tax records and so are likely more accurate.

county residents aged between 20 and 70). The point estimate of –0.288 (p<0.001) is virtually identical to that in 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 6 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 7 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 5 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.[27] 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 replaces the contiguous-border-county-group/year fixed effects with year fixed effects. Doing so significantly increases the estimated tax effect to –0.523% (p<0.001), 42.5% greater (in absolute terms) than in the baseline. 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 4, 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).[28] 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

[27] None of the demographic controls is significant at the 5% level.
[28] 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 C explains, the BEA adjusts farmers' income for changes in the value of inventories. Farmers' income itself is not sensitive to corporate tax cuts.

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 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. A third concern is that our estimates may be confounded by secular place-by-time trends, specifically, the decline in manufacturing in the rust belt. Close inspection of Figure 2 shows that some of the largest tax increases are concentrated in the industrial Midwest in the 1970s and 1980s, an area which suffered declines in industrial activity, increasing budget deficits, and tax increases to balance these budgets.

Table 6 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. Columns 5 and 6 exclude treated counties in the industrial Midwest (OH, PA, IN, MI, IL, and WV) in the 1970s and 1980s. Doing so reduces the point estimates for tax increases by around a tenth compared to the baseline, though the point estimates remain statistically significant and economically large.[29]

Table 7 presents further robustness checks. A natural interpretation of the results in Tables 4 and 5 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

[29] Excluding other potentially influential observations similarly leaves our results unaffected. For example, restricting the sample to tax increases and tax cuts of commensurate size, dropping tax increases and tax cuts larger than two or five percentage points yields point estimates for tax increases that are at least as large and statistically significant as those found in the baseline and point estimates for tax cuts that are no larger than those in the baseline.

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 7 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 4 and 5 – 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.[30] 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 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

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

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.[31] 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.[32] 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 8 estimates the effects of increases 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 8 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

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

[32] 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).

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).[33]

In either case, as Table 9 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, 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 10 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

[33] 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)).

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.[34]

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 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: reduced spending by treated households would spill over 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 hinterland counties in the interior of the control state that due to their distance from the border are less affected by the spillover.[35]

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[34] 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).

[35] 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.

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. It is even possible that the overall level of employment remains unchanged as jobs are simply moved around the border region.

To the extent that transport costs are an issue, border counties in the control state should benefit more from corporate spillovers than the control state's hinterland counties. Thus, a comparison of border and hinterland counties in the control state can shed light on both household and corporate spillovers.

Finally, 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 have little impact (outside recessions) 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 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 hinterland counties in control states which we compare to those border counties in the same control state that neighbor a treated state that increases (columns 1 and 3 of Table 11) or cuts its corporate tax rate (columns 2 and 4).[36] The household spending channel implies that hinterland 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 hinterland 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 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

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

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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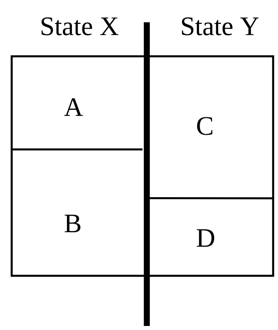
Appendix A. Treatments and Controls.

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.[37]

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.[38] That is, B, C, and D form a "group" of contiguous border counties, with B being treated and C and D serving as controls.



[37] 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.
[38] 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. Variable Definitions.

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.

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.

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 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 C. 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 4 and 5 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,[39] 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.

[39] Since farms employ relatively few workers, this effect is not large enough to raise total county employment, not even, as we show in Table 10, 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 4 and 5. In columns 1 and 2, we repeat our baseline estimates from column 3 of Tables 4 and 5, 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 4 and 5. 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 B. 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.

	Baseline measures: column 3 from Tables 4 and 5		County total (including farms and self-employed)				
Dep. var.: Change in log	employment (1)	income (2)	employment (3)	income (4)	Farm employment only (5)	Farm income (6)	Farm income (excl. inventory) (7)
magnitude of tax increase	-0.282***	-0.420***	-0.256**	-0.403***	-0.083	-0.678	-1.069
	0.075	*0.087*	*0.072*	*0.199*	*0.174*	*1.505*	*1.924*
magnitude of tax cut	0.065	0.132	0.081	2.191**	1.294***	8.242**	-2.618
	0.128	*0.175*	*0.123*	*0.934*	*0.415*	*3.310*	*3.320*
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)

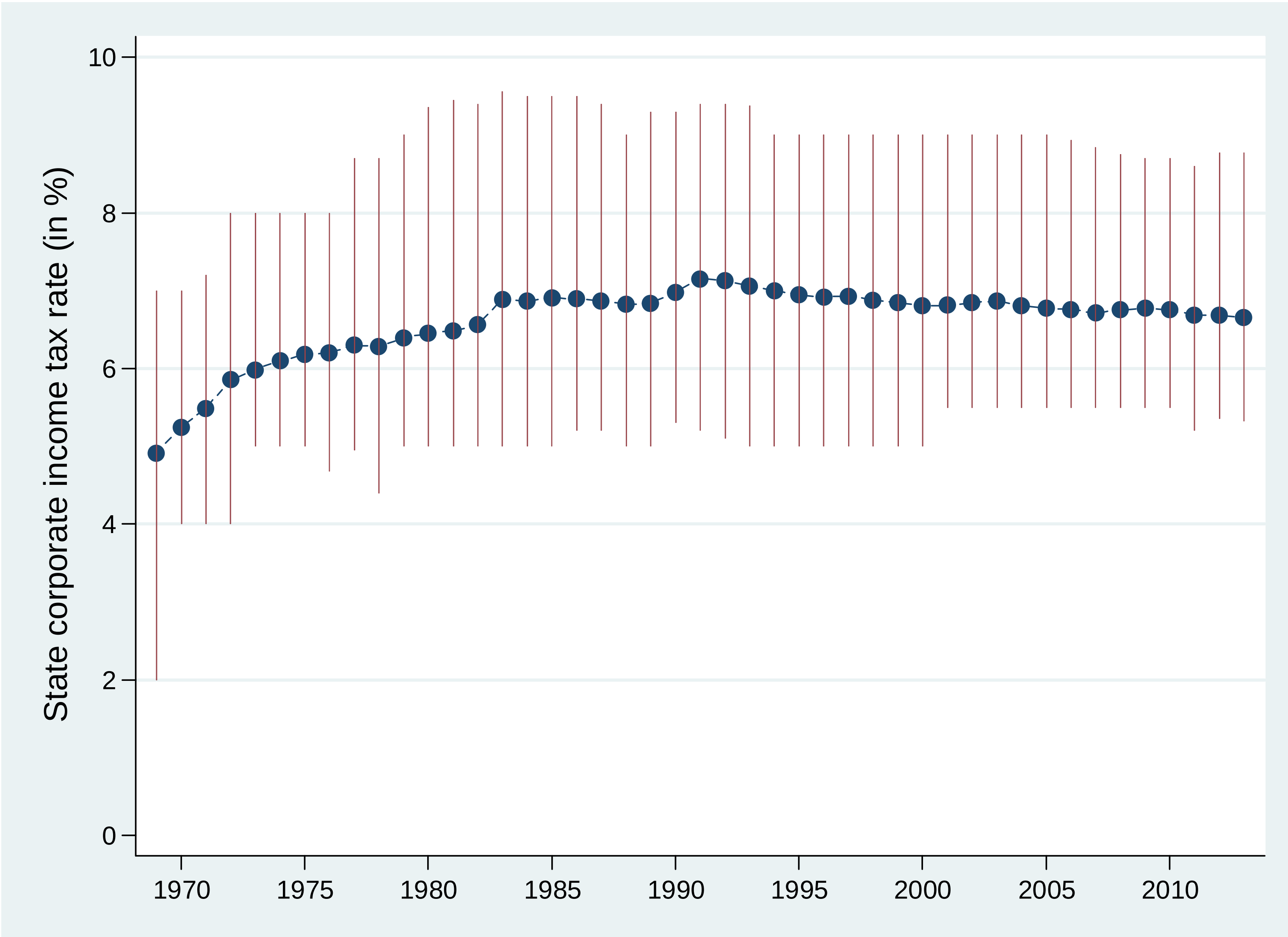


Figure 2. Distribution of Corporate Tax Changes Across States and Time
To make the figure readable, we perturb the magnitudes of the tax changes by at most 20bps in some instances.

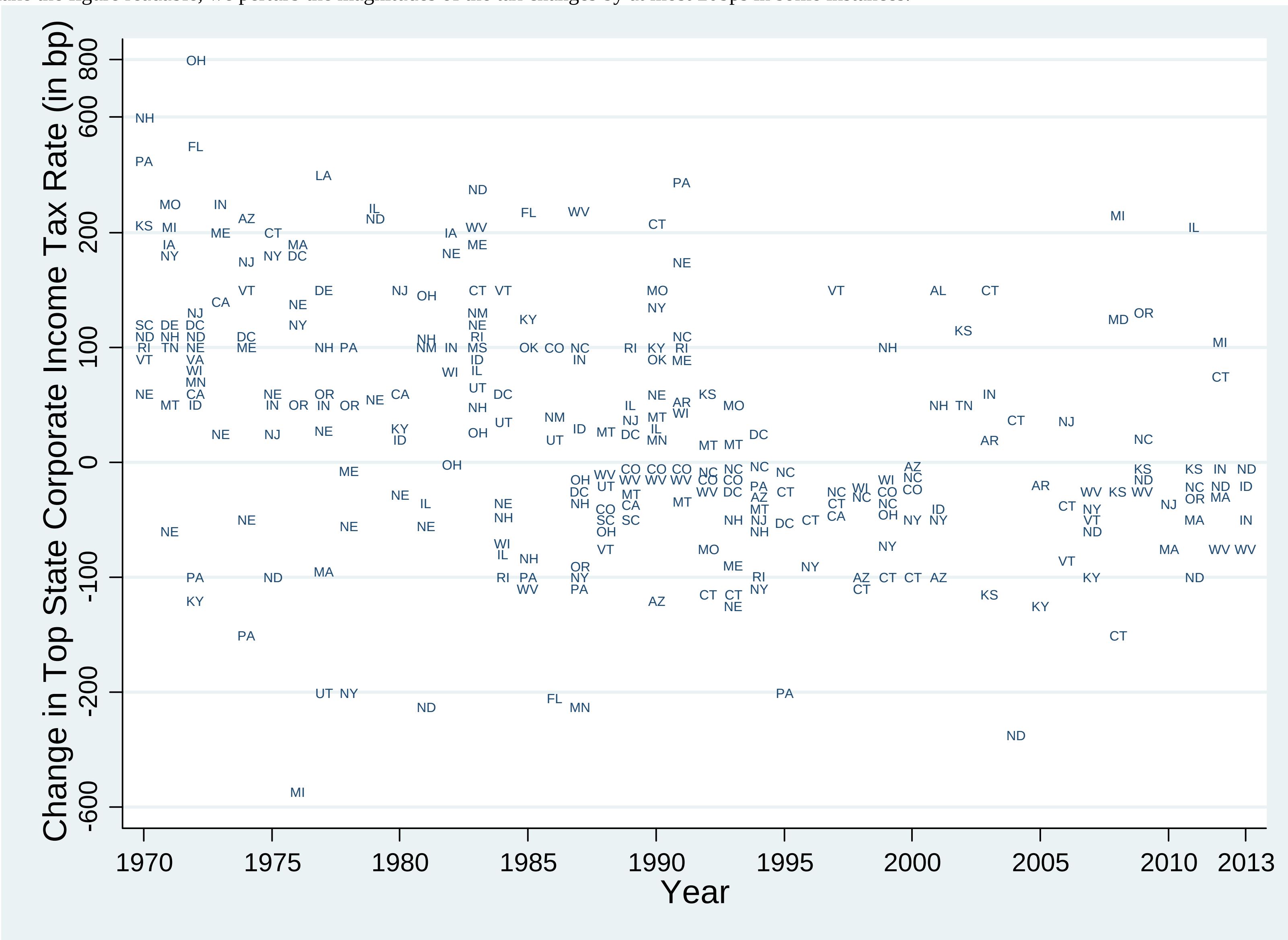


Figure 3. Impact Response Functions

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 4 and 5.

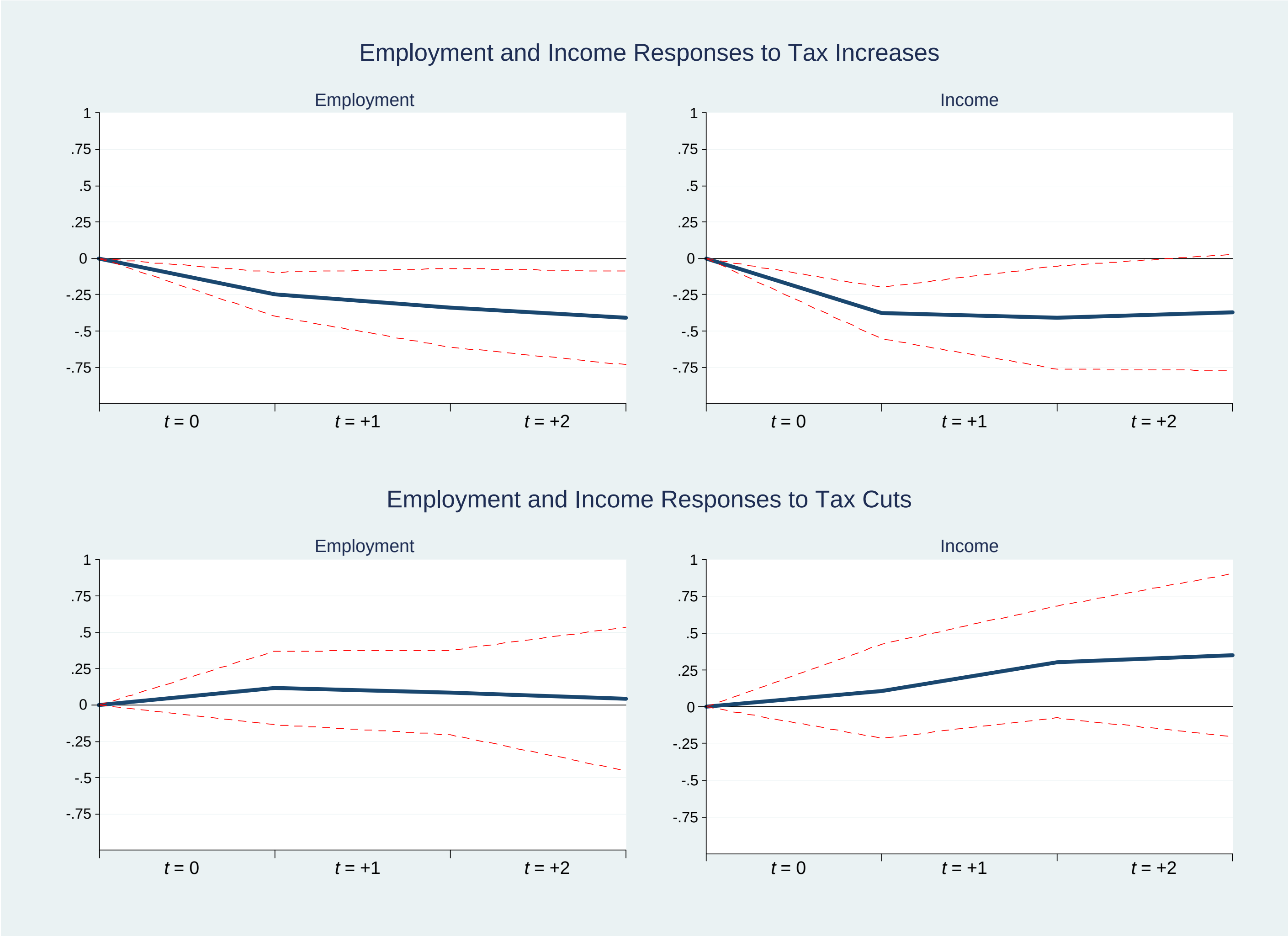


Table 1. Summary Statistics and Coincident State-level Policy Changes.

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 C 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 B.

		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. Unit Root Tests of State Corporate Income Tax Rates.

We test for the presence of unit roots in state corporate income tax rates using augmented Dickey-Fuller tests on each individual state's time series over the period 1969-2013 and in clusters of states using a Breitung panel unit root test that allows for cross-state dependence in tax rates. Clusters are defined either as a state and its contiguous neighbors (giving 49 clusters, including DC's but excluding Alaska and Hawaii) or as states that are located in a given Census region. The critical values for the Dickey-Fuller test are -2.63 (1%), -1.95 (5%), and -1.608 (10%).

	Augmented Dickey-Fuller	Breitung panel unit root tests				
		state and its neighbors		by Census region		
State	test stat.	test stat.	*p*-value	Census region	test stat.	*p*-value
AK	-0.103	n.a.		Div. 9: West: Pacific	-0.886	0.188
AL	0.916	-0.158	0.437	Div. 6: South: East South Central	-0.829	0.204
AR	0.825	-0.414	0.339	Div. 7: South: West South Central	-0.208	0.418
AZ	-0.446	-0.448	0.327	Div. 8: West: Mountain	-0.482	0.315
CA	0.841	-0.845	0.199	Div. 9: West: Pacific	-0.886	0.188
CO	-0.419	-0.022	0.491	Div. 8: West: Mountain	-0.482	0.315
CT	-0.066	-1.904	0.028	Div. 1: New England	-1.470	0.071
DC	1.162	0.392	0.653	Div. 5: South: South Atlantic	-0.340	0.367
DE	1.118	-0.108	0.457	Div. 5: South: South Atlantic	-0.340	0.367
FL	-0.107	-0.089	0.465	Div. 5: South: South Atlantic	-0.340	0.367
GA	n.a.	-0.068	0.473	Div. 5: South: South Atlantic	-0.340	0.367
HI	0.867	n.a.		Div. 9: West: Pacific	-0.886	0.188
IA	1.122	0.008	0.503	Div. 4: Mid-West: West North Central	-0.233	0.408
ID	0.922	-0.724	0.235	Div. 8: West: Mountain	-0.482	0.315
IL	0.556	-0.513	0.304	Div. 3: Mid-West: East North Central	-0.263	0.396
IN	0.797	-0.268	0.394	Div. 3: Mid-West: East North Central	-0.263	0.396
KS	0.512	0.021	0.508	Div. 4: Mid-West: West North Central	-0.233	0.408
KY	-0.536	-0.310	0.378	Div. 6: South: East South Central	-0.829	0.204
LA	0.534	-0.166	0.434	Div. 7: South: West South Central	-0.208	0.418
MA	-0.332	-2.246	0.012	Div. 1: New England	-1.470	0.071
MD	0.978	-0.586	0.279	Div. 5: South: South Atlantic	-0.340	0.367
ME	0.995	-0.647	0.259	Div. 1: New England	-1.470	0.071
MI	-0.735	-0.125	0.450	Div. 3: Mid-West: East North Central	-0.263	0.396
MN	-0.698	-0.681	0.248	Div. 4: Mid-West: West North Central	-0.233	0.408
MO	0.586	-0.279	0.390	Div. 4: Mid-West: West North Central	-0.233	0.408
MS	0.847	-0.470	0.319	Div. 6: South: East South Central	-0.829	0.204
MT	0.477	-1.204	0.114	Div. 8: West: Mountain	-0.482	0.315
NC	0.433	-0.026	0.490	Div. 5: South: South Atlantic	-0.340	0.367
ND	-0.440	-1.213	0.113	Div. 4: Mid-West: West North Central	-0.233	0.408
NE	0.827	0.201	0.580	Div. 4: Mid-West: West North Central	-0.233	0.408
NH	0.291	-0.519	0.302	Div. 1: New England	-1.470	0.071
NJ	1.198	-0.253	0.400	Div. 2: Middle Atlantic	-0.210	0.417
NM	1.371	-0.333	0.370	Div. 8: West: Mountain	-0.482	0.315
NV	n.a.	-0.911	0.181	Div. 8: West: Mountain	-0.482	0.315
NY	-0.210	-0.840	0.200	Div. 2: Middle Atlantic	-0.210	0.417
OH	0.040	-0.276	0.391	Div. 3: Mid-West: East North Central	-0.263	0.396
OK	1.221	0.066	0.526	Div. 7: South: West South Central	-0.208	0.418
OR	0.735	-0.956	0.170	Div. 9: West: Pacific	-0.886	0.188
PA	-0.008	-0.345	0.365	Div. 2: Middle Atlantic	-0.210	0.417
RI	0.613	-1.574	0.058	Div. 1: New England	-1.470	0.071
SC	-0.111	-0.263	0.396	Div. 5: South: South Atlantic	-0.340	0.367
SD	n.a.	-0.324	0.373	Div. 4: Mid-West: West North Central	-0.233	0.408
TN	1.181	-0.303	0.381	Div. 6: South: East South Central	-0.829	0.204
TX	n.a.	-0.342	0.366	Div. 7: South: West South Central	-0.208	0.418
UT	-0.711	-0.417	0.338	Div. 8: West: Mountain	-0.482	0.315
VA	0.839	-0.865	0.194	Div. 5: South: South Atlantic	-0.340	0.367
VT	0.752	-1.518	0.065	Div. 1: New England	-1.470	0.071
WA	n.a.	-0.990	0.161	Div. 9: West: Pacific	-0.886	0.188
WI	0.450	-0.643	0.260	Div. 3: Mid-West: East North Central	-0.263	0.396
WV	-0.020	-0.244	0.404	Div. 5: South: South Atlantic	-0.340	0.367
WY	n.a.	0.167	0.566	Div. 8: West: Mountain	-0.482	0.315

Table 3. Summary Statistics.

Panel A provides summary statistics for our dependent variables in all treated and control border counties and in hinterland counties (i.e., counties located in a state's interior that border a border county), 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 B. The final three columns report differences-in-means. To estimate statistical significance, we use *t*-tests. *** and ** denote significance at the 1% and 5% level (two-sided), respectively.

	Border county-years				Hinterland county-years				Differences in means		
	in treated states (N = 3,390)		in control states (N = 3,650)		in treated states (N = 1,707)		in control states (N = 4,410)		Border: treated-controls	Treated: border-hinterland	Control: border-hinterland
	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*	0.331	*0.126*	0.346	*0.141*	-0.012***	0.018***	0.016***
wage employment/cnty. pop'n aged 20-70	0.587	*0.183*	0.606	*0.216*	0.554	*0.196*	0.572	*0.219*	-0.019***	0.033***	0.034***
wage employment excl. govt./cnty. pop'n	0.270	*0.111*	0.282	*0.137*	0.263	*0.117*	0.274	*0.124*	-0.012***	0.007**	0.009***
self-employment/county population	0.083	*0.039*	0.080	*0.035*	0.073	*0.028*	0.078	*0.036*	0.003**	0.010***	-0.002**
farm employment/county population	0.056	*0.060*	0.054	*0.058*	0.047	*0.047*	0.047	*0.045*	0.002*	0.010***	0.007***
Income (in 2005 dollars)											
wage income/county population	11,271	*7,498*	11,695	*7,838*	10,638	*6,543*	11,175	*7,618*	-423**	634***	520***
wage income excl. govt./cnty. pop'n	8,536	*6,363*	8,958	*6,846*	8,341	*5,813*	8,621	*6,377*	-423***	195	337**
wage income incl. transfers/cnty. pop'n	14,821	*8,093*	15,166	*8,265*	13,987	*7,131*	14,756	*8,141*	-345	834***	410***
wage income (residence) /cnty. pop'n	12,524	*5,623*	12,907	*5,725*	12,438	*5,301*	13,221	*5,493*	-384***	86	314**
self-employed income/cnty. pop'n	1,720	*1,077*	1,664	*897*	1,501	*757*	1,594	*857*	55**	218***	71***
farm income/cnty. pop'n	1,182	*2,199*	1,198	*2,281*	853	*1,743*	880	*1,709*	-16	329***	318***
Panel B. Log changes x 100 (year prior to tax change)											
Employment											
wage employment /county population	0.407	*4.400*	0.449	*4.729*	0.510	*4.083*	0.474	*4.463*	-0.042	-0.103	-0.025
wage employment/cnty. pop'n aged 20-70	0.068	*4.432*	0.071	*4.777*	0.083	*4.117*	0.065	*4.505*	-0.003	-0.014	0.006
wage employment excl. govt./cnty. pop'n	0.350	*5.554*	0.416	*6.012*	0.510	*5.419*	0.493	*5.505*	-0.066	-0.160	-0.077
self-employment/county population	1.477	*7.985*	1.597	*7.413*	1.562	*7.698*	1.893	*8.775*	-0.120	-0.085	-0.296
farm employment/county population	-2.086	*5.311*	-1.861	*5.654*	-1.992	*5.352*	-2.259	*5.601*	-0.225	-0.094	0.398***
Income											
wage income/county population	1.797	*6.118*	1.793	*6.459*	1.786	*5.751*	2.069	*5.769*	0.005	0.011	-0.276**
wage income excl. govt./cnty. pop'n	1.695	*7.848*	1.660	*8.489*	1.684	*7.787*	2.087	*7.343*	0.035	0.011	-0.428**
wage income incl. transfers/cnty. pop'n	2.380	*4.670*	2.345	*5.084*	2.475	*4.098*	2.551	*4.274*	0.035	-0.095	-0.206
wage income (residence) /cnty. pop'n	1.912	*5.022*	1.786	*5.157*	1.803	*4.161*	2.135	*4.274*	0.126	-0.109	-0.348***
self-employed income/cnty. pop'n	-0.047	*12.430*	-0.181	*13.037*	0.099	*11.957*	0.560	*12.726*	0.134	-0.146	-0.742**
farm income/cnty. pop'n	-0.717	*68.448*	-0.704	*68.079*	-0.715	*79.115*	-3.624	*70.439*	-0.013	0.003	2.920

Table 4. 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. Column 4 models the raw number of jobs. Column 5 scales jobs by the number of county residents aged between 20 and 70. Column 6 excludes government employment from our baseline employment measure, scaled by total county population. Column 7 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 B. 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.

	Dep. var.: Change in log employment						
	scaled by total county population			unscaled	scaled by population aged 20-70	excluding govt. employ-ment	self employ-ment only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ tax rate	-0.241***	-0.336***					
	0.065	*0.060*					
magnitude of tax increase			-0.282***	-0.289***	-0.288***	-0.289***	-0.183
			0.075	*0.070*	*0.075*	*0.086*	*0.140*
magnitude of tax cut			0.065	0.105	0.100	0.008	0.299
			0.128	*0.129*	*0.126*	*0.146*	*0.349*
Demographic controls	x	x	x	x	x	x	x
Group–year fixed effects	x		x	x	x	x	x
Year fixed effects		x					
χ^2 test: (2) > (1)?		2.43*					
F test: |inc.| > |cut|?			2.03*				
Adjusted R^2	9.1%	9.6%	9.1%	15.5%	8.8%	10.0%	16.1%
Number of county-years	10,366	7,040	10,366	10,366	10,334	10,366	10,366

Table 5. 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 B. 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.

	Dep. var.: Change in log income						
				excluding govt.	including transfers	by place of residence	self employ-ment only
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Δ tax rate	-0.367***	-0.523***					
	0.072	*0.092*					
magnitude of tax increase			-0.420***	-0.422***	-0.307***	-0.247***	-0.165
			0.087	*0.098*	*0.091*	*0.070*	*0.189*
magnitude of tax cut			0.132	-0.014	0.088	0.146	0.518
			0.175	*0.216*	*0.135*	*0.141*	*0.444*
Demographic controls	x	x	x	x	x	x	x
Group–year fixed effects	x		x	x	x	x	x
Year fixed effects		x					
χ^2 test: (2) > (1)?		2.78**					
F test: |inc.| > |cut|?			1.88*				
Adjusted R^2	20.9%	17.5%	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 6. Politically Influential Counties, Distant County Pairs, the Rust Belt, and County Size.

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. Columns 5 and 6 remove treated "rust belt" counties in the industrial Midwest (in OH, PA, IN, MI, IL, and WV) which in the 1980s suffered declines in industrial activity and tax increases to balance state budgets. Columns 7 and 8 report estimates weighted by county employment measured the year before a tax change. 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 B. 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.

	"Non-influential" counties only		"Close" counties only		Non "rust belt" counties only		Weighted by employment	
Dep. var.: Change in log	employment (1)	income (2)	employment (3)	income (4)	employment (5)	income (6)	employment (7)	income (8)
magnitude of tax increase	-0.282***	-0.432***	-0.329**	-0.433**	-0.258**	-0.387***	-0.223**	-0.217**
	0.078	*0.089*	*0.152*	*0.188*	*0.120*	*0.124*	*0.108*	*0.097*
magnitude of tax cut	0.055	0.126	0.016	0.121	0.135	0.188	0.049	0.273
	0.136	*0.185*	*0.376*	*0.603*	*0.145*	*0.206*	*0.117*	*0.208*
Demographic controls	x	x	x	x	x	x	x	x
Group–year fixed effects	x	x	x	x	x	x	x	x
Adjusted R^2	8.0%	19.6%	25.5%	30.9%	7.5%	19.6%	19.3%	35.1%
Number of observations	9,580	9,580	1,515	1,515	9,032	9,032	10,366	10,366

Table 7. 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 B. 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.

	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		
Dep. var.: Change in log	employ-ment (1)	income (2)	employ-ment (3)	income (4)	employ-ment (5)	income (6)	employ-ment (7)	income (8)	income (9)
magnitude of tax increase	-0.332***	-0.274***	-0.255***	-0.411***	-0.516**	-0.605***	-0.184**	-0.311***	-0.302**
	0.106	*0.094*	*0.076*	*0.091*	*0.212*	*0.214*	*0.079*	*0.100*	*0.125*
magnitude of tax cut	-0.038	0.282	0.140	0.178	-0.201	-0.385	-0.022	-0.054	-0.121
	0.276	*0.282*	*0.151*	*0.179*	*0.549*	*0.772*	*0.201*	*0.287*	*0.182*
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 8. 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 B. 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.

	Conditioned on national recession		Conditioned on regional recession		Conditioned on state recession	
Dep. var.: Change in log	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***
	0.125	*0.167*	*0.106*	*0.147*	*0.100*	*0.130*
x no recession	-0.309***	-0.426***	-0.303***	-0.388***	-0.267***	-0.412***
	0.089	*0.092*	*0.091*	*0.095*	*0.090*	*0.106*
Magnitude of tax cut						
x recession	0.639***	1.000	0.597**	1.173*	0.681**	1.166**
	0.213	*0.635*	*0.240*	*0.603*	*0.268*	*0.513*
x no recession	-0.081	-0.089	-0.043	-0.079	-0.136	-0.204
	0.149	*0.185*	*0.143*	*0.167*	*0.156*	*0.184*
Demographic controls	x	x	x	x	x	x
Group–year fixed effects	x	x	x	x	x	x
Wald test (*F*):						
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 R^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 9. 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 B. 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.

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

Table 10. 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 4 and 5. For variable definitions and details of their construction, see Appendix B. Standard errors are clustered at the state level. ***, **, and * denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Conditioned on per capita personal income		Conditioned on population density		Conditioned on small-business employment share	
Dep. var.: Change in log	employment (1)	income (2)	employment (3)	income (4)	employment (5)	income (6)
Magnitude of tax increase						
x rich county	-0.271***	-0.444***				
	0.088	*0.122*				
x poor county	-0.297**	-0.386***				
	0.134	*0.141*				
x high-density county			-0.245***	-0.385***		
			0.076	*0.093*		
x low-density county			-0.354**	-0.488***		
			0.141	*0.166*		
x high small-business shr.					-0.282	-0.463***
					0.174	*0.172*
x low small-business shr.					-0.319**	-0.582***
					0.130	*0.163*
Magnitude of tax cut						
x rich county	0.125	-0.040				
	0.154	*0.173*				
x poor county	-0.039	0.428				
	0.211	*0.288*				
x high-density county			-0.018	-0.068		
			0.132	*0.149*		
x low-density county			0.127	0.284		
			0.191	*0.247*		
x high small-business shr.					0.172	0.217
					0.158	*0.216*
x low small-business shr.					-0.040	-0.172
					0.120	*0.195*
Demographic controls	x	x	x	x	x	x
Group–year fixed effects	x	x	x	x	x	x
Wald test (*F*):						
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 R^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 11. 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 hinterland counties (i.e., counties located in a state's interior that border a border county), which being further away from the border should be less affected by the spillover. Corporate spillovers imply the opposite. To test this, columns 1 through 4 compare hinterland 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 4 and 5 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 B. 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 (1)	around tax cut in neighboring state (2)	around tax increase in neighboring state (3)	around tax cut in neighboring state (4)	employment (5)	income (6)
=1 if hinterland county	-0.016	-0.109	0.015	-0.169		
	0.142	*0.143*	*0.194*	*0.210*		
magnitude of tax increase						
x control county has lower tax					-0.278***	-0.405***
					0.074	*0.094*
x control county has higher tax					-0.459*	-0.554**
					0.229	*0.224*
magnitude of tax cut						
x control county has lower tax					0.011	0.202
					0.215	*0.243*
x control county has higher tax					0.201	0.104
					0.250	*0.288*
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 (*F*):						
Tax increase: higher = lower					0.64	0.43
Tax cut: higher = lower					0.25	0.06
Adjusted R^2	15.5%	17.2%	23.2%	20.9%	9.1%	20.9%
Number of county-years	4,141	3,890	4,141	3,890	10,366	10,366