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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 wage income, while corporate tax cuts only boost economic activity if implemented during recessions. 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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1. Introduction

Despite the recent enactment of a large cut in the federal corporate tax rate,[1] the effects that corporate taxes have on economic activity remains controversial. Academic studies, for example, have found mixed evidence on how companies respond to changes in corporate tax rates,[2] while opinion among public commentators appears starkly divided. This controversy in large part stems from the fact that the effects of corporate tax changes are so difficult to measure empirically.

The challenges are twofold. First, changes in tax policy are unlikely to be random and instead may themselves be influenced by prevailing economic conditions and other factors. For example, governments may cut taxes in the hope of stimulating sluggish growth. As a result, observed correlations between changes in tax policy and ex post economic outcomes 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. Moreover, tax reform at the federal level often involves highly complex pieces of legislation affecting numerous aspects of the tax code, making it nearly impossible to evaluate the effects attributable to a change in the corporate tax rate, while holding all else equal.

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 outcomes such as employment and income would have evolved had the tax change not occurred. The absence of a counterfactual

[1] Among its many provisions, the Tax Cuts and Jobs Act of 2017 cut the statutory federal corporate income tax rate from 35% to 21%.

[2] For example, Mertens and Ravn (2013) find that federal corporate tax cuts have significant effects on aggregate investment, but not on employment, while Hassett and Hubbard (2002) conclude that most studies find limited effects of federal corporate tax changes on investment. At the state level, Giroud and Rauh (2017) find notable effects of corporate tax changes on employment. We further review the literature below.

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 two key elements. First, we focus on individual U.S. states rather than the federal level. In contrast to federal corporate tax rates, which change only rarely,[3] changes in state-level corporate taxes are plentiful: we count 250 changes in state corporate income tax rates from 1970 to 2010.

The second element of our empirical strategy is designed to minimize the effects of confounding variation in economic conditions as much as possible. To this end, we exploit the fact that a state's tax jurisdiction stops at its border. This suggests that when a state undertakes a corporate tax change, its neighboring states, which arguably share similar economic conditions but where taxes haven't changed, might provide a suitable "control group" against which to measure the effects of the tax change. To get as close to a plausible counterfactual as possible, however, our tests compare not neighboring *states* but rather contiguous *counties* located on either side of a state border.[4] Because of their proximity to each other, these neighboring counties are likely to be experiencing similar local economic conditions prior to one them being affected by a tax change – and indeed, our tests validate that this "parallel trends" assumption holds in the data. Thus, by comparing outcomes in the "treated" county to its neighboring control county on the other side of the state border, we can effectively remove the biasing influence of otherwise unobservable variation in local economic conditions. Doing so allows us isolate the effects specifically attributable to the tax change. Essentially, our strategy is therefore a standard diff-in-diff approach that exploits a spatial discontinuity in tax policy for identification purposes.

[3] Since 1970, there have been only four major changes in the top federal corporate income tax rate: in 1979 (from 48% to 46%), in 1986 (from 46% to 34%), in 1993 (from 34% to 35%), and in 2018 (from 35% to 21%).

[4] Several prior studies have implemented the border-county methodology in other contexts, including Holmes (1998), Huang (2008), Dube, Lester, and Reich (2010), Heider and Ljungqvist (2015), and Hagedorn et al. (2016).

Our estimates suggest that a one percentage point corporate tax increase (cut) leads to employment in the affected county falling (rising) by about 0.2 percent and total wage income falling (rising) by about 0.3 percent (as measured relative to the neighboring county across the border).[5] Interestingly, we find evidence of asymmetric effects: tax increases are uniformly harmful, with point estimates similar in magnitude to those mentioned above for the symmetric specification. Our estimates for the effects on tax cuts, on the other hand, are not in general statistically significant. The one exception is when tax cuts are implemented during a recession; in this case, tax cuts lead to a sizeable positive response in both employment and wage income. The latter finding suggests that corporate tax cuts, when used counter-cyclically, may potentially be an effective policy tool in stimulating employment and income during economic downturns.

While a key strength of our empirical strategy is that treated and control border counties share similar local economic conditions and trends, this fact also raises a potential identification concern: tax changes on one side of the border could potentially trigger changes in the behavior of firms or households on the other. For example, since we find that tax increases reduce wage income, this could spill over to control counties to the extent that affected households spend less money not just at home but also in neighboring counties. This type of "household spending spillover" would attenuate the estimated effects, as the tax increase would hurt both the treated and the control county. Changes in corporate behavior ("corporate spillovers"), on the other hand, might 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, thus amplifying the estimated effects.

We test for whether such spillovers are likely to be present, and if so, whether they attenuate or amplify our estimates. Our tests are based on the observation that cross-border spillovers, if

[5] Information on other variables that might be of interest, such as investment, is not available at the county level.

present, will be stronger in control border counties and then dissipate the further we move into the interior of the control state.[6] We find no evidence of such geographic patterns, which gives us confidence that our estimates are unlikely to be biased due to household spending or corporate spillovers. This suggests that control border counties on average remain on their pre-treatment trend even after a tax change in a neighboring state. They hence provide a plausible set of counterfactuals for how economic conditions would have evolved in treated counties had they not been subject to a tax change, thus supporting a causal interpretation of our results.

Our findings survive many other robustness checks. For example, our results are unchanged if we control for local industry-level shocks; remove from the sample "influential" counties that account for more than 10% of a state's population; restrict the analysis to "close" border-county pairs, whose geographic midpoints are no more than 20 miles apart; focus only on corporate tax changes that do not coincide with other notable state-level policy changes; or restrict the sample to border county pairs with similar pre-treatment tax rates.

Our setting also permits us to study the heterogeneous impact of corporate tax changes. One interesting result to emerge from this analysis is that tax increases have more harmful effects in states that, prior to the tax increase, have low corporate tax rates. This is consistent with the notion that, prior to the tax increase, more tax-sensitive economic activity endogenously locates in such low-tax states. Thus, when a tax increase does occur, its effects are more harmful. To our knowledge, this type of non-linearity has not previously been documented.

Our paper contributes to the literature on the effects of tax policy on economic activity going back to Hall and Jorgensen (1967). Within this literature, much empirical work on how state taxes affect economic activity has focused on personal (as opposed to corporate) taxes, starting

[6] For example, if distance matters in firms' decisions where to relocate operations in response to a corporate tax increase, one would expect control border counties to be disproportionately impacted relative to "hinterland" counties in the control state.

with earlier studies by Helms (1985) and Mofidi and Stone (1990), to more recent papers by Reed and Rogers (2004), Reed (2008), and Gale, Krupkin, and Rueben (2015), to name but a few.[7] Two particularly relevant recent studies that focus on state-level corporate taxes are Suarez Serrato and Zidar (2016) and Giroud and Rauh (2017).[8] The former rely on state-level corporate tax changes to structurally estimate a spatial equilibrium model of firm and worker location choices, finding that approximately a third of corporate tax incidence is borne by workers.[9] Giroud and Rauh (2017) 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, estimating that employment and establishment counts have corporate tax elasticities of -0.4. On the whole, our estimates are quite similar to theirs, with differences likely due to differing methodologies.

Methodologically, our paper belongs to a growing literature that uses diff-in-diff or instrumental-variables estimators to study the effects of fiscal policy at the state level.[10] The strength of our research design is that allows us to run a relatively clean experiment. However, we caution against naïvely extrapolating our estimates to assess the possible impact of a federal corporate tax change. For one, tax changes at the federal level would induce general equilibrium effects that are essentially held constant in our local comparison of border-county pairs. Nonetheless, we believe that our results remain qualitatively useful in the context of the broader debate about corporate tax reform: in essence, our key finding is that companies appear to be

[7] There is also a voluminous literature on the economic effects of federal taxes. Studies with a particular focus on identification generally rely on either SVARs, as in Blanchard and Perotti (2002) and Mountford and Uhlig (2009), or "narrative identification," as in Romer and Romer (2010), Mertens and Ravn (2013) and Zidar (2018). Of these only Mertens and Ravn (2013) study corporate taxes. For alternative approaches, see Hassett and Hubbard (2002) and Yagan (2015). Yet 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).

[8] Other relevant papers include Goolsbee and Maydew (2001), who investigate how changes in state apportionment formulae affect employment; Chirinko and Wilson (2008), who find that more manufacturing establishments locate on the side of the state border with a lower user cost of capital state due to favorable investment tax credits; Felix and Hines (2009), who evaluate the effect of state corporate taxes on unionized workers' wages; and Moretti and Wilson (2017), who study how higher state corporate taxes induce migration of star scientists.

[9] Using data from Germany, Siegloch, Fuest, and Peichl (2018) estimate an incidence rate close to 50%.

[10] Recent examples focusing on government spending shocks include: Chodorow-Reich et al. (2012), Clemens and Miran (2012), Nakamura and Steinsson (2014), Fishback and Kachanovskaya (2015), and Shoag (2015).

highly responsive to changes in corporate taxes. Our methodology allows us to more confidently attribute these responses specifically to changes in corporate tax rates rather than other confounding factors. Since we are able to identify these strong responses relatively cleanly, our analysis suggests that companies are indeed highly sensitive to corporate tax incentives.

2. State Corporate Income Taxes

Most states tax corporate activities within their borders, usually by taxing profits.[11] 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 a state will affect all taxable corporate economic activity in that state, regardless of where the firms active in a state are themselves incorporated or headquartered.[12] State taxes are a meaningful part of corporations' overall tax burden. Heider and Ljungqvist (2015) estimate that in recent decades state taxes account for 21% of total income taxes paid by the average 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 1989-2010, we use the information listed in Appendices A and B of Heider and Ljungqvist (2015). 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).[13]

[11] The exceptions, as of 2010, are OH, TX, and WA, which use a gross receipts tax assessed on revenue rather than on income, and NV, SD, and WY, which did not levy either type of corporate tax. Throughout the paper, we restrict our analysis to mainland states, including DC.

[12] 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. As detailed below, we exploit this heterogeneity in several of our tests.

[13] 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.

2.1 Variation in State Corporate Income Taxes

There is substantial variation in state corporate income taxes over time. To illustrate general trends, Figure 1 plots the average tax rate over time, along with bands representing the 20^{th} to 80^{th} percentile range. Averaged across the states, tax rates increased from 5.1% in 1970 and have since plateaued at around 7%.

We identify 135 tax increases in 43 states (including DC) and 115 tax cuts in 32 states from 1970 to 2010.[14] Table 1, Panel A reports summary statistics. The mean tax increase measures 127 basis points, a 21% increase from a year earlier. The median tax increase is 100 basis points and the standard deviation is 115 basis points. The mean tax cut measures 68 basis points, an 8% reduction from a year earlier. The median tax cut is 50 basis points and the standard deviation is 74 basis points.

Figure 2 shows the distribution of corporate income tax changes over time. The pace of tax increases has slowed over time, while tax cuts have followed a hump-shaped pattern.

2.2 Coincident State-level Policy Changes

Like every state-level diff-in-diff, our identification strategy invites an important potential concern: corporate 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. 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 (2018) 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

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[14] We ignore a small number of cases involving states that switched from a tax on income to a tax on revenue or vice versa, the most prominent examples being Michigan, Ohio, and Texas.

sample coincide with the initiation of a state incentive program, or with a change in such a program. The overlap with state taxes on bank profits is more substantial. In all cases, however, we will show that 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. In practice, this is unlikely to confound our findings because receipts from corporate taxes account for a very small share of state government revenues. For example, corporate taxes accounted for only around 5% of state government revenues in 2010.[15] A similar point is made by Suarez Serrato and Zidar (2016).

2.3 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 (from all states) by an apportionment formula.[16] Apportionment formulae differ across states. As of 2010, ten 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. All other states use a three-factor formula based on an average of the proportions of sales, property, and employees attributable to the state; and of these, 22 states overweight the sales factor, while the remainder of states use an equally-weighted formula.

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

[15] See Figure 1 in http://www2.census.gov/govs/statetax/2010stcreport.pdf.
[16] I.e., 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.[17] 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. In practice, as shown in Table 1, Panel B, states virtually never change apportionment rules when they change tax rates.[18]

3. Sample and Data

3.1 Outcome Variables

We use data from the Bureau of Economic Analysis' (BEA) Regional Economic Accounts for the period 1969 to 2010, which report annual measures for various definitions of employment and income for each of the 3,132 counties in the U.S. 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.[19] Employment is measured by the location of the place of work, not by the worker's place of residence, and so includes workers commuting in from other counties.

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[17] See Appendix Table 3, columns 5 and 6.
[18] Data for changes in apportionment rules are taken from Bernthal et al. (2012), Appendix C, and start in 1978.
[19] This measure reflects the extensive margin of employment: whether or not the average county resident has a job. Data on the intensive margin – hours worked conditional on having a job – are not available at the county level.

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 wage 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 in the appendix.

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.

3.2 Summary Statistics and Pre-Trends in Local Economic Conditions

Table 2 provides summary statistics of our dependent variables for treated and control border counties and for their hinterland counties (defined as counties located in a state's interior that are one-removed from the border). 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. E.g., employment in treated counties averages 35% of county population, significantly below the mean of 36.2% in control counties. Per capita income is also significantly lower, by $423, in treated border counties.[20]

For identification purposes, what is more important than differences in levels is differences in growth rates. Identification relies on the parallel-trends assumption, which in our setting means that employment and income should on average grow at the same rates in treated and control

[20] The per capita income may seem low, but recall that we are scaling wage income by total county population and taking an equally weighted average across counties.

border counties in the absence of a corporate tax change. Panel B of Table 2 provides a first preliminary test of the parallel-trends assumption by comparing growth rates in the year before a tax change. As shown, 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. 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; this is accomplished by the "first difference" in our diff-in-diff approach.

4. 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, and 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 helps 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.[21] 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 is to difference away the confounding effects of unobserved local economic conditions.

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 5.5. In short, we find little evidence of such cross-border spillovers in our setting.

4.1 Regression Specifications

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

[21] 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.

$$\Delta\ln(Y)_{c,s,t} = \beta\Delta Tax_{s,t-1} + \delta\Delta X_{c,s,t} + \alpha_{g,t} + \varepsilon_{c,s,t} \quad (1)$$

where *c*, *s*, and *t* index counties, states, and time, respectively; Δ is the first-difference operator; $\ln(Y)_{c,s,t}$ is the outcome variable of interest, usually the natural log of either total county employment or total county wage income (both scaled by county population and measured as of the end of the calendar year); $\Delta Tax_{s,t-1}$ is the percentage-point change in state corporate tax rates (effective from the beginning of the calendar year, which is denoted with a *t*−1 subscript[22]); $X_{c,s,t}$ is a set of control variables; $\alpha_{g,t}$ are border-county-group-by-year fixed effects, with subscript *g* referencing a group of contiguous border counties; and $\varepsilon_{c,s,t}$ is the usual error term. Standard errors are clustered at the state level. For further information on variable definitions, see Appendix B.

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

$$\Delta\ln(Y)_{c,s,t} = \sum_{i=-2}^{3} \beta_i \, \Delta Tax_{s,t+i} + \delta\Delta X_{c,s,t} + \alpha_{g,t} + \varepsilon_{c,s,t} \quad (2)$$

using 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 to allow sufficient time for potential reversals or delayed responses to occur.

We estimate both equations (1) and (2) in first-differenced form, which effectively removes county- and state-specific fixed effects that would be present in the corresponding levels equations (i.e., prior to first-differencing). The specification thus allows us to easily accommodate repeated treatments (i.e., a county experiencing a sequence of tax increases or tax

[22] In a small number of instances, tax changes come into effect midway through the calendar year, rather than at the beginning. We code these the same way as the other tax changes.

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 the fixed effects, $\alpha_{g,t}$, 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. For further details on the construction of border-county sample and our estimation approach, see Appendix A.

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 Dynamic effects

We begin by presenting graphic evidence of the dynamic responses of employment and wage income to corporate tax changes, based on the estimation of equation (2). The results, presented in Figure 3, show the year-on-year percentage changes in total county employment (left panel) and total county wage income (right panel) in the years surrounding the corporate tax change, as measured relative to the unaffected neighboring counties on the other side of the state border.

In the years before a tax change (years -2 through 0), there are no statistically significant differences in employment and wage income growth for counties located in states whose tax rates later change compared with their cross-border neighbors. This finding supports the parallel-trends assumption: economic conditions are indeed roughly similar across state lines before a state changes its corporate tax rates. One year after a tax change becomes effective, however, a response is clearly evident. Our estimates suggest that a one percentage point tax increase (cut) leads to employment in the affected county falling (rising) by about 0.2 percent and total wage income falling (rising) by about 0.3 percent (as measured relative to the neighboring county across the border). In the following years, growth in employment and income returns to rates that are statistically indistinguishable from those in neighboring counties across the state border.

These findings suggest that changes in corporate tax rates effectively cause a level-shift in employment and total wage income one year later. Although this adjustment might seem fast, in our setting it might not be wholly surprising given that the state corporate tax changes in our sample are typically quite modest (averaging about one percentage point in magnitude, see Table 1). These modestly sized corporate tax changes would presumably also entail relatively modest adjustments by affected firms. Indeed, one way to interpret our estimates is that for the typical corporate tax increase (cut) of about one percentage point, about two jobs out of every thousand are lost (created). Our results indicate that firms are able to effectuate these relatively modest responses after one year of a tax change coming into effect.

5.2 Employment

Table 3 explores the effects of corporate taxes on employment further by reporting estimates of equation (1). Column 1 corroborates the results of the previous section, showing that changes in corporate income taxes have a negative and statistically significant effect on employment, controlling both for observed changes in county demographics and for unobserved variation in local economic conditions using border-county-group-by-year fixed effects ($\alpha_{g,t}$ in equation (1)).[23] Economically, the point estimate implies that a one percentage-point increase (cut) in the

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[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.

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 on the other side of the state border whose corporate tax rates remain unchanged.

Column 2 considers an alternative measure of the change in the corporate tax rate that is commonly used in the tax literature: the change in the natural log of one minus the tax rate. This specification provides us with an estimate of the elasticity of employment with respect to the net-of-tax rate. As shown, this leads to an estimate that is virtually identical in magnitude and statistical significance as that presented in column 1 (0.221, p<0.001).

How important is it to remove variation in local economic conditions? Column 3 reports a specification that omits the border-county-group-by-year fixed effects and instead includes year fixed effects only.[24] This increases the magnitude of the estimated tax effect noticeably, by almost 40%, 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 employment. This implies that the unobserved local variation goes in the same direction as the effect induced by the tax change (e.g., employment-reducing tax increases coincide with times when employment is already falling), which underscores the importance of controlling for local economic conditions using our border-county approach.

Columns 1-3 assume that the effect of corporate tax changes is symmetric. Column 4 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.

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[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 3.

We next 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 5 shows that corporate tax changes have no statistically significant effect on the number of self-employed. The tax-increase coefficient for the self-employed is -0.137 (p=0.223), and the tax cut coefficient is similarly statistically insignificant.

The placebo test 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.

Column 6 refines 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.[25] These data allow us to compare employment in bordering counties *within the same industry*, by including border-county-group-by-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.

This appears not to be the case, however. The estimate in column 6 shows that a one percentage point increase in the corporate tax rate reduces employment by 0.184% (p=0.024),

[25] 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.

measured relative to employment in the same industry in bordering counties whose tax rates remain unchanged. Tax cuts continue to have no significant effect on employment.

In Appendix Table 1, columns 1 to 3, we present additional robustness checks based on alternative employment measures. Specifically, we consider the change in employment without scaling by county population; employment scaled by the working age population (those aged 20-70); and private-sector employment only, excluding government employees. None of these alternative measures has any effect on our results.

5.3 Income

Table 4 presents estimates of the effect of corporate taxes 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.[26] Economically, the point estimate implies that 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 on the other side of the state border whose corporate taxes are unchanged. As in the case of employment, estimates of the elasticity of wage income with respect to the net-of-tax rate, shown is column 2, are virtually identical in magnitude and statistical significance as those presented in column 1 (0.338, p<0.001).

Column 3 replaces the border-county-group-by-year fixed effects with year fixed effects. Doing so significantly increases the magnitude of the estimated tax effect to -0.523% (p<0.001), which is about 40% greater (in absolute terms) than 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

[26] None of the demographic controls is significant at the 5% level.

(e.g., employment-reducing tax increases coincide with times when per capita wage income is already falling), again highlighting the importance of controlling for local economic conditions using the border-county approach.

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

In contrast, the effect of tax cuts on income is small (with a point estimate of 0.132 in column 4) and not statistically significant (p=0.455). The absence of an income effect indirectly rules out that tax cuts boost hours worked conditional on having a job (something that lack of data prevents us from estimating directly). If they did, we should find an increase in income following tax cuts. We thus conclude that in our setting, tax cuts appear to affect neither the extensive margin of employment (the number of jobs) nor the intensive margin (hours worked).

Column 5 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 taxes have a significant effect on the pre-tax income of the self-employed (who 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.

Columns 5 and 6 control for local industry-level shocks, just as we did in the case of employment. For income, the industry breakdown is available at both the SIC1/NAICS2 level and the SIC2/NAICS3 level. The results show that whether we control for industry-level shocks at the SIC1/NAICS2 level (column 5) or at the finer SIC2/NAICS3 level (column 6), we find that tax increases reduce income by approximately 0.3% (p=0.003 and p=0.019, respectively), while tax cuts have no effect.

In Appendix Table 1, columns 4 to 6, we present additional robustness checks based on alternative measures of wage income. Specifically, we consider private-sector wage income only, excluding income earned by government employees; wage income plus government transfers (such as unemployment insurance and welfare benefits), which may act as automatic stabilizers; and wage income by place of residence rather than by place of work, to account for the effect of commuters. None of these alternative measures changes our conclusions.

5.4 Impact Response Functions

We cap off this set of results by presenting impact response functions, analogous to those commonly shown in macroeconomics papers. Figure 4 plots the cumulative estimated response to a one percentage point tax increase or tax cut for the three years after it becomes effective, along with 95% confidence bands.[27] As the figure shows, tax increases effectively cause a level-shift in economic activity (just as implied by Figure 3), with both employment and income falling significantly one year after the tax increase becomes effective (t = 1), followed by no subsequent reversion over the next two years.[28] It thus appears that firms are able to react relatively quickly to the modestly sized tax increases observed in our setting. (For a discussion of the speed of the adjustment, see Section 5.1.) In contrast, we don't find that corporate tax cuts have significant effects on employment or income at any point.

[27] Consistent with the validity of the parallel-trends assumption shown earlier, none of the estimated pre-tax-change effects is statistically significant.

[28] Specifically, in response to a one percentage point tax increase employment falls by 0.239% one year after the tax increase becomes effective (p=0.002) and by an insignificant 0.074% and 0.088% incrementally over the next two years. Income falls by 0.366% one year after the tax increase becomes effective (p<0.001) and then rises by an insignificant 0.001% and 0.022% incrementally over the next two years.

5.5 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. Such spillovers, if they are present, could result from changes in the behavior of either firms or households.

5.5.1 Potential Spillover Effects of Corporate Tax Increases

Consider the case of a corporate tax increase. Our findings suggest that firms in treated counties respond to a corporate tax increase by reducing their demand for labor, which in turn lowers county income. However, there are potentially two ways in which a control county might be indirectly affected due to spillovers emanating from the treated county.

First, since households in the treated county have lower incomes, they may spend less, not just in their home county but in the wider local economy – including the 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. (The diff-in-diff estimate would thus be smaller than it would be without the spillover.)

Whether this channel is in operation can be tested. If reduced household spending is leading to negative spillovers across the state border, then this effect would be stronger in control counties closer to the border compared to other counties in the control state that are located further from the border. We will therefore test whether there are differences in employment and income growth in control-border counties relative to hinterland counties in the interior of the control state (again defined as those counties one-removed from the border).[29]

[29] We borrow the term "hinterland" county from Huang (2008). Also, recall that income and employment are measured by place of work, which allows 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. Our results are also robust to measuring income by place of residence, see Appendix Table 1, column 6.

A second type of cross-border spillover may be due to firms reacting to a corporate tax increase by shifting operations from the treated county to the control county in order 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.

If distance matters in firms' decisions as to where to relocate operations following a corporate tax increase, the control state's border counties 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 (with the two types of spillover implying opposite effects).[30] In the case of corporate spillovers, an additional test may be useful. Corporate relocations across the border are arguably more likely if, after the tax increase, the treated state has a higher tax rate than the neighboring control state. Empirically, it would be surprising if firms reacted to a corporate tax increase in one state by relocating to an even higher taxing state.

5.5.2 Potential Spillover Effects of Corporate Tax Cuts

A possible reason for our finding that corporate tax cuts have little effect on average 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

[30] To be clear, firms might react to a corporate tax increase by shifting operations to potentially anywhere in the country (see Giroud and Rauh (2017)). Our concern, however, is to test only for whether border counties in control states are *disproportionately* affected by this type of relocation – and this is what our tests rule out.

income to corporate tax cuts.

5.5.3 Testing for Spillover Effects

In Table 5, Panel A, we test for both "household" and "corporate" spillovers by comparing outcomes in border versus hinterland counties, both in the same control states, around the time of tax increases and tax cuts in neighboring treated states. Specifically, we regress the change in employment or income growth on an indicator for whether a county is a hinterland county. Critically, we always compare border counties in control states to their neighboring (i.e., adjacent) hinterland counties by including border/hinterland-group-by-year fixed effects. These effectively control for all shocks to local economic conditions common to border and hinterland counties and thus allow us to isolate the differential effect of being a hinterland county.[31] 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 growth (columns 1 and 2) or income growth (columns 3 and 4) between hinterland and border counties in control states around the time of tax increases or tax cuts in neighboring treated states. We believe that these findings provide strong evidence that our estimates are not systematically biased due to either household or corporate spillovers.

Table 5, Panel B, returns to our baseline sample and allows 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 if firms respond to corporate tax increases by relocating activity across the border, all else equal, relocation is more

[31] Our conclusions don't change if we compare all border counties in a control state to all hinterland counties, or to all counties in the interior of the control state (thus including state-by-year fixed effects in the regressions instead of border/hinterland-group-by-year fixed effects). In all these tests some smaller states (e.g., Delaware and Rhode Island) and DC have no interior and so are omitted.

likely to take place if the neighboring control state has a lower tax rate. This would imply greater amplification of the effect of tax increases 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 county 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 treated state that has cut its corporate tax rate below that of their home state. Overall, it therefore seems unlikely that our estimated treatment effects are inflated due to firms in treated counties moving operations directly across the state border in response to tax changes.

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 serious concern in our setting. In other words, it appears that control border counties on average remain on their pre-treatment trend even after a tax change in a neighboring state. They hence provide a plausible set of counterfactuals against which to measure the economic effects of corporate tax changes in adjacent treated counties.

5.6 Robustness and Alternative Channels

We next present several important robustness checks. Geography, in particular, gives rise to a number of potential concerns with our empirical strategy. The first 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 prior to a tax change. A third concern is more specific: one could be concerned that the decline in manufacturing in the rust belt might be driving our results.[32] A fourth concern is that our results, being equal-weighted, might be driven by the experience of small (and so potentially unrepresentative) counties.

Table 6 shows that our results are robust to these geographic concerns. Columns 1 and 2 remove potentially politically influential counties by restricting the sample to border counties that account for no more than 10% of a state's population, as measured in the year before a tax change. Columns 3 and 4 restrict the sample to border-county pairs whose geographic mid-points are no more than 20 miles apart.[33] Columns 5 and 6 exclude counties in the industrial Midwest (OH, PA, IN, MI, IL, and WV) in the 1970s and 1980s. Columns 7 and 8 present weighted least squares regressions (weighting by employment measured prior to the relevant tax change).[34] In each case, the point estimates are nearly identical to our baseline estimates.[35]

Table 7 presents further robustness checks related to various aspects of state tax policy. A natural interpretation of the results in Tables 3 and 4 is that firms reduce their labor demand as some activities cease to be economically viable at the higher tax rate. But there is also another possibility: firms might be responding to a contemporaneous reduction in credit. This may occur because banks too pay state corporate taxes, albeit often on a special schedule that only applies to banks. An increase in state taxes on banks could thus reduce the supply of credit available in the state,[36] and firms might then be responding to a credit supply shock rather than a corporate tax increase occurring at the same time.

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[32] Some large 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.

[33] We obtain latitudes and longitudes of county midpoints from the Census Bureau's Gazetteer files and use the great-circle distance formula to calculate the shortest distance between the two midpoints. The results are similarly robust to dropping counties covering large geographic areas, common in the western part of the U.S.

[34] Although the tax increase coefficients are somewhat smaller in the weighted least squares regressions, using the test developed in DuMouchel and Duncan (1983), we cannot reject the null hypothesis that their size is the same as in the our baseline OLS estimates.

[35] Excluding other potentially influential observations similarly leaves our results unaffected. For example, restricting the sample to tax increases and tax cuts of commensurate size and dropping tax increases and tax cuts larger than two, or larger than five, percentage points leads to qualitatively similar conclusions.

[36] For evidence on this channel, see Smolyansky (2018).

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, with economic magnitudes and statistical significance comparable to the overall sample.

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 R&D or for hiring more labor.[37] This has no effect on our results – the point estimates are virtually identical to those in Tables 3 and 4, suggesting that our findings are not driven by coincident changes in state tax incentive programs.

So far, our analysis has focused on changes in state corporate tax rates. However, the tax burden faced by firms will also be affected by the state tax base (see Suarez Serrato and Zidar (2018)). To investigate if variation in the state tax base affects our results, columns 5 and 6 include additional state tax base control variables in our baseline regressions, following the approach in Suarez Serrato and Zidar (2016).[38] This leaves our results unchanged.

Columns 7 and 8 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 concern 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 our broad conclusions are unchanged. If anything, our results become stronger:

[37] For details on these state tax credits, see Chirinko and Wilson (2008), Wilson (2009), and Chirinko and Wilson (2017).
[38] In particular, we include the same tax base controls as listed in the Online Appendix of Suarez Serrato and Zidar (2016), Section E.4.1. These include controls for throwback rules, combined reporting, tax credits, loss carry-forward and carry-back rules, federal income tax deductibility and accelerated depreciation, among others.

in this subsample, the estimated effect of a corporate tax increase on employment is –0.516% (p=0.021) and on income –0.605% (p=0.008), while tax cuts continue to have no statistically significant effects.

5.7 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, which are measured 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 (in the region or the state, respectively) has fallen for at least two quarters in the year prior to the tax change becoming effective.

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 wage 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 wage income increases by around 1%. During expansions, on the other hand, we continue to find that tax cuts have no significant effects.

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

5.8 Heterogeneous Effects

To shed light on the channel and provide further robustness, we next test for heterogeneous effects. As noted earlier, self-employment income tends to be taxed as personal income rather than corporate income, and indeed our prior results validated that neither the employment nor the income of the self-employed is affected by corporate tax changes (see column 5 of Tables 3 and 4). A corollary of this finding is that counties where self-employment makes up a larger share of economic activity should be less affected by corporate tax changes. In columns 1 and 2 of Table 9, we test this hypothesis by interacting the tax increase and tax cut variables with indicators for whether the county's share of income earned by proprietors, relative to total county income, is above or below the median among all counties (as measured in the year prior to the relevant tax change). The results confirm our hypothesis. Indeed, tax increases only have significantly negative effects in counties in which proprietors earn a low share of total county income, and the estimated effects in these counties are more negative than in our baseline specification. These findings give us further confidence that the effects we are picking up are due specifically to corporate tax changes directly affecting economic activity, as opposed to other confounding factors.

In columns 3 and 4 of Table 9, we test for whether corporate tax changes have differential effects based on the pre-existing *level* of a state's corporate taxes. We believe this test is interesting because it is an open question whether a tax increase of one percentage point would have the same effect in a state whose prior corporate tax rate was, say, 2%, compared to a state where it was 10%. Our framework allows us to easily test for this type of nonlinearity. To do so, we interact the tax increase and tax cut variables with indicators for whether the county's state corporate tax rate was above or below the national median among all counties (as measured in the year prior to the relevant tax change).

The results show that corporate tax increases only have significantly negative effects in states that start off with low corporate tax rates. Moreover, in these states, the effects are more negative than in our baseline specification (and also more negative than in states with high corporate tax rates). These results make sense for the following reason: prior to a given tax change taking place, firms will endogenously choose where to locate; and overall, one would expect that economic activity that is more tax sensitive to endogenously locate in states with lower corporate tax rates. Thus, when these low-taxing states do increase their corporate tax rates, the negative effects are especially pronounced. This finding arguably has the interesting policy implication that low taxing states may find it particularly costly to raise taxes, because the type of economic activity that has selected into the low-tax environment is more tax sensitive. To our knowledge, we are the first to document this type of nonlinearity.

We have also investigated several other potential heterogeneous effects, based on the flexibility of the state's labor markets, and on several county characteristics. If tax increases harm employment and income by reducing firms' demand for labor, the effects might differ based on the strength of unionization in a state. In Appendix Table 2, we investigate this possibility by testing for heterogeneous effects based on whether the state has a right-to-work law in place and whether the workforce is heavily unionized (i.e., whether their private-sector unionization rate is above or below the national median).[39] In either case, however, we do not find evidence of heterogeneous effects. Similarly, in Appendix Table 3, we investigate heterogeneous effects based on county income per capita (i.e., whether the county is "rich" or "poor"), county population density, and the share of county employment attributable to small establishments. In each case, we fail to find evidence of heterogeneous effects. We conclude from these tests that our estimates are remarkably stable, and that our results are not driven by either the degree of state unionization or by key differences in county characteristics.

6. Conclusions

Against the background of continuing controversy over the benefits of corporate tax reform, we ask how corporate taxes affect employment and wage income. Answering this question is

[39] 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)).

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 reducing omitted-variable biases resulting from the confounding influence of unobserved variation in local economic conditions. Effectively, our empirical approach exploits the fact that state tax policy ends at the state border, while economic conditions do not.

Our estimates suggest that a one percentage point corporate tax increase (cut) leads to employment in the affected county falling (rising) by about 0.2 percent and total wage income falling (rising) by about 0.3 percent (as measured relative to the neighboring county across the border). We find evidence of asymmetric effects: tax increases are uniformly harmful, while tax cuts only appear to be effectual in boosting economic activity if implemented during recessions.

The strength of our research design is that it allows us to run a "clean experiment." However, we caution against naïvely extrapolating our estimates to assess the possible impact of a federal corporate tax change. Tax changes at the federal level will give rise to general equilibrium effects that we cannot account for in our local comparison of bordering counties. Moreover, federal corporate tax changes—particularly cuts—have in recent history been of a magnitude that lies well outside those that we observe in our sample. Nonetheless, we believe that our results remain qualitatively useful in the context of the broader debate about corporate tax reform. In essence, our key finding is that companies appear to be highly responsive to changes in corporate taxes. Our methodology allows us to more confidently attribute these responses specifically to changes in corporate tax rates rather than other confounding factors. Since we are able to identify these strong responses quite cleanly, our analysis suggests that companies are indeed highly sensitive to corporate tax incentives.

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Appendix A. Treatments and Controls.

We estimate equations (1) in a sample that includes only years in which one of the counties in a contiguous border-county group experiences a tax change. And analogously, we estimate equation (2) in a sample that includes only the six year window around one of the counties in a contiguous border-county group experiencing a tax change. Other 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. This approach allows for the possibility that a treated county can itself become a control county if sufficient time has elapsed since it last experienced a tax change.[40]

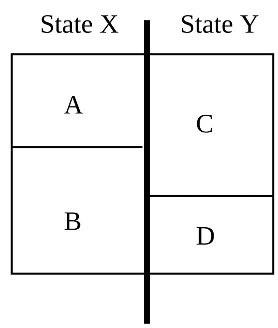
There are 3,132 counties in the U.S.[41] Of these, 943 are "treated" in the sense that they lie on the border of a state that changes corporate taxes between 1970 and 2010. Since a state can change corporate taxes multiple times, we have a total of 3,390 treated county-years. These are matched to 3,650 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.)

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

[40] Throughout that analysis, we impose the conditions that 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.
[41] 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.

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.[42]

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



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

[42] 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.
[43] This approach is standard in studies implementing a border discontinuity research design. See for example Dube, Lester, and Reich (2010).

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

All dependent variables are from the Personal Income and Employment Summary of the Bureau of Economic Analysis's (BEA) Regional Economic Accounts, unless stated otherwise.

Employment is 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. Employment in a county is measured by the location of the place of work, not by the worker's place of residence.

Employment (excluding government) is the total number of wage and salary jobs (line code 7020) 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).

Self-employment is 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).

Wage income by place of work is measured as the earnings by place of work (line code 35) minus proprietors' income (line code 70), scaled by total county population (line code 20).

Wage income by place of work (excluding government) is measured as the earnings by place of work (line code 35 in the BEA's) minus proprietors' income (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 (line code 20).

Wage income by place of work (including transfers) is measured as the 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). Transfers receipts include food stamps, unemployment insurance, and training assistance.

Wage income by place of residence is measured as the 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).

Self-employment income by place of work is measured as the nonfarm proprietors' income (line code 72), scaled by total county population (line code 20).

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.

Figure 1. Average State Corporate Income Tax Rate by Year.

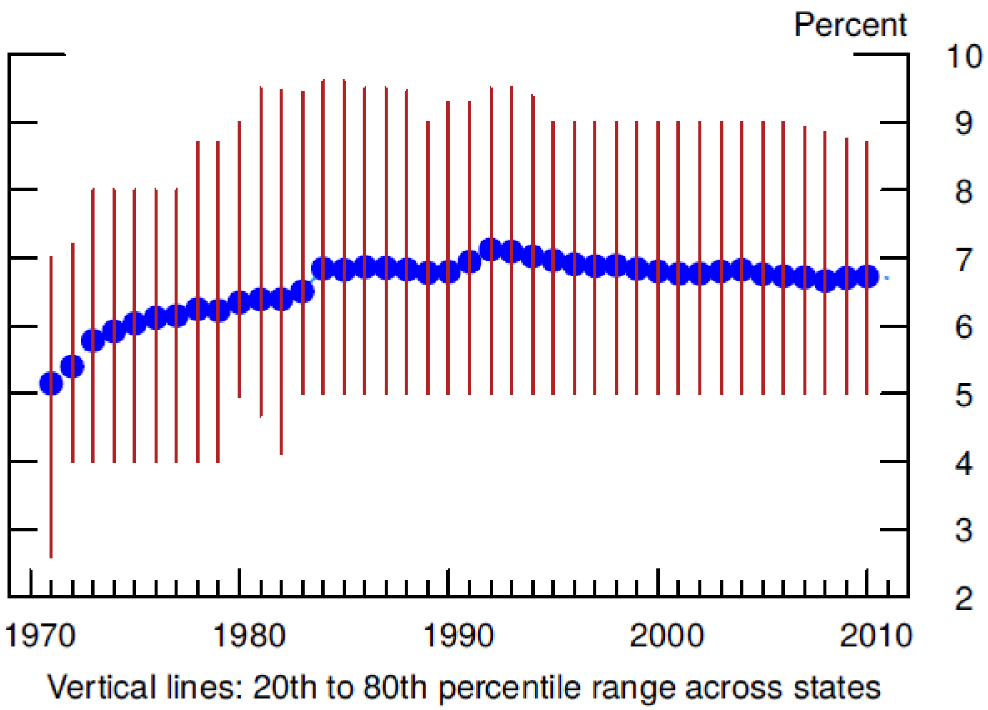


Figure 2. Number of State Corporate Tax Changes per Year.

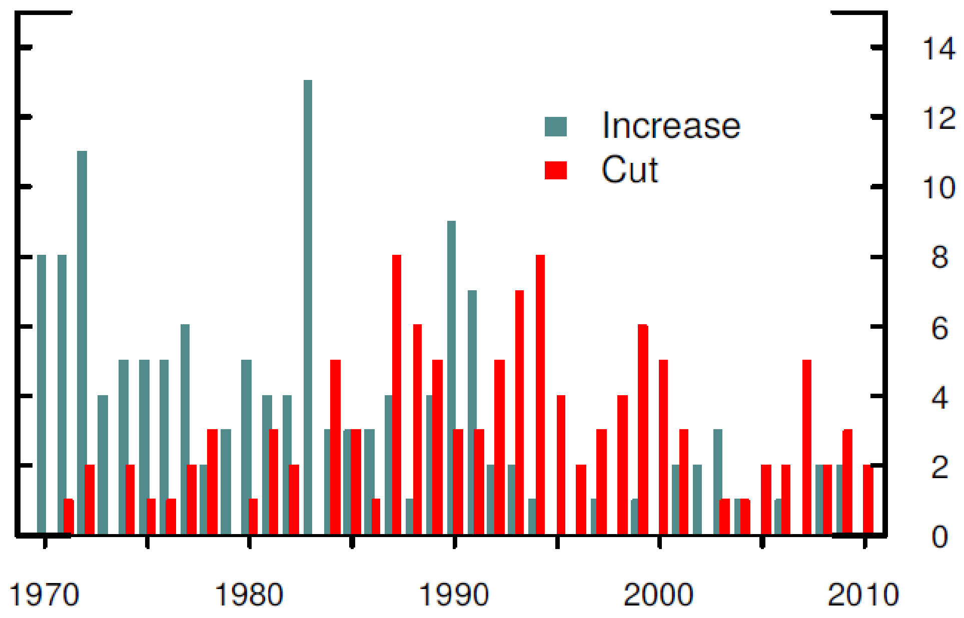


Figure 3. Dynamic Effects of Corporate Income Tax Changes.
This figure plots the dynamic effects of state corporate income tax changes on employment and wage income based on the estimation of equation (2), along with 95% confidence bands. The figure shows the estimated year-on-year percentage changes in county employment and wage income in response to a one percentage point increase (cut) in the state corporate income tax rate, as measured relative to border counties in neighboring states where taxes have not changed.

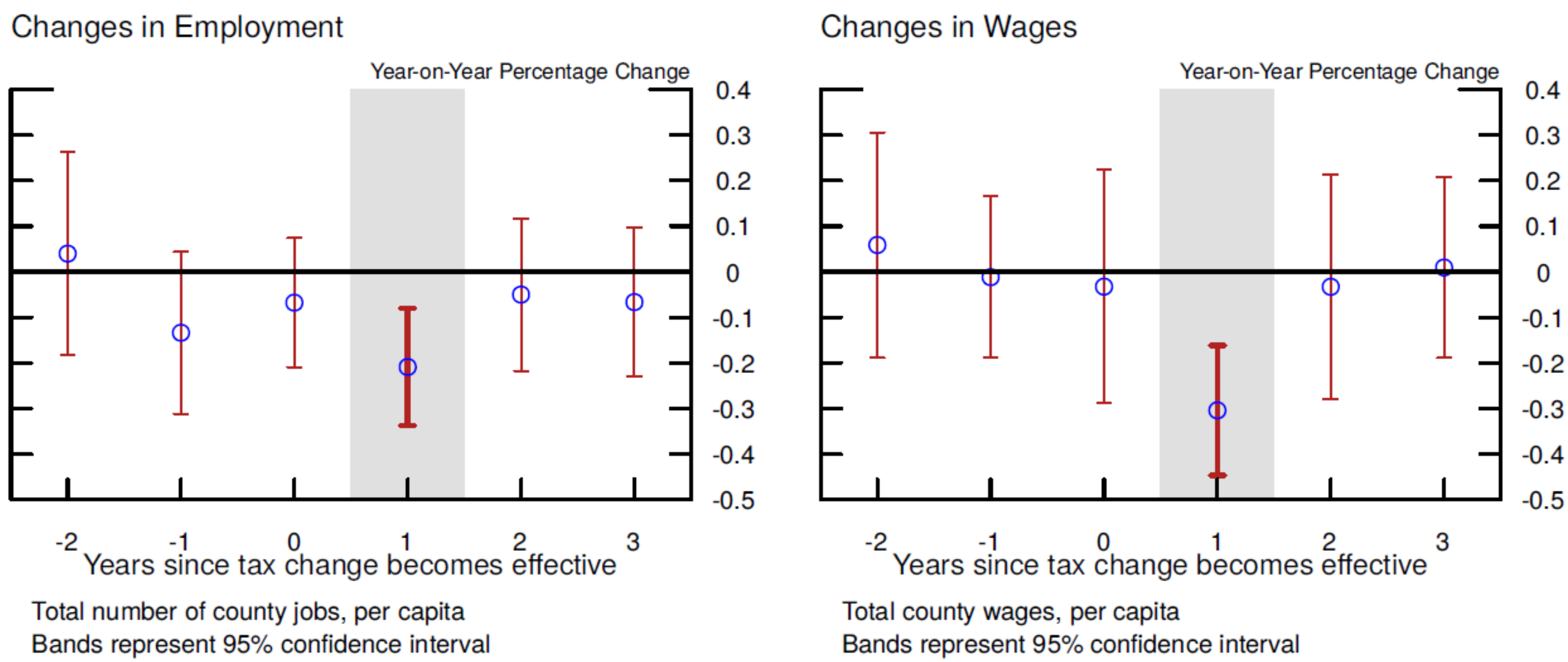


Figure 4. Impact Response Functions.
This figure plots the impact responses of employment and wage income to changes in state corporate tax rates, along with 95% confidence bands. The figure shows the cumulative effect of a one percentage point tax increase or tax cut on employment and wage income, starting one year after the tax change becomes effective (t = 1). The y-axis measures the cumulative percent change in our baseline employment and wage income measures.

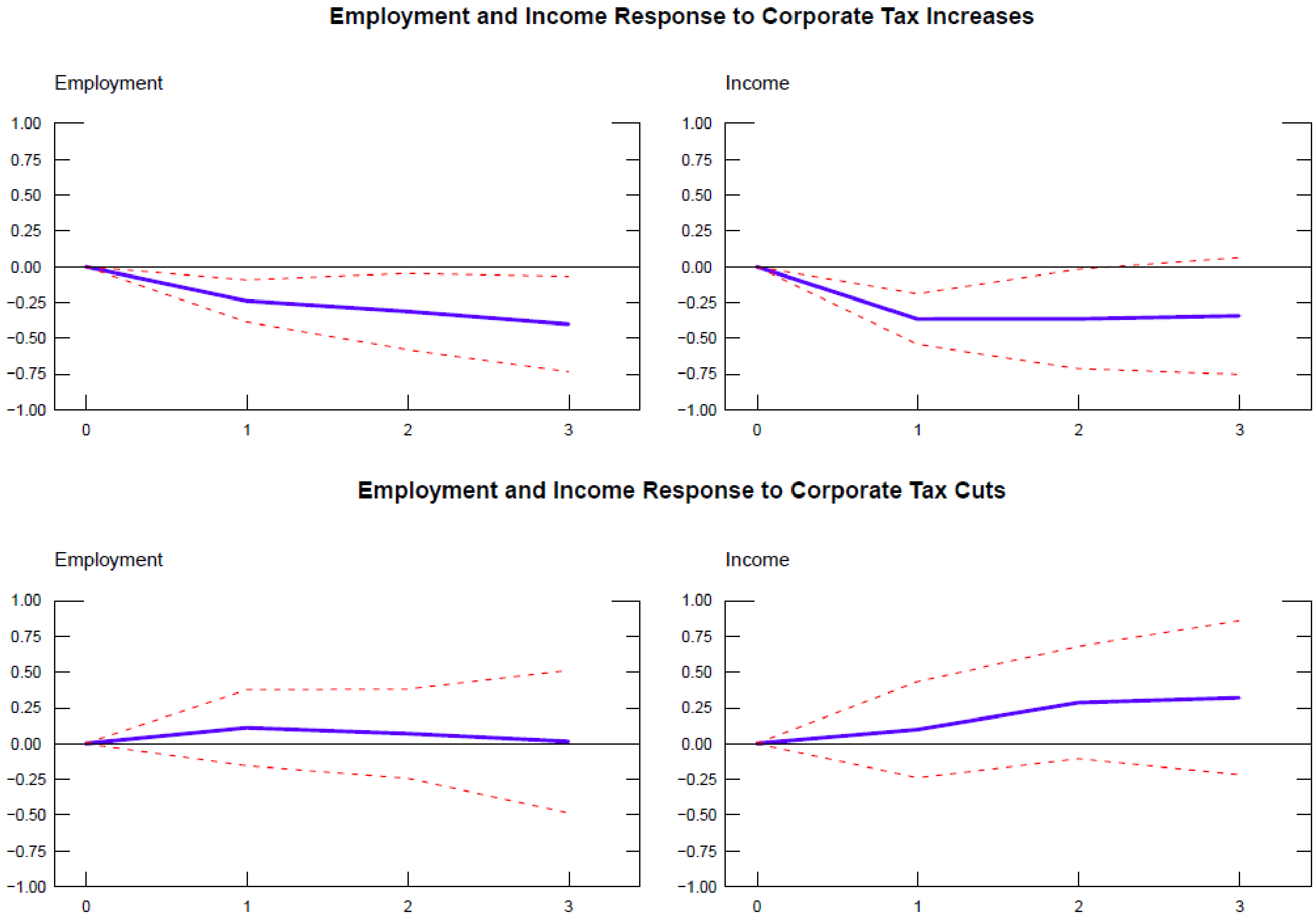


Table 1. Corporate Tax Change Summary Statistics and Coincident State-level Policy Changes.

Panel A reports summary statistics for the 250 changes in state corporate income tax rates that occurred over the period from 1970 to 2010. The data sources are detailed in the text. 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 other state-level policy 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.

		Tax increases	Tax cuts
Panel A. Summary statistics			
Number of tax changes		135	115
Mean tax change		1.27%	-0.68%
Standard deviation		1.15%	0.74%
Minimum tax change		0.16%	-5.45%
Median tax change		1.00%	-0.50%
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	1	5
	investment tax credits	1	6
	job creation tax credits	0	0
	changes in state tax on banks		
	increase in tax on banks	97	1
	cut in state tax on banks	3	79
	changes in apportionment rules	4	3

Table 2. 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 are one-removed from the border), measured the year before a tax change. Panel B reports changes in the 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%, 5%, and 10% 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**
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***
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
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**

Table 3. Effect of Corporate Tax Changes on Employment.

We estimate the effect of corporate income taxes on employment in a differences-in-differences specification that regresses log changes in county employment on changes in the state's top marginal corporate income tax rate. Our baseline employment measure in columns 1 to 4 is the total number of wage and salary jobs (full-time and part-time) in a county scaled by total county population. Column 5 estimates the effect on the number of nonfarm self-employed workers. Treated counties are those that are located along the border of a state that changes its corporate income tax rate. Control counties are those that lie on the other side of the border in a neighboring state that has not changed its corporate income tax rate. Except in columns 3 and 6, each regression includes a set of group-by-year fixed effects common to groups of continuous (i.e., adjacent) counties on both sides of the border, thus removing the influence of time-varying shocks to local economic conditions. In column 6, the unit of analysis is a county-industry-year, with the industry breakdown at the SIC1/NAICS2 level. Column 6 includes a set of group*industry-year fixed effects to control for time-varying shocks that affect the same industry in groups of contiguous border counties. All specifications include changes in county-level demographic controls as detailed in the text (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.

	continuous tax change variable (1)	elasticity (2)	no border fixed effects (3)	asymmetric tax change effects (4)	self-employment only (5)	industry: SIC1/ NAICS2 (6)
Δ tax rate	-0.241***		-0.336***			
	0.065		*0.060*			
Δ ln(1 – tax rate)		0.221***				
		0.061				
magnitude of tax increase				-0.282***	-0.137	-0.184**
				0.075	*0.111*	*0.079*
magnitude of tax cut				0.065	0.150	-0.022
				0.128	*0.252*	*0.201*
Demographic controls	x	x	x	x	x	x
Year fixed effects			x			
Group-by-year fixed effects	x	x		x	x	
Group*industry-year FE: SIC1/NAICS2						x
Adjusted R^2	9.1%	9.1%	9.6%	9.1%	21.7%	13.2%
Number of county-years	10,366	10,366	7,040	10,366	10,366	81,723

Table 4. Effect of Corporate Tax Changes on Income.

We estimate the effect of corporate income taxes on wage and self-employment income in a differences-in-differences specification that regresses log changes in county income on changes in the state's top marginal corporate income tax rate. Our baseline income measure in columns 1 to 4 is total wage and salary income (by place of work) in a county scaled by total county population. Column 5 estimates the effect on income from nonfarm self-employment. All income variables are pre-tax. Treated counties are those that are located along the border of a state that changes its corporate income tax rate. Control counties are those that lie on the other side of the border in a neighboring state that has not changed its corporate income tax rate. Except in columns 3, 6 and 7, each regression includes a set of group-by-year fixed effects common to groups of continuous (i.e., adjacent) counties on both sides of the border, thus removing the influence of time-varying shocks to local economic conditions. In column 6 and 7, the unit of analysis is a county-industry-year, with the industry breakdown at the SIC1/NAICS2 level in column 6, and the SIC2/NAICS3 level in column 7. Column 6 and 7 include a set of group*industry-year fixed effects to control for time-varying shocks that affect the same industry in groups of contiguous border counties. All specifications include changes in county-level demographic controls as detailed in the text (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.

	continuous tax change variable (1)	elasticity (2)	no border fixed effects (3)	asymmetric tax change effects (4)	self-employment only (5)	industry: SIC1/ NAICS2 (6)	industry: SIC2/ NAICS3 (7)
Δ tax rate	-0.367***		-0.523***				
	0.072		*0.092*				
Δ ln(1 – tax rate)		0.338***					
		0.069					
magnitude of tax increase				-0.420***	-0.165	-0.311***	-0.302**
				0.087	*0.189*	*0.100*	*0.125*
magnitude of tax cut				0.132	0.518	-0.054	-0.121
				0.175	*0.444*	*0.287*	*0.182*
Demographic controls	x	x	x	x	x	x	x
Year fixed effects			x				
Group-by-year fixed		x					
effects	x			x	x		
Group*industry–year FE:							
SIC1/NAICS2						x	
SIC2/NAICS3							x
Adjusted R^2	20.9%	20.9%	17.5%	20.9%	37.3%	19.7%	26.0%
Number of county-years	10,366	10,366	7,040	10,366	10,366	82,769	267,691

Table 5. Testing for Cross-Border Spillovers.

This table tests for cross-border spillovers emanating from changes in either household or firm behavior. For example, following a corporate tax increase, firms may move operations from the treated county to the control county, creating jobs there. Such a substitution of workers from treated to control counties would amplify the estimated treatment effect. Alternatively, in response to a corporate tax increase, laid off workers in the treated county might spend less across the border – the potential reduction in demand in the control county might lead to layoffs there, which would attenuate our estimated treatment effects. Panel A tests for spillovers by examining the effects in control states, and comparing control border counties to hinterland counties (i.e., counties located in a control state's interior that are one-removed from the border). For example, household spillovers in response to tax rises (tax cuts) imply systematic reductions (increases) in economic activity in control border counties relative to hinterland counties, which being further away from the border should be less affected by the spillover. Panel B returns to the baseline estimation sample of Tables 3 and 4 to further test for corporate spillovers. Columns 1 and 2 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. Arguably, if corporate spillovers exist, firms are more likely to shift operations in response to a tax increase if the neighboring control county has a lower corporate tax rate (and vice versa for a tax cut). 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.

PANEL A: Border versus hinterland counties in control states

	Change in log employment		Change in log income	
	around tax increase in neigh-boring state	around tax cut in neigh-boring state	around tax increase in neigh-boring state	around tax cut in neigh-boring state
	(1)	(2)	(3)	(4)
=1 if hinterland county	0.085	-0.089	0.174	-0.043
	0.241	*0.306*	*0.392*	*0.402*
Demographic controls	x	x	x	x
Border/hinterland-group-by-year FE	x	x	x	x
Adjusted R^2	26.2%	23.9%	30.5%	26.9%
Number of county-years	4,026	3,636	4,026	3,636

PANEL B: Testing for corporate spillovers

	higher/lower tax rate in control states	
Dep. var.: Change in log	employment (1)	income (2)
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*
x high economic integration		
x low economic integration		
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*
x high economic integration		
x low economic integration		
Demographic controls	x	x
Group–year fixed effects	x	x
Wald test (*F*):		
Tax increase: high = low	0.64	0.43
Tax cut: high = low	0.25	0.06
Adjusted R^2	9.1%	20.9%
Number of county-years	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 county pairs by restricting the estimation sample to border county pairs whose geographic midpoints are no more than 20 miles apart. Columns 5 and 6 remove "rust belt" counties in the industrial Midwest (in OH, PA, IN, MI, IL, and WV) which in the 1970s and 1980s suffered declines in industrial activity. Columns 7 and 8 report estimates weighted by county employment measured the year before a tax change. Each specification includes a set of group-by-year fixed effects (as in Tables 3 and 4) to remove the influence of time-varying shocks to local economic conditions. 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.

	"Non-influential" counties only		"Close" counties only		Non "rust belt" counties only		Weighted by employment	
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)
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, the Tax Base, and Tax Differentials.

To further investigate robustness, columns 1 and 2 drop corporate tax changes that coincide with bank tax changes. 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 add state tax base controls, following the approach in Suarez-Serrato and Zidar (2016). Columns 7 and 8 restrict the sample to border county pairs whose tax rates differ by at most 20 basis points two years prior to treatment. Each specification includes a set of group-by-year fixed effects (as in Tables 3 and 4) to remove the influence of time-varying shocks to local economic conditions. 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		Including state tax base controls		Tax rates at most 20bp apart	
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)
magnitude of tax increase	-0.332***	-0.274***	-0.282***	-0.420***	-0.301***	-0.425***	-0.516**	-0.605***
	0.106	*0.094*	*0.075*	*0.087*	*0.087*	*0.090*	*0.212*	*0.214*
magnitude of tax cut	-0.038	0.282	0.107	0.152	0.079	0.163	-0.201	-0.385
	0.276	*0.282*	*0.153*	*0.183*	*0.154*	*0.202*	*0.549*	*0.772*
Demographic controls	x	x	x	x	x	x	x	x
Group–year fixed effects	x	x	x	x	x	x	x	x
Tax base controls					x	x		
Adjusted R^2	11.9%	22.1%	9.2%	21.1%	21.1%	19.4%	20.2%	19.4%
Number of observations	2,906	2,906	10,099	10,099	10,214	10,214	860	860

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 (with regions defined by the BEA), and columns 5 and 6 split the sample according to whether a tax change occurs during a state recession. Regional and state recessions are coded based on whether real personal income (in the region or the state, respectively) has fallen for at least two quarters in the year prior to the tax change becoming effective. Each specification includes a set of group-by-year fixed effects (as in Tables 3 and 4) to remove the influence of time-varying shocks to local economic conditions. 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.

	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. Proprietors' Share, and Asymmetric Tax Rate Effects.

We examine whether state corporate income tax changes have heterogeneous effects depending on: the share of county income that is not likely to be directly affected by corporate taxation (columns 1 and 2); and the level of state corporate taxes prior to a tax change (columns 3 and 4). Since proprietors' income tends to be taxed as personal rather than corporate income, columns 1 and 2 split the sample according to whether a county's share of nonfarm proprietors' income relative to total income is above or below the national median among all counties one year earlier. To investigate potential asymmetric effects based on the preexisting level of state corporate taxes, columns 3 and 4 split the sample according to whether a county's state corporate tax rate one year prior to a tax change is above or below the national median among all counties. Each specification includes a set of group-by-year fixed effects (as in Tables 3 and 4) to remove the influence of time-varying shocks to local economic conditions. 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.

	Conditioned on proprietors' share		Conditioned on high/low tax rate	
Dep. var.: Change in log	employment (1)	income (2)	employment (3)	income (4)
Magnitude of tax increase				
x high proprietors' share	-0.153	-0.201		
	0.112	*0.145*		
x low proprietors' share	-0.368***	-0.566***		
	0.101	*0.130*		
x high tax rate			-0.026	-0.189*
			0.090	*0.111*
x low tax rate			-0.397***	-0.524***
			0.092	*0.089*
Magnitude of tax cut				
x high proprietors' share	-0.011	0.084		
	0.187	*0.219*		
x low proprietors' share	0.137	0.179		
	0.158	*0.208*		
x high tax rate			0.026	0.112
			0.128	*0.179*
x low tax rate			0.356	0.281
			0.900	*0.776*
Demographic controls	x	x	x	x
Group–year fixed effects	x	x	x	x
Wald test (F):				
Tax increase: high = low	2.18	3.16*	9.40***	6.90**
Tax cut: high = low	0.39	0.15	0.12	0.04
Adjusted R^2	9.1%	20.9%	9.1%	20.9%
Number of county-years	10,366	10,366	10,366	10,366

INTERNET APPENDIX

(NOT INTENDED FOR PUBLICATION)

Appendix Table 1. Robustness: Alternative Measures of Employment and Income.

This table presents regression specifications that are the same at Tables 3 and 4 using alternative measures of employment (columns 1 to 3) and wage income (columns 4 and 6). Column 1 models the raw number of jobs, without scaling by county population. Column 2 scales jobs by the number of county residents aged between 20 and 70. Column 3 excludes government employment from our baseline employment measure, 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. Each specification includes a set of group-by-year fixed effects (as in Tables 3 and 4) to remove the influence of time-varying shocks to local economic conditions. 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.

Dep. var.:	Change in log employment			Change in log income		
	unscaled (1)	scaled by population aged 20-70 (2)	excluding govt. (3)	excluding govt. (4)	including transfers (5)	by place of residence (6)
magnitude of tax increase	-0.289***	-0.288***	-0.289***	-0.422***	-0.307***	-0.247***
	0.070	*0.075*	*0.086*	*0.098*	*0.091*	*0.070*
magnitude of tax cut	0.105	0.100	0.008	-0.014	0.088	0.146
	0.129	*0.126*	*0.146*	*0.216*	*0.135*	*0.141*
Demographic controls	x	x	x	x	x	x
Group–year fixed effects	x	x	x	x	x	x
Adjusted R^2	15.5%	8.8%	10.0%	18.2%	19.1%	41.1%
Number of county-years	10,366	10,334	10,366	10,366	10,366	10,366

Appendix Table 2. Labor Market Flexibility.

We examine whether state corporate income tax changes have heterogeneous effects on employment and wage 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 (as detailed in Ellwood and Fine (1987) and updated by Matsa (2010)). 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 (the data come from Hirsch and Macpherson (2003) as updated on their website, www.unionstats.com). Each specification includes a set of group-by-year fixed effects (as in Tables 3 and 4) to remove the influence of time-varying shocks to local economic conditions. 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.

	Conditioned on right-to-work state		Conditioned on unionization rate	
Dep. var.: Change in log	employ-ment (1)	income (2)	employ-ment (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

Appendix Table 3. Heterogeneous Effects by County Characteristics.

We examine whether state corporate income tax changes have heterogeneous effects on employment and wage 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 establishments with fewer than ten employees is above or below the national median among all counties one year earlier (data are from County Business Patterns, available only from 1974; the measure is constructed following Adelino, Schoar, and Severino (2015)). Each specification includes a set of group-by-year fixed effects (as in Tables 3 and 4). 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	employ-ment (1)	income (2)	employ-ment (3)	income (4)	employ-ment (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