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# **The corporate tax, apportionment rules and employment: Evidence using policy discontinuity at U.S. state borders**

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## **Abstract**

A recent set of empirical works highlights a puzzling asymmetric response of labor market outcomes to the corporate tax. This paper explores a potential source of this disparity, using differentials in profit accounting rules across U.S. states. I exploit policy discontinuities at state borders by pairing counties in states featuring a tax change with their contiguous counterparts in control states. I notice that corporate tax cuts do not boost employment while tax hikes reduce job creation. The incidence of tax increases on employment seems limited in states with a single sales factor apportionment formula and pronounced in states that use a triple factor apportionment rule. I present a basic conceptual framework that explains this pattern.

**JEL Classifications:** H22, H25, H32, H71

**Keywords:** Tax incidence, Profit-shifting, Corporate tax, Profit apportionment, Employment.

## 1. Introduction

The theory of public finance has contributed a substantial amount of literature to the analysis of the corporate tax incidence (Harberger 1962, Grubert and Mutti 1985, Gravelle and Smetters 2006, Randolph 2006, Gravelle 2008). Recently, a series of empirical works questioned the implicit implications of a symmetric response of labor market outcomes to the corporate tax (Ljungqvist and Smolyansky 2014, Fuest et.al 2017). This behavioral oddity which I refer to as the “corporate tax asymmetry puzzle” is one of the main objectives of this analysis. I relate the incidence of the corporate tax to state apportionment rules, with the goal of understanding the sources of the above-mentioned puzzle. First, I notice that corporate tax cuts do not boost employment while tax hikes reduce job creation. The incidence of tax increases on employment seems negligible in states with a single sales factor apportionment formula and pronounced in states that use a multiple factor apportionment rule. I present a basic theoretical model that partially explains these patterns. To identify these effects, I adopt a spatial discontinuity approach that compares counties straddling a state borderline around tax policy reforms.

Specifically, a one percentage point increase in a state’s effective corporate tax rate significantly reduces the growth of employment by 0.21 percentage point. However, state corporate tax cuts do not bear any significant impact on job creation. The magnitude of the incidence of a business tax hike on employment is not quite meaningful in states using a single sales factor apportionment but rises significantly to -0.29 percentage point in states using a multiple factor apportionment formula. I expose a basic theoretical framework to explain the disparity of the incidence of a corporate tax increase across states with different accounting rules.

The empirical literature could be characterized by two different lines of argument. The critical methodological challenge remains the validity of the parallel trend assumption across the studied units. Contributions of the likes of Djankov et al. (2006) or Carroll (2009), build on the neoclassical tradition and focus on the of the impact of the tax operating through factor reallocations between corporate and non-corporate sectors (indirect effects). In contrast, a rather heterodox approach emerged recently and measure the so-called “direct effect” of the tax. The proponents of this school emphasize the importance of labor market institutions and market imperfections in the analysis of the incidence. Most notable are market power (Liu and Altshuler 2013) and imperfect labor market (Felix et al. 2009 and Devereux et al. 2010). Both schools implicitly treat the incidence as symmetric meaning that the magnitude of the effect of a corporate rate cut is similar to that of a rate increase in absolute value. Recently, this presumption came under scrutiny with a couple of empirical works that documented (Ljungqvist and Smolyansky 2014, Fuest et.al 2017) a potential asymmetric response.

This paper exploits corporate tax reforms across states in the U.S. to investigate the relationship between the corporate tax, employment, and tax accounting rules specifically apportionment formulas. I investigate the employment sensitivity to the tax and explore the importance of single vs multiple factor apportionment rules in the differential response of job creation to business taxation. This investigation differs from other similar empirical works on the topic (Goolsbee & Maydew 2001, Gupta & Hofmann 2003) in that it uses a different identification strategy and compares the two broadly defined regimes of apportionment. I restrict the analysis to contiguous counties (straddling across state borders) which share similar economic conditions and are more likely to provide ideal controls for tax changes on either side of the border.

While this approach has been used previously in labor economics to study the effects of minimum wage legislation (Card and Krueger, 1994, 2000 and Dube et al. 2010), only Ljungqvist and Smolyansky (2014) adopted it in the context of corporate taxation. I use the same identification strategy but explore the tax effects in the short-run. I also describe a basic theoretical framework that underscores the mechanics at play and sheds more light on the empirical findings.

My research advances the literature on corporate taxation in multiple ways. First, the spatial discontinuity identification strategy attempts to address one of the recurrent caveats of existing empirical studies on the topic by controlling for pre-existing trends in employment across different geographies. Previous works at the state-level only controlled for the time-invariant source of heterogeneity. By focusing on neighboring counties (like Ljungqvist and Smolyansky 2014) with similar economic conditions, I compare a county in a state with a corporate tax change to a control twin county on the other side of the border.

Second and importantly, even though profit-shifting across national borders has attracted a great amount of empirical works (Huizinga and Laeven 2008, Bartelsman et al. 2003, Clausing 2003), very few papers considered how this consideration plays out within the same country. Different financial accounting rules across jurisdictions within the same country favor tax minimization practices, that could reinforce the deadweight loss of a corporate tax reform. Mintz and Smart (2003) noticed that Canadian firms minimize provincial corporate tax liabilities through a variety of financial techniques, such as lending among affiliates; while Klassen and Shackelford (1998) observed that regional corporate tax collections in the U.S and Canada are concave in tax rates, consistent with firms shifting their tax bases to favorable jurisdictions. There is widespread evidence supporting the claim that formula apportionment rules in corporate profit taxation affect incentives to shift income (Gordon and Wilson 1986, Nielsen and Raimondos-Moller 2001, Hines 2009) through transfer pricing notably. Little documented though, is the impact these state rules have on investment (Gupta and Hofmann 2003) or employment (Goolsbee and Maydew 2001, Clausing 2016), notwithstanding a well-established theoretical background.

I consider the role of state rulings regarding corporate profit accounting, in the response of employment to the tax. To the best of my knowledge, only Goolsbee and Maydew (2001) and Clausing (2016) explore a similar question with employment, though the authors did not focus on the two main apportionment regimes. The majority of U.S corporations operate in multiple states and are subject to different treatments of profits earned in other jurisdictions. Most states use an apportionment formula based on sales, employment, and property to break down the portion of a corporation's profits to be taxed. After explaining how these rulings matter for a corporation trying to minimize tax liabilities, I explore how labor demand respond to these legislations in the face of a corporate tax increase.

Third, and not the least, measuring the corporate tax rate has been a challenging exercise. Many papers use the statutory tax rate (Hassett and Hubbard 1996, Carroll 2009, Ljungqvist and Smolyansky 2014 and Hassett and Mathur 2010) which features some limitations. Provisions available at the state level such as the deductibility of some costs could substantially lower the effective tax burden on firms. As a result, others prefer an effective tax rate (Laura Vartia 2008, Djankov et al. 2006 and Liu and Altshuler 2013) measured as a user cost of capital investment. I use an effective state corporate tax measure that was initially constructed by Wilson and Chirinko up to 2006. I extend the effective tax series to 2014, by collecting additional information on the deductibility of federal tax payments from state corporate tax liabilities. The extended dataset

which is available upon request captures the deductibility of federal tax liabilities in the measurement of the tax burden for firms.

The rest of the paper is organized as follows. Section 2 presents a basic theoretical framework to underscore how apportionment rules affect employment choices across states. Section 3 explores the policy context with a description of state tax accounting rules in the U.S. Section 4 describes the methodology and empirical strategy, while section 5 presents the data. Section 6 analyzes the main results and section 7 examines the robustness of these findings to alternative methodological considerations. Section 8 highlights the limitations of the study and explores the avenues for future research.

## 2. Conceptual framework

The model described below considers a multistate corporation facing several rules when filing taxes across different states. Most states use an apportionment rule to determine the share of aggregate corporate income taxable within their boundaries. Currently, a majority of states use a single sales factor apportionment rule with the breakdown of taxable income within the state being measured on the basis of sales in the state. Alternatively, other states use a formula that involves sales, employment (or payroll), and property.

The model presented below considers the incentives a multistate firm faces when allocating employment across states. Assume away trade frictions, transportation or production relocation costs and consider that labor and capital are perfectly mobile across states. This implies that production in a specific location is not constrained by sales, while wage and interest rates are equalized across jurisdictions. Suppose for simplicity that aggregate profits  $\Pi$  is measured uniformly across states. This hypothesis is inconsequential with regards to the results derived below. I will respectively consider how (i) a multiple-factor and (ii) a single sales apportionment rules affect the allocation of employment across states when corporate tax rates are altered.

Suppose that the multistate corporation operates in  $n$  states and let  $S_i$ ,  $E_i$ ,  $K_i$ , and  $P_i$  refer to sales, employment, capital and property of the corporation in state  $i$  while  $S$ ,  $E$ ,  $K$  and  $P$  represent the aggregated (national) equivalents of the same variables.  $\alpha_i$  measures the generosity of the tax code with respect to the deductibility of capital expenditures in state  $i$  and  $\theta_{is}$  represents the weight for factor  $s$  in the apportionment rule in state  $i$ . These weights are obviously constrained to lie between zero and one. For a corporation, tax payments in state  $i$  are given by:

$$T_i = \tau_i \Pi \left[ \theta_{i1} \frac{S_i}{S} + \theta_{i2} \frac{E_i}{E} + (1 - \theta_{i1} - \theta_{i2}) \frac{P_i}{P} \right] \quad \text{with} \quad \Pi = \sum_{j=1}^{j=n} F(K_j, E_j) - wE_j - \alpha_j \tau_j r K_j \quad (1)$$

Equation (1) carries important implications with respect to the way one should think about the corporate tax. First, since payroll costs are deductible from aggregate profits, the corporate tax can be treated as a subsidy to employment regardless of the jurisdiction where it is being allocated. To see this, consider the marginal impact of a one-unit increase in employment in state  $j \neq i$  (holding aggregate employment unchanged) on the corporation's tax liabilities in state  $i$ .

$$\frac{\partial T_i}{\partial E_j} = -\tau_i w \left[ \theta_{i1} \frac{S_i}{S} + \theta_{i2} \frac{E_i}{E} + (1 - \theta_{i1} - \theta_{i2}) \frac{P_i}{P} \right] \leq 0 \quad (2)$$

Second, the three-factor formula apportionment rule implicitly defines a tax on employment, sales, and property respectively in state  $i$ . To see why rearrange the expression for tax liabilities in equation (1) as follows:

$$T_i = \left[ \left( \theta_{i1} \frac{\Pi}{S} \right) \tau_i S_i + \left( \theta_{i2} \frac{\Pi}{E} \right) \tau_i E_i + (1 - \theta_{i1} - \theta_{i2}) \left( \frac{\Pi}{P} \right) \tau_i P_i \right] \quad (3)$$

This version of equation (1) clearly indicates that the corporate tax should be treated as a partial tax on employment in state  $i$  (McClure 1980, Serrato and Zidar 2016). Holding sales and property fixed, it appears immediately that the corporation would like to minimize employment in states with high tax rates. Two main insights can be drawn from equations (2) and (3). Higher tax rates in a given state  $i$  induce countervailing effects on a multistate firm's employment in that state.

The first effect which is not distortionary relates to the higher subsidy the greater corporate tax would allow the corporation to deduct no matter where employment is being counted. Holding aggregate employment  $E$ , sales  $S$ , and property  $P$  unchanged, this employment subsidy will encourage the firm to move employment to the state with a higher tax rate. The second force operates in opposite direction to the first. A high tax rate in state  $i$  implies that the implicit tax on employment is higher in that state, which would provide incentives to move employment in lower tax jurisdictions. The effect that prevails out of the two will dictate how employment responds to corporate rate changes in the presence of a three-factor apportionment formula. To formalize this argument, consider the marginal effect of the corporate rate on tax payments by a corporation in a given state  $i$ .

$$\frac{\partial T_i}{\partial \tau_i} = \Pi \left[ \theta_{i1} \frac{S_i}{S} + \theta_{i2} \frac{E_i}{E} + (1 - \theta_{i1} - \theta_{i2}) \frac{P_i}{P} \right] \quad (4)$$

Next, taking the derivative of this marginal effect with respect to employment in state  $i$  yields<sup>1</sup>:

$$\frac{\partial}{\partial E_i} \left( \frac{\partial T_i}{\partial \tau_i} \right) = \frac{\partial \Pi}{\partial E_i} \Omega + \frac{\theta_{i2}}{E} \Pi = \Omega \left( \frac{\theta_{i2}}{\Omega} \frac{\Pi}{E} - w \right) \quad \text{with} \quad \Omega = \left( \theta_{i1} \frac{S_i}{S} + \theta_{i2} \frac{E_i}{E} + (1 - \theta_{i1} - \theta_{i2}) \frac{P_i}{P} \right) \quad (5)$$

$$\text{It follows from this derivation that } \frac{\partial}{\partial E_i} \left( \frac{\partial T_i}{\partial \tau_i} \right) \geq 0 \text{ if } \frac{\theta_{i2}}{\Omega} \geq \frac{wE}{\Pi} = \frac{W}{\Pi} \quad (6)$$

In other words, as long as condition (6) holds the impact of the corporate tax on tax liabilities in state  $i$  would be higher if the corporation employs a greater share of its employees in the considered state. Condition (6) simply states that for the incidence of the tax on the corporation's liabilities in a given jurisdiction to be increasing in the level of employment in the state, the relative weight of employment in the apportionment formula used should exceed the aggregate wage share of the firm's profits.

To further illustrate this point, think of the wage-aggregate profits ratio as a reflexion of the benefits of a tax increase for the corporation, due to higher deductions in state  $i$ . On the other side, the corporate tax hike is costly to the firm and that cost depends upon the apportionment formula. When too much weight is attached to employment in this formula, the ensuing cost is borne in a greater proportion by employment. To fix ideas, let think about the range of values associated with each of the parameters in condition (6). With a uniform formula apportionment rule  $\theta_{i2}$  equals one-third and as long as we assume that the corporate firm operates across a large set of states,  $\Omega$  should reasonably be smaller than  $\theta_{i2}$  implying that the left-hand side should be higher than one. With regard to the right-hand side of condition (6), recent data from the Bureau of Economic Analysis indicate that profits have been rising relative to workers' compensation reaching an all-time high

<sup>1</sup> I am holding aggregate employment  $E$  fixed here. I am only interested in how the rate change affects incentives to shuffle employment across states to minimize tax liabilities.

level of 1.8 in 2015. This suggests that the right-hand side of condition (6) is probably less than one today, though it used to be much higher a few decades ago. As a result, condition (6) would hold probably true today in this simplistic scenario. I predict that in the face of a corporate rate increase, corporations would move labor to a different state to cushion the costs induced by the profit loss (condition 6 holds).

With a single sales factor apportionment formula, the previous derivations become:

$$T_i = \tau_i \Pi \left[ \frac{S_i}{S} \right], \quad \frac{\partial T_i}{\partial \tau_i} = \Pi \left[ \frac{S_i}{S} \right] \quad \text{and} \quad \frac{\partial}{\partial E_i} \left( \frac{\partial T_i}{\partial \tau_i} \right) = -w \left[ \frac{S_i}{S} \right] \leq 0 \quad (7)$$

This formula implies that with sales being the only factor that determines tax payments in state  $i$ , a domestic corporate tax increase provides a higher subsidy ( $-w\tau_i$  vs  $-w\tau_i S_i/S$ ) to employment both at home and elsewhere. However, the subsidy is lower at home than it is abroad. The impact of a tax increase on the corporation's liabilities is limited in states where the firm employs a greater share of its workforce, implying that one should not expect a large employment destruction in this scenario. Plus, if domestic sales are relatively important, there should not be an incentive to lower employment in state  $i$ , given that the subsidy approaches what it is in other jurisdictions. I predict that in states with a single sales factor apportionment rule, corporate rate increases should not translate into substantial disemployment effects.

I am also aware of other considerations that might alter the incentives described in this section. For instance, the presence of combined reporting and throw-back or throw-out rules will likely play a significant role in the way corporations react to changes in business income taxation. These policies affect the corporate aggregate profits  $\Pi$  or the weight share of each factor in the apportionment formula.

### 3. Policy background

#### 3.1 Why study state corporate tax changes?

Given that the federal corporate tax has not changed much since over the period 1986-2014, exploiting differences across states offers a greater deal of variation in the fiscal treatment of corporate profits over time. To illustrate these dynamics, I graph the average effective top marginal tax rates over time. Averaged across states, tax rates increased from 3.7% in 1960 to a high of 7.0% in 1993 and have since fallen to 6.5% in 2014, the lowest it has been since 1981. Only seven states have lower tax rates in 2014 than they did in 1960; 36 have higher tax rates.

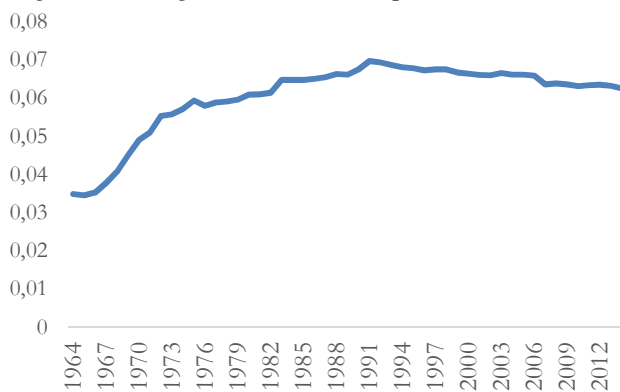
For instance, of all the states with a corporate tax in 1960, New Jersey imposed the lowest rate (1.7%) while Idaho's rate of 9.5% was the highest in the country. This pattern persisted up until 1965 with Idaho increasing its rate by one percentage point and New Jersey maintaining its corporate rate identical. However, by 2014 New Jersey had moved above the national average with a 9.0% rate while Idaho is now located in the middle of the pack with a 7.4% rate after hitting an all-time low of 6.0% in the early 70s. In the meantime, Iowa which was part of the lowest quintile in 1960 has become the state with the highest rate in 2014 (12.0%) above Pennsylvania (9.9%) and DC (9.9%). In contrast, Ohio which did not have a corporate tax in place in 1960, has moved to the bottom of the pack in 2014 with the lowest rate of 0.3%, after hitting a record of 8.9% in the mid-80s.

Before the rise of economic liberalism in the early 1980s, the typical state is about twelve times more likely to feature a corporate tax increase than a decrease in any given year. This disparity is also associated with substantial differences in rate changes. Prior to 1980, the regular state

corporate tax cut averages 0.6 percentage point while the usual tax increase averages 1.1 percentage point. As a result, there is an upward trend in the average state corporate tax rate over the period 1964-1980 (See Figure 1). In contrast, following the two major tax legislations<sup>2</sup> in the 1980s, there was a reversal in the frequency of business tax hikes and reductions.

Over the period 1988-2014, there are about three state tax cuts as opposed to one tax increase on average in a given year. Relatedly, the average rate cut substantially increased (from 0.6 percent to 1.0 percent) while the average rate hike barely changed (from 1.1 percent to 1.0 percent). This combination of factors explains the moderate downward trend observed on the average corporate tax rate curve over the period 1988-2014. It could also be linked to the growing extent of tax competition between states, as evidenced by multiple empirical studies. This features a narrowing of the divergence of the taxation of capital across U.S. states.

Figure 1: Average effective state corporate rates



However, state tax reforms are not random and could be motivated by the business cycle which itself affects labor market outcomes such as employment. Even if state tax changes were random, disentangling the change in employment that is attributable to the tax poses critical methodological challenges.

### 3.2 A note on state taxable income

U.S corporations operate in multiple states with different rules regarding the definition of taxable income. From the perspective of tax authorities, this creates a challenge, particularly with tax optimization incentives. The literature on tax minimization considerably studied how multinational businesses take advantage of existing differentials in tax rates and rules to maximize aggregate profits. The same consideration carries to the regional level since political jurisdictions in the U.S. treat business income differently. Most states use an apportionment formula to measure tax liabilities within their boundaries. I identify four main regimes of apportionment used by states: (i) single sales-factor (ii) three-factor with uniform weight, (iii) three factors with sales double, and (iv) three factors with variation in each weight.

There is substantial variation in the rules used to measure profits across states and over time. As of 2016, 24 states use a single sales-factor, 6 states use a uniform triple factor rules, 10 states use a three-factor formula with sales double-weighted, 4 states have no income tax, the remaining states use triple-factor schemes with variations in the weight attributed to each component. The

<sup>2</sup> Economic Recovery Tax Act in 1981 and Tax Reform Act in 1986



pre-eminence of sales as a basis for business income taxation has not always been quite as strong. For instance, back in 1983, only eight states assigned a weight greater than one-third on sales. This number jumped up to 31 by 2000. This trend has implications regarding the response of employment to corporate tax reforms over time. Besides, there are other differences across states with respect to the measurement of profits.

The combined reporting agreement first adopted in 1983 by 16 U.S states, requires corporations to account in their aggregate earnings, profits from the parent and all affiliated subsidiaries of the multistate corporation. Even though I do not consider the role such policies would have on the demand for labor demand, I report in the robustness checks, results for sub-samples of states with combined reporting legislation. The variation in consolidated profit reporting laws provides an additional dimension along which activity or shifting incentives differ.

Plus, given that a few states do not tax corporate income and there is a disparity in the way corporate profits are measured across states, some jurisdictions instituted throwback and throwout rules. The goal is to set a clear treatment for untaxed income. These rulings could also impact the labor demand response to a corporate tax reform since they would affect  $\Pi$  in the conceptual framework.

## **4. Methodology**

### **4.1 Identification strategy: spatial discontinuity**

A key challenge for many econometric studies to control for unobservable factors that vary over time, especially when those factors are restricted to a certain geographic space. Since it is hard to observe appropriate counterfactual trends against which to compare tax treated states, some studies consider non-treated states as controls (Felix and Hines 2009 and Djankov et. al 2006). This approach would lead to biased estimates if the parallel trend assumption does not hold across states. To circumvent this potential source of endogeneity, I focus exclusively on contiguous counties because state tax reforms are exogenous from the perspective of a county.

Unobservable time-varying determinants of employment related to climate variations, access to transportation networks or agglomeration economies are difficult to quantify or control for at the county level. However, these factors are more likely to be similar within a pair of contiguous counties, which implies that comparing these counties over time could help minimize the bias created by such unobserved heterogeneous patterns.

The use of contiguous counties to study the effects of public policy is not new in the literature. This approach draws on the twin-unit empirical strategy which controls for individual unobserved ability by comparing outcomes across two identical units. The contiguous county methodology used in this paper consists of matching each geographic unit with its neighboring counterpart and considers that proximity increases homogeneity through trade, mobility and the benefits of agglomeration economies.

The integration of labor and product markets favor the development of local economies between contiguous counties. This results in similar employment growth patterns over the long-run. Businesses on either side of the border have access to an extended local demand and workers could work on both sides of the border. Given that identification rests critically on the assumption that contiguous counties are homogenous due to their geographic proximity, I focus on closer counties (distance between centers of gravity less than 100 miles) in the robustness checks.

A significant body of empirical works use this spatial discontinuity identification approach to study the impact of policy changes on economic outcomes. These include (Card and Krueger 1994 and Dube et al. 2010) on the minimum wage, Chirinko and Wilson (2008) on tax-induced changes in the user cost of capital, Ljungqvist and Smolyansky (2014) on corporate tax changes and Rocco Huang (2008) on deregulation in the financial sector. Though the identification strategy is the same, these papers adopt different econometric specifications ranging from county group effects in panel regressions (Ljungqvist and Smolyansky 2014) to contiguous matching (Rocco Huang 2008 and Chirinko and Wilson 2008).

I use a methodology identical to Rocco Huang (2008) and compare the average growth of employment between a pair of contiguous counties before and after a state corporate tax change. I only consider isolated tax changes in order to identify the tax impact. Specifically, I focus on tax changes that are neither preceded nor followed by other tax changes in a window of three years prior to or after the considered event. Therefore, this paper compares average growth rates of employment between a pair of contiguous counties, over the three years preceding an isolated tax change to the same average over a window of three years after the change. This approach which consists of pooling several event studies is justified by econometric considerations that I describe below.

#### **4.2 Why use an event study approach?**

This paper pools several corporate tax change events across states. The tax impact is derived from a difference in difference approach. Bertrand, Duflo, and Mullainathan (2004) suggest that using a panel structure in a difference in difference identification approach underestimates standard errors in the presence of serially correlated dependent variables like employment. They do not find econometric corrections that place a specific parametric form on the time-series process to correct the problem. Nevertheless, they prove that comparing averages over periods before and after policy reforms works well.

I follow the same methodology by comparing average employment growth before and after a set of corporate tax changes between pairs of contiguous counties. Like Rocco Huang (2008), I choose a three-year period to strike a reasonable balance between the time span of the data structure and economic theory. However, I also explore the sensitivity of the results with alternative windows. This paper defines the control and treatment units along with the “pre” and “post” treatment periods as follows:

- **“Pre-” Period:** This period covers a span of three years before one of the two counties straddling a given border segment experiments a tax change. During this period, both states did not implement a corporate tax change suggesting that there was no corporate tax treatment. Both counties in a contiguous pair were not treated in the sense of a corporate tax reform but could be subjected to other policy changes. The main goal here is to control for any pre-existing trend in the outcome of interest.
- **“Post-” Period:** Like the “pre” treatment, this period covers a span of three years during which one of the two states changed its corporate tax reform, but the other state did not. The county located on the side of the border featuring the tax reform is part of the treatment group while its contiguous counterpart on the other side of the border serves as a control. Comparing the growth of employment over this treatment period will help to capture the tax incidence.

### 4.3 Are contiguous counties good controls?

I check the validity of the identification strategy by comparing contiguous county pairs over the year preceding an isolated state corporate tax reform. Comparing the absolute difference in growth of population, employment and income during these years, I intend to verify if neighboring counties located on the other side of a state borderline represent a better control for a contiguous county. If these counties approximate observable economic characteristics of a specific county more than all other counties, then using them as controls could be justified.

In the table below, I compare the average absolute difference in employment, population and personal income growth between counties located across a state borderline and their contiguous counterparts to the same difference between these counties and all other counties. The results in table 1 suggest that contiguous counties are much closer to their neighboring counties than they are to other counties in terms of demographic and economic growth opportunities. I also report the standard deviations to emphasize the variation of the differences between the two groups.

Table 1: Growth differentials with contiguous counties vs. all counties (%)

		Avg. Diff. Growth	Mean	Std. Dev.	Min	Max
<b>Employment</b>	Cont. Counties	<b>0.006</b>	0.009	0.0003	0.0439	
	All other Counties	0.012	0.012	0.0004	0.0431	
<b>Population</b>	Cont. Counties	<b>0.003</b>	0.003	0.0003	0.0148	
	All other Counties	0.009	0.005	0.0015	0.0209	
<b>Income</b>	Cont. Counties	<b>0.010</b>	0.014	0.0003	0.0651	
	All other Counties	0.023	0.020	0.0016	0.0947	

*growth measured as a percentage change relative to the previous year  
Average on absolute value of growth differences*

### 4.4 Empirical strategy

The following analysis has several objectives. First, it describes the estimation approach used in this paper. It also describes the assumptions related to the error terms and details the standard error clustering methodology. Finally, the section explains how the potential presence of spatial correlations is addressed.

#### 4.4.1 Estimation method

The primary identification assumption of this paper consists of comparing contiguous counties which are plausibly similar except for the differentials in corporate tax treatment. For a pair of contiguous counties  $i$  and  $j$ , the following system of equations represents the starting point. The dependent variable  $g_{ipt}(g_{jpt})$  refers to the growth of employment while  $X_{ipt}(X_{jpt})$  represents a set of covariates measured over the period  $t$  for county  $i(j)$  belonging to the contiguous pair  $p$ .  $\tau$  refers to the corporate tax variable which is the effective tax rate obtained from Chirinko and Wilson (2008).

The term  $\eta_{pt}$  is critical as it ensures that a pair of contiguous counties shares a similar time variant unobserved heterogeneity.  $\theta_{i(j)}$  represents the time-invariant county specific effect. Period  $t$  in this specification refers to a three-year span and the growth variable measures the average growth rate over this time span.

$$\begin{aligned} g_{ipt} &= \alpha + \theta_i + \beta\tau_{it} + \eta_{pt} + \delta X_{ipt} + \varepsilon_{ipt} \\ g_{jpt} &= \alpha + \theta_j + \beta\tau_{jt} + \eta_{pt} + \delta X_{jpt} + \varepsilon_{jpt} \end{aligned} \quad (8)$$

To identify the employment response to the corporate tax change, I difference this equation twice. First, to eliminate the unobserved time-invariant county level heterogeneity, I take the first difference for each of the counties in the set of equations (1) around a tax change. This results in the set of equations (2) which compares the average growth of employment three years prior to the tax reform to the same average three years after.

$$\begin{aligned} \Delta g_{ipt} &= \beta\Delta\tau_{it} + \Delta\eta_{pt} + \delta\Delta X_{ipt} + \Delta\varepsilon_{ipt} \\ \Delta g_{jpt} &= \beta\Delta\tau_{jt} + \Delta\eta_{pt} + \delta\Delta X_{jpt} + \Delta\varepsilon_{jpt} \end{aligned} \quad (9)$$

The first difference wipes out any time-invariant heterogeneity at the county or state level. Counties have different averages of employment growth due to county-specific characteristics such as the presence of natural resources or initial per capita income which do not vary much over time. Also, many legislations or rules which are set up at the state level affect county economic patterns, but they barely change over time. The time difference eliminates such heterogeneous state characteristics.

However, since this first difference still includes a time-variant heterogeneity component within the pair, I further refine the estimation method by subtracting the two equations in the set (2). This results in equation (3) which is the final specification used in all our regressions. Even though empirical studies using panels, control for the time-invariant heterogeneity in labor market outcomes, they generally fail to consider the differential dynamics across regions. Differencing twice allows us to eliminate the two sources of endogeneity in this type of analysis.

$$\begin{aligned} (\Delta g_{ipt} - \Delta g_{jpt}) &= \beta(\Delta\tau_{it} - \Delta\tau_{jt}) + \delta(\Delta X_{ipt} - \Delta X_{jpt}) + (\Delta\varepsilon_{ipt} - \Delta\varepsilon_{jpt}) \text{ or equivalently} \\ \Delta_t^p y_{ipt} &= \beta\Delta_t^p \tau_{it} + \delta\Delta_t^p X_{ipt} + \Delta_t^p \varepsilon_{ipt} \end{aligned} \quad (10)$$

#### 4.4.2 Standard errors

The estimation method of this paper entails significant relationships between errors across contiguous counties belonging to the same state on the one hand but also between pairs straddling the same border segment on the other side. Given that a county could be paired with several other counties located across a border portion, and state corporate tax changes affect all counties in a state, the error terms are related along several dimensions. The following hypotheses relate to these linkages between the disturbance processes, as well as the classical orthogonality condition between regressors and errors.

A1:  $E(\varepsilon_{ipt}, \varepsilon_{jqs}) \neq 0$  for  $i, j$  in pairs  $p$  and  $q \in$  the same state (S) or the same border segment (B)

A2:  $E(\varepsilon_{ipt}, \varepsilon_{jqs}) = 0$  for  $i, j$  in pairs  $p$  and  $q \notin$  the same state and  $\notin$  the same border segment

A3:  $E(\varepsilon_{ipt}, X_{jpt}) = E(\varepsilon_{ipt}, \tau_{jt}) = 0$  for  $i=j$  or  $i, j \in$  the same county pair

Assumption A3 stipulates that unexpected shocks occurring in a county that is part of a contiguous pair are unrelated to corporate tax rates and the set of regressors in either of the counties. This implies the absence of spatial spillovers to some extent. If businesses located in a treated county reacts to the tax by shifting production to the other side of the border, and this triggers household migrating or commuting to the other side of the border, tax rate shocks in a county could influence population growth in the adjacent county. Even though these behavioral reactions are much limited in the short-run, I explore this possibility as a sensitivity check.

To account for these interdependencies, the error terms are clustered along two dimensions: state and border segment. I use the code written by Mitchell Petersen (2006) based on the formula provided by Cameron and Miller (2006) for non-nested two-way clustering in their paper "Robust inference with multi-way clustering". The methodology underlying this approach is exposed in the appendix.

#### 4.4.3 Regression specifications

The main dependent variable used in all the specifications is the growth of employment ( $\text{gemp}_{pt}$ ), though I report results for personal income. As control variables, I include the traditional determinants of employment in the literature, for which I could gather time series data at the county level over the period of analysis. In addition to the corporate tax ( $\text{effcit}_{pt}$ ), the analysis controls for the growth of population ( $\text{gpop}_{pt}$ ) and the number of establishments ( $\text{firms}_{pt}$ ). To capture time variant heterogeneity at the state level, I include the average growth of employment in the interior contiguous counties ( $\text{gemp}_{cpt}$ ).

If as suggested by a few empirical works on the corporate tax (Ljungqvist & Smolyansky 2014, Fuest et al. 2017), the response of employment (or wages) to the taxation of profits is asymmetric, then one ought to consider the possibility of an asymmetric incidence of tax cuts and tax hikes separately in the analysis of the incidence. I distinguish two panels of regressions. The first panel of regressions described in equation (11) combines all tax events without distinction.

$$\Delta_t^p \text{gemp}_{pt} = \beta_1 \Delta_t^p \text{effcit}_{pt} + \delta_1 \Delta_t^p \text{gpop}_{pt} + \delta_2 \Delta_t^p \text{gemp}_{cpt} + \delta_3 \Delta_t^p \text{firms}_{pt} + \Delta_t^p \varepsilon_{pt} \quad (11)$$

The second panel highlighted in equations (12) and (13) respectively restricts the analysis to tax cuts and hikes events.

$$\Delta_t^p \text{gemp}_{pt} = \beta_1 \Delta_t^p \text{effcit}_{pt}^- + \delta_1 \Delta_t^p \text{gpop}_{pt} + \delta_2 \Delta_t^p \text{gemp}_{cpt} + \delta_3 \Delta_t^p \text{firms}_{pt} + \Delta_t^p \varepsilon_{pt} \quad (12)$$

$$\Delta_t^p \text{gemp}_{pt} = \beta_2 \Delta_t^p \text{effcit}_{pt}^+ + \delta_1 \Delta_t^p \text{gpop}_{pt} + \delta_2 \Delta_t^p \text{gemp}_{cpt} + \delta_3 \Delta_t^p \text{firms}_{pt} + \Delta_t^p \varepsilon_{pt} \quad (13)$$

In addition, each regression in the second panel is estimated on different samples of border segments. I respectively consider (a) all pairs of contiguous counties, (b) pairs for which treatment occurs in the county with a single sales factor apportionment rule and (c) pairs for which treatment occurs in the county with a multiple-factor apportionment rule. Though the weight attached to employment in this latter category varies widely, I chose to aggregate these schemes due to the complexity of the formula used in some states and limitations of the dataset. For sub-samples (b) and (c), the analysis focuses on corporate tax increases even if all results are reported.

### 5. Data Sources and samples

In this section, I describe the data used, the samples of tax events and border segments considered in this paper. First, I describe the general structure of U.S state corporate tax systems. Later, I present the sources of the data as well as the main variables used in the regressions. Finally, I analyze changes in state corporate tax rates over the period 1964-2014.

#### 5.1 Structure of U.S state corporate tax systems

On top of the federal corporate income tax, most U.S. states impose a tax on the profits of firms operating within their jurisdiction. Of the 50 states, only Nevada, South Dakota, Texas, Washington and Wyoming do not tax corporations as separate productive units in 2014 but treat them as pass-through entities. Texas and Washington impose a business sales tax identical to a value-added tax, irrespective of the legal form of organization.

Given the identification strategy which compares contiguous counties, I exclude Hawaii and Alaska which are not located on the Mainland U.S territory. Also, I include all states even those that do not tax corporations separately. These non-corporate tax states could still represent good controls for tax changes in their neighboring counterparts.

The tax schedule is not linear in most states, and provisions are made with regards to the deductibility of federal tax payments in some states. Most of the literature on corporate tax incidence uses the highest bracket of the schedule to capture the burden of the tax on firms (Ljungqvist and Smolyansky 2014 and Felix and Hines 2009). I follow the same approach in this paper but use an effective state corporate tax rate that was initially compiled by Chirinko and Wilson (2008).

## **5.2 Data sources**

This paper relies on the comparison of employment variations between contiguous counties around exogenous corporate tax reform. Of the 3,142 counties and county equivalents existing in the U.S., 3109 are located on the mainland U.S. territory. In total, I consider 978 different counties belonging to 2384 county pairs over the period 1969-2014.

The Census Bureau through the Longitudinal Employer-Household Dynamics (LEHD) platform provides detailed county-level data on employment going back to 1970. The dataset is published on a quarterly basis and covers 98% of employers surveyed by the Quarterly Census of Employment and Wages (QCEW). The Bureau also reports population statistics at the county level at the annual time step going back to 1969.

The QCEW data are derived from quarterly tax reports submitted to State workforce agencies by employers, subject to State Unemployment Insurance laws and from Federal agencies subject to the Unemployment Compensation for Federal Employees (UCFE) program. The statistics include both full-time and part-time employees. I measure the growth of employment and population as relative annual changes at the end of each year.

I also report the impact of the tax on personal income, even though the analysis is focused mainly on the response of employment. Personal income is measured by the Bureau of Economic Analysis (BEA) as the annual earnings in a given county and is broken down by residence status to account for commuters. The BEA reports these statistics going back to 1969 for each county or county equivalent in the U.S. This variable is a proxy for the county gross product.

Chirinko and Wilson (2008) compiled an extensive dataset on state corporate tax variables from 1964 to 2006. The authors report along with statutory corporate tax rates, valuable statistics on the effective corporate tax rates, investment tax credits and federal tax deductibility status by states. I extend the dataset to 2014 by collecting additional information on state corporate tax rates from the “Book of the States” and federal deductibility status from state websites. A description of the formula used by the authors to compute effective state corporate tax rates is provided in the appendix. As for the number of establishments, the Census Bureau through the County Business Patterns program releases statistics on the number of production units in each county going back to 1975. Finally, information relative to corporate profit accounting rules were obtained from Bernthal et al. 2012 in their tax apportionment report for the Wisconsin Department of Revenue.

## **5.3 Sample of tax events and border segments**

I survey 340 state tax reforms in the U.S over the period 1969-2014. Of these, 133 were tax cuts while 207 are hikes. Given that this paper considers isolated tax events, I further reduce the sample

of corporate tax events. In total, 53 tax events (32 hikes and 21 cuts) are considered isolated in this study. I respectively consider the response of employment when the tax reform occurs in a state using (i) a single sales factor and (ii) a multiple-factor for profits apportionment.

## 6. Discussion

### 6.1 The effects of the corporate tax on employment growth

All the tables in the next two sections describe the employment and personal income response to the tax. Nonetheless, the ensuing analysis would be based primarily on the employment results. Table 2 reports the main impacts on the employment (and income) response to corporate tax reforms. Panel 1 relates to the effects of tax changes in general (all tax events). In panel 2, I distinguish between tax cuts and hikes. The results indicate that tax changes affect employment and income growth in the short-run. The findings in panel 2 suggest that the aggregated effects are driven primarily by tax increases. These growth effects are measured relative to control contiguous counties.

The employment growth reduction for a corporate change equals 0.07 percentage point and is found to be significant at one percent. This estimate ranges from -0.10 to -0.03 with a 95% confidence interval. This aggregate effect is mostly driven by tax hikes which reduce employment growth in a county by 0.21 percentage point on average, while tax cuts do not seem to bear any significant positive effect on job creation. The table also reports estimates of the incidence on a county's personal income with a growth sensitivity of -0.06 percentage point in general and -0.11 percentage point for tax hikes.

The asymmetry of the tax incidence has also been found by a couple of recent empirical works. Ljungqvist and Smolyansky (2014) relate this behavioral asymmetry to the business cycle noting that corporate tax cuts only boost economic activities when implemented during recessions, while tax increases lead to significant disemployment effects in general. Using micro firm-level evidence, Fuest et al. (2017) note that corporate rate increases are associated with a negative and significant drop in wages paid by German corporations, while estimates for rate cuts are noisy and inconclusive. Overall, this asymmetric response which is at odds with traditional neoclassical theory remains a puzzle. I explore in the following section; how formula apportionment regimes affect the employment response to corporate tax increases.

Table 2: Effects of corporate tax changes

	<i>growth relative to Contiguous</i>	
	<b>Employment</b>	<b>Income</b>
	<i>Panel 1</i>	
<b>Tax change (%)</b>	-0.07*** (0.02)	-0.06** (0.03)
	<i>Panel 2</i>	
<b>Tax hike (%)</b>	-0.21*** (0.06)	-0.11* (0.06)
<b>Tax cut (%)</b>	-0.00 (0.02)	-0.03 (0.04)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%  
Growth measured as a relative change, N=1931

## 6.2 The importance of corporate profit accounting rules

To appreciate the importance of state corporate profit accounting rules this paper considers the tax effects within sub-samples of border segments. First, I examine the tax effects along border segments where the tax reform occurs in a state using a single sales factor to fraction profits. The results in table 3 suggest that the employment response to a tax change wanes down (-0.07 percentage point vs -0.04 percentage point in general) when employment is not part of the factors used to apportion profits. Specifically, as expected based on the conceptual framework, corporate tax increases do not seem to significantly reduce employment in this sub-sample. The measured estimate for employment is negative and remarkably smaller albeit not significantly different zero. In states that use sales to break up profits for taxation, the implicit tax on employment is zero, and the location of employment does not alter taxable income as much.

This result implies that the costs of corporate tax increases, cannot be mitigated through labor reallocations. It does not, however, suggest that corporations do not react to corporate tax increases in this scenario. There are several other dimensions that could be manipulated by a firm to minimize tax liabilities in response to a new policy environment. These include income shifting between affiliates that could lower the aggregate profits reported by a multistate business (Klassen 1998, Mintz & Smart 2003). Plus, other aspects of the tax code such as throwback and throwout rules, along with the presence of a combined reporting agreement could all play some role in the way firms cushion the losses induced by a rate hike. I also notice for this sub-sample that the effects of a corporate tax cut on employment remain imprecise.

Table 3: Effects with a single sales factor rule

	<i>growth relative to Contiguous</i>	
	<b>Employment</b>	<b>Income</b>
	<i>Panel 1</i>	
<b>Tax change (%)</b>	-0.04** (0.02)	-0.02 (0.11)
	<i>Panel 2</i>	
<b>Tax hike (%)</b>	-0.03 (0.02)	0.05 (0.12)
<b>Tax cut (%)</b>	-0.10 (0.07)	0.03 (0.14)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%  
Growth measured as a relative change, N=341

Next, I restrict the analysis to the sub-sample of border segments for which treatment occurs in the county using a multiple-factor apportionment rule. There is a lot of variation in the weights attached to each factor across states. Though there has been a general trend towards assigning more importance to sales over time, this was not always the case. I abstract from the magnitude of the weight attached to employment in the formula used in each state, due essentially to the data structure, which does not allow me to identify these exact weights over the entire period of study.



Table 4: Effects with a 3-factor apportionment rule

	<i>growth relative to Contiguous</i>	
	<b>Employment</b>	<b>Income</b>
	<i>Panel 1</i>	
<b>Tax change (%)</b>	-0.08*** (0.03)	-0.04 (0.05)
	<i>Panel 2</i>	
<b>Tax hike (%)</b>	-0.29*** (0.08)	-0.10 (0.06)
<b>Tax cut (%)</b>	0.02 (0.03)	0.04 (0.07)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%  
Growth measured as a relative change, N=1374

As evidenced in table 4, I notice a substantial response of labor demand to corporate tax changes in states using a three-factor apportionment rule. The estimated effect slightly decreases and equals -0.08 percentage point for this sub-sample compared to -0.07 percentage point for the full sample of events. Both parameters are significant though not statistically different. In contrast, the response of labor demand to a corporate rate increase appears to be more pronounced when the hike occurs in a state using an apportionment formula that assigns a positive weight to employment.

The estimate in the latter subgroup equals -0.29 percentage point (vs -0.21 percentage point for the full sample) implying that a corporate tax increase reduces the growth of employment by about one-third of a percentage point. This finding is consistent with the conclusions of the conceptual framework, which emphasize that wherever a multiple-factor rule is in place, the loss in profitability caused by a corporate tax increase is more significant in states where a corporation employs a larger share of its aggregate workforce. Unfortunately, I am not able to exploit a micro-level firm data structure, that would be more appropriate in this context. I also notice that the response of employment to a corporate tax cut becomes positive but remains imprecise.

## 7. Robustness checks

This section presents several checks to test the robustness of the measured effects. I investigate the validity of the parallel trend assumption between contiguous counties, and other potential pitfalls inherent to the methodology of this analysis.

### 7.1 Do counties react to state tax changes?

The empirical strategy used here compares contiguous counties around corporate tax reforms. If treated counties react to state policy changes by altering policy instruments that influence business activities and the demand for labor, this method will likely underestimate the tax impact on employment. I investigate this possibility by regressing the growth of county property and sales tax collections, expenditures, and revenue on current and lagged changes in tax rates. The results summarized in table 5 suggest that such local government interventions may have occurred. All these local public finance aggregates decreased during years of state tax reforms. This could reflect a contraction of domestic economic activity, in the aftermath of a tax reform, or counties may just be adopting policies to offset the effect of a state tax reform on their economies. As a result, the tax impacts measured here might just provide a lower bound of the true response to corporate tax changes.

Table 5: Corporate tax change and county public finance

	$\delta(\text{Protax})$	$\delta(\text{Saltax})$	$\delta(\text{Expend})$	$\delta(\text{Revenue})$
$\delta(\text{effcit})$	-0.037*** (-0.01)	-0.06*** (-0.02)	-0.079*** (-0.02)	-0.079*** (-0.02)
1. $\delta(\text{effcit})$	0.004 (0.03)	0.05 (0.05)	0.06** (0.03)	0.05* (0.03)
12. $\delta(\text{effcit})$	0.04 (0.03)	0.04 (0.03)	0.03 (0.03)	0.04 (0.03)
13. $\delta(\text{effcit})$	0.005 (0.04)	0.005 (0.06)	0.02 (0.03)	0.01 (0.03)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%

Saltax= Sales taxes, Protax=Property taxes, Expend=Gov. expenditures, revenue= Gov. revenue

## 7.2 Results with closer contiguous pairs

The spatial discontinuity identification approach requires that contiguous counties share similar economic conditions. This draws on the importance of proximity which facilitates the integration of local economies. I explore the implications of that hypothesis by analyzing the tax effects in a sub-sample of much closer contiguous counties. I use geographic coordinates of county centers of gravity to calculate the distance between contiguous pairs. The results in table 6 relate to adjacent county pairs located within a hundred miles of each other (distance between their geographic centers of gravity). The estimated effects on this sub-sample remain qualitatively similar to those reported earlier.

Table 6: Effects with closer counties (less than 100 miles)

	<i>growth relative to Contiguous</i>	
	<b>Employment</b>	<b>Income</b>
	<i>Panel 1</i>	
<b>Tax change (%)</b>	-0.05** (0.03)	-0.06** (0.03)
	<i>Panel 2</i>	
<b>Tax hike (%)</b>	-0.18*** (0.06)	-0.09 (0.06)
<b>Tax cut (%)</b>	0.01 (0.02)	-0.03 (0.04)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%  
growth measured relative to contiguous counties. N=1755

## 7.3 Results with economically similar counties

An alternative way of checking the importance of the parallel trend assumption is to restrict the analysis to contiguous counties that are similar based on observable economic characteristics. First, I calculate the difference in the growth of employment, personal income and population between a pair of contiguous counties during the years preceding a corporate tax change. Second, I construct a “similarity” index that sums up the absolute value of these differences. The higher this metric gets, the less similar the counties in a pair are (based on observable socio-economic variables). I later exclude the pairs of counties for which the index is above the top decile of the similarity distribution. The results in table 7 suggest that restricting the analysis to this sub-sample does not qualitatively affect the main tax effects.

Table 7: Tax effects with economically similar counties

	<i>growth relative to Contiguous</i>	
	<b>Employment</b>	<b>Income</b>
	<i>Panel 1</i>	
<b>Tax change (%)</b>	-0.05*** (0.02)	-0.06** (0.03)
	<i>Panel 2</i>	
<b>Tax hike (%)</b>	-0.19*** (0.06)	-0.10 (0.06)
<b>Tax cut (%)</b>	0.00 (0.02)	-0.02 (0.04)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%  
growth measured relative to contiguous counties

#### 7.4 Results in the presence of combined reporting

Combined reporting is a provision that requires corporations to report profits for parent and affiliated businesses when filing taxes. The goal is to limit income shifting within a corporation. In the presence of this legislation, firms cannot escape a corporate tax hike through transfer pricing to affiliates and a few other tax avoidance tricks. Therefore, one should expect the employment sensitivity to a tax increase to be more pronounced. The results in table 8, support this hypothesis. A one percentage point increase in the corporate tax translates into a -0.40 percentage point loss of employment growth (vs -0.26 when there is no combined reporting) in states using a multiple factor apportionment rule. Plus, for this sub-sample, the response of employment when only sales are used to apportion profits is very volatile and imprecise due notably to the limited sample size.

Table 8: Tax effects with the presence of combined reporting

	<i>growth relative to Contiguous</i>	
	<b>Employment</b>	<b>Income</b>
	<i>Panel 1</i>	
<b>Tax change (%)</b>	-0.04** (0.02)	-0.07** (0.03)
	<i>Panel 2</i>	
<b>SS -- Tax hike (%)</b>	-0.09 (0.06)	-4.10 (2.06)
<b>TF -- Tax hike (%)</b>	-0.40*** (0.10)	-0.19*** (0.03)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%  
Apportionment rule SS= Single Sales, TF=Triple Factor

#### 7.5 Cross-border spillovers

One major limitation with this spatial discontinuity identification is the presence of cross-border spillovers. These cross-border interactions are particularly concerning in studies that measure long-run effects. If businesses react to the tax by shifting activities to the other side of the border or households losing their jobs in treated counties commute to the other side of the border to take advantage of newly created employment opportunities, then the measured estimates will be biased. I explore this possibility by regressing the growth rates of employment, average wages and income in a county on current and previous corporate tax rates in the contiguous pair (using the tax events that are not considered in my main sample).

The results in table 8 do not highlight any significant spatial cross-border spillovers on employment. Though county personal income which proxies the gross county product seems to react to past corporate policy changes on the other side of the border, the employment result does not warrant any spatial interactions that would possibly undermine the consistency of my estimates.

Table 8: Spillovers of tax changes on contiguous counties

	$\delta(\text{lcemp})$	$\delta(\text{lcwage})$	$\delta(\text{lcperc})$
effcit	-1.09 (0.97)	0.29 (0.30)	0.33 (0.22)
1.effcit	0.76 (2.04)	-1.35 (1.41)	-1.06** (0.5)
12.effcit	0.96 (1.53)	2.02 (2.00)	1.37*** (0.32)
13.effcit	-0.9 (1.14)	-1.00 (0.76)	-0.63*** (0.27)

\* significant at 10%; \*\* significant at 5%; \*\*\* significant at 1%, Effcit= Effective tax rate  
Lcemp=log(emp) lcwage=log(wage) and lcperc=log(Income) in contiguous county peer

## 8. Limitations and perspective for future works

I only estimate short-run tax effects which barely account for the spatial reallocation of capital across jurisdictions. By comparing average growth rates over a period of three years, this paper measures the immediate change in employment subsequent to the tax change but the long-run distortions could be much different than what is described in this study. To estimate the long-run economic impact of a tax event using the same identification strategy, I would have to observe contiguous counties over a longer time frame which considerably reduces the number of tax reforms that would qualify as good policy events.

Also, there is good reason to believe that the response of employment to a corporate tax increase in a state using a multiple-factor apportionment rule, will depend on the weight attached to employment. When sales are given substantial importance in the scheme adopted, theory suggests that employment should not react as much to tax changes, implying a response that would be determined by the magnitude of the weight attached to each factor. Unfortunately, I am not able to obtain accurate records on the weight assigned to each factor used by a state going back to 1969. The discontinuous effect measured between the two regimes of single-sales vs multiple-factor could be masked by a heterogeneity of effects within the second group. I implicitly assumed with this approach that there are non-linearities in the relationship between apportionment rules and the labor demand incidence of the corporate tax. In contrast, if the true association between these variables is linear, then the weight share of employment in the formula should be interacted with the magnitude of rate changes to capture the true parameter of interest. As detailed datasets emerge, future works on this topic should focus on that consideration.

Alternatively, micro-level data on the activities of multistate firms would represent the ideal dataset for this problematic. One could easily relate variations in a corporation's labor demand across states to corporate tax rates and the weight assigned to employment in each state. The aggregate effects measured at the county level could be biased if general equilibrium effects are non-negligible. For instance, the true employment loss in the corporate sector could be attenuated by the demand for labor in the domestic non-corporate sector. Likewise, the fact that corporate tax cuts do not seem to boost county employment does not necessarily imply that corporations do not

react to these incentives. This finding could also be contaminated by the demand for labor in other sectors of the local economy. Using firm-level data could help mitigate these concerns.

The investigation carried out in this paper contributes to the debate on the appropriate design for the corporate tax code in multiple ways. The recent conversation around the federal corporate tax code has been framed around the opportunity of reducing tax avoidance and spur more tax collections for public entities. The opportunity of an international formula apportionment rule has been presented as a solution to this inefficiency. If tax liabilities of a multinational American corporation were to be based on the share of employment, sales, and property in the U.S., this would definitely affect the firm's allocation of employment across countries. Using a single sales based rule should technically limit this incentive, but appears more complicated in practice. Consolidation of sales requires international cooperation and there are several loopholes such as the fungibility of e-commerce sales.

Last, there is vigorous debate among public economists regarding the opportunity of a corporate tax cut. Recently the U.S. adopted a new code that substantially reduces the corporate tax rate. While the effects of this reform are still unfolding, the evidence expressed in this paper seems to suggest that the rate cut might not automatically translate in more job creation. Just like I notice that the negative impact of a corporate tax increase is highly pronounced with the presence of a multiple-factor apportionment rule, there must be other features of corporate tax policy that mediate the incidence of a business tax cut on labor demand. Going forward, the asymmetric puzzle that I discussed in the introduction, should remain a subject of investigation for scholars in the field. The evidence against a meaningful positive effect of tax reductions is increasingly getting stronger. However, labor market institutions, wage-setting mechanisms or behavioral corporate finance analysis could all help explain this oddity.

## **9. Conclusion**

In this paper, I study how formula apportionment rules affect the employment sensitivity to state corporate tax reforms. I compare the response of labor demand when the reform occurs in a state using a single sales-factor to the incidence when multiple factors are used to break down the share of profits that will be taxed.

In a first step, I found that corporate tax cuts do not bear a significant effect on employment, while corporate tax hikes reduce the growth of labor demand in the short-run. This result is consistent with a recent set of papers (Ljungqvist and Smolyansky 2014, Fuest et al. 2017) which also noticed similar asymmetric effects. My findings indicate that a one percentage point corporate tax change reduces employment growth by 0.07 percentage point. This result is driven by corporate tax increases as a one percentage point business tax hike reduces job creation by 0.21 percentage point. This effect varies with the apportionment formula adopted. When states apportion profits based off a single sales-factor, the incidence of a corporate tax hike on employment decreases substantially to -0.03 percentage point and is not statistically significant. In contrast, in states using a multiple-factor apportionment where employment is given some weight in the determination of taxable income, the impact of a one percentage point business tax increase amplifies to -0.29 percentage. These results were derived using a spatial discontinuity identification strategy that compares contiguous counties straddling state borderlines around business tax reforms. It allows me to control for time-variant determinants of employment at the local level. However, I explore the validity of this assumption in the robustness checks.

There is widespread evidence supporting the claim that formula apportionment rules in corporate profit taxation affect incentives to shift income (Gordon & Wilson 1986, Nielsen & Raimondos-Moller 2003, Hines 2009) through transfer pricing notably. Little documented though, is the impact these state rules have on investment (Gupta & Hofmann 2003) or employment (Goolsbee & Maydew 2001, Clausing 2016), notwithstanding a well-established theoretical background. The results described in this paper complement Goolsbee & Maydew 2001, who noticed that apportionment rules affect manufacturing employment and Clausing 2016 who did not observe any significant association between the two. The findings also carry some implications regarding the appropriate apportionment rule to adopt. Policymakers should be aware of the fact that, when employment is included in the set of factors that define taxable income, there is an implicit tax on employment, which limits the ability to raise tax rates without introducing other distortions on the labor market.

One major caveat of this study is that, due to the limitation of the data structure, I am not able to study the dynamic effects of the corporate tax incidence using the same identification strategy. Given that the tax triggers reallocations of factors across the economy, one should expect changes in the long-run to differ from short-run impacts. Also, the spatial discontinuity approach features a strong internal validity but is limited for external extrapolation to a different context.

Future research on this topic could be expanded along two avenues. First, upcoming empirical studies should focus on uncovering the absence of a meaningful positive effect of a corporate tax cut on employment. It would be interesting to explore how labor market institutions and the bargaining power of labor mediate the association between corporate tax cuts and labor demand or wages. Second, future works on this subject should examine the opportunity of an international apportionment formula. Though using employment as a criterion to determine taxable income, could hurt labor demand, it might be interesting to explore how effective a sales-only apportionment rule could get. Last, micro-level data on the activities of multistate firms would represent the ideal dataset for this problematic. One could easily relate variations in a corporation's labor demand across states to corporate tax rates and the weight assigned to employment in each state.

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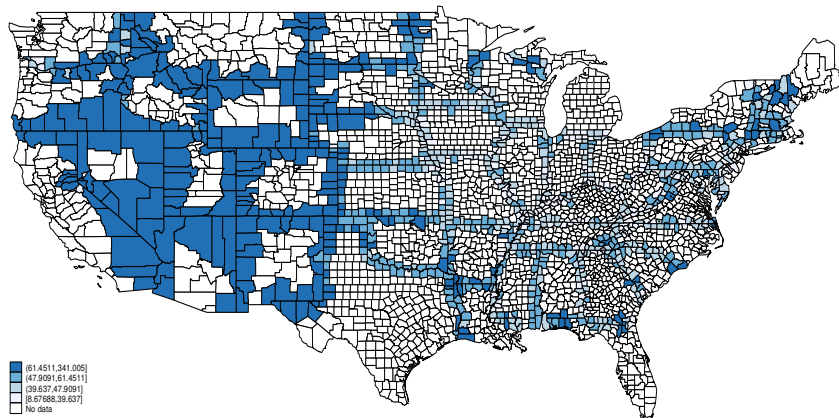
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Appendix A

**Table A1:** Summary statistics of main variables

<i>Variable</i>	<i>Description</i>		<i>Mean</i>	<i>Std. Dev.</i>
<i>Employment</i>	Employment	overall	44917.91	164338.3
		between		159964.7
		within		37477.2
<i>Effcit</i>	Effective state corporate tax rates	overall	0.0599248	0.025454
		between		0.022244
		within		0.012379
<i>Establishments</i>	Number of establishments	overall	41.89928	71.43846
		between		59.21136
		within		36.86805
<i>CIT</i>	Statutory state corporate tax rates	overall	0.0592841	0.030204
		between		0.027939
		within		0.011487
<i>Population</i>	County population	overall	83310.55	274210.3
		between		269515.1
		within		50137.87
<i>Income</i>	County personal Income	overall	1990945	8893925
		between		7228974
		within		3,773,177

**Figure A1:** Map of U.S. contiguous counties



## Appendix B: Note on state effective corporate tax rates

The corporate effective tax rates used in this paper are obtained from Chirinko and Wilson (2006). On top of the statutory corporate income tax, most states provide firms with instruments that reduce the tax burden on profits. Chirinko and Wilson considered the deductibility of federal corporate taxes from state tax liabilities to construct an effective corporate tax variable. While some states allow full deductibility of federal corporate taxes from state taxable income and other allow no deductibility at all, Iowa and Missouri allow only 50% deductibility.

Denoting the provision for federal tax deductibility in state  $s$  over period  $t$  as  $v_{s,t} = \{1.0, 0.5, 0.0\}$ , the effective corporate tax rate in state  $s$  in period  $t$  is defined by:

$$\tau_{s,t}^{E,S} = \tau_t^{L,S} (1 - \tau_{s,t}^{E,F} v_{s,t}) \quad (\text{B1})$$

where  $\tau_t^{L,S}$  denotes the statutory corporate tax rate in state  $s$  over period  $t$  and  $\tau_{s,t}^{E,F}$  represents the effective corporate tax rate at the federal level over the same period. Considering that in many states the corporate tax schedule is not linear, the authors measure  $\tau_t^{L,S}$  with the marginal legislated tax rate for the highest bracket.

Similarly, given that state corporate tax payments are fully deductible from federal tax liabilities, the effective corporate tax rate at the federal level is given by:

$$\tau_{s,t}^{E,F} = \tau_t^{L,F} (1 - \tau_{s,t}^{E,S}) \quad (\text{B2})$$

Using equations (1) and (2), Chirinko and Wilson suggested that the effective corporate income tax rates at the state and federal levels are systematically related. Solving for the effective corporate tax rates respectively at the state and federal levels yields the final expressions:

$$\tau_{s,t}^{E,S} = \frac{\tau_{s,t}^{L,S} (1 - v_{s,t} \tau_t^{L,F})}{(1 - v_{s,t} \tau_{s,t}^{L,S} \tau_t^{L,F})} \quad (\text{B3})$$

$$\tau_{s,t}^{E,F} = \frac{\tau_t^{L,F} (1 - \tau_{s,t}^{L,S})}{(1 - v_{s,t} \tau_{s,t}^{L,S} \tau_t^{L,F})} \quad (\text{B4})$$

Collecting data on state and federal corporate tax rates along with state provisions regarding federal tax deductibility, I extended the state effective corporate tax series computed by Chirinko and Wilson from 2006 to 2014.

**Appendix C: Two-way clustering by Cameron, Gelbach and Miller (2006)**

Consider situations where each observation may belong to more than one “dimension” of clusters. For instance, if there are two dimensions of clusters, each individual  $i$  can belong to a cluster  $g \in \{1, 2, \dots, G\}$ , as well as to a cluster  $h \in \{1, 2, \dots, H\}$ , and we have:

$$y_{igh} = x'_{igh}\beta + u_{igh} \quad (C1)$$

where we assume that for  $i \neq j$   $E[u_{igh}u_{jg'h'} | x_{igh}, x_{jg'h'}] = 0$  unless  $g = g'$  or  $h = h'$

Errors belonging to the same group (along either dimension) are correlated. Assuming that the two clusters are non-nested, the disturbance variance-covariance matrix  $\Omega = V[u|X]$  cannot be expressed as a diagonal matrix. A consistent two-way cluster-robust estimate of the variance-covariance matrix of the OLS estimator is given by:

$$\hat{V}[\hat{\beta}] = (X'X)^{-1}\hat{B}(X'X)^{-1} \quad (C2)$$

where  $\hat{B} = X'(\hat{u}\hat{u}'.S^{GH})X$  and  $S^{GH} = S^H + S^G - S^{G \cap H}$ .

$S^H$  is an  $N \times N$  indicator matrix with  $ij^{\text{th}}$  entry equal to one if the  $i^{\text{th}}$  and  $j^{\text{th}}$  observation belong to the same cluster  $h \in \{1, 2, \dots, H\}$ ,  $S^G$  denotes a similar  $N \times N$  indicator matrix with  $ij^{\text{th}}$  entry equal to one if the  $i^{\text{th}}$  and  $j^{\text{th}}$  observation belong to the same cluster  $g \in \{1, 2, \dots, G\}$ , and  $S^{G \cap H}$  refers to an  $N \times N$  indicator matrix with  $ij^{\text{th}}$  entry equal to one if the  $i^{\text{th}}$  and  $j^{\text{th}}$  observation belong to both the same cluster  $g \in \{1, 2, \dots, G\}$  and the same cluster  $h \in \{1, 2, \dots, H\}$ .