DISSERTATION

IN SEARCH OF THE INCIDENCE OF THE CORPORATE TAX ON EMPLOYMENT AND WAGES: EVIDENCE FROM U.S. STATE TAX REFORMS

Submitted by

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ABSTRACT

IN SEARCH OF THE INCIDENCE OF THE CORPORATE TAX ON EMPLOYMENT AND WAGES: EVIDENCE FROM U.S. STATE CORPORATE TAX REFORMS

Several controversies emerged recently following a series of corporate inversions designed to minimize corporate tax liabilities. Moreover, secular stagnation and ever-decreasing levels of corporate tax collections by federal and state jurisdictions in the U.S contributed to the resurgence of a widespread interest in tax reform. To reduce corporate tax evasion and promote economic growth, policymakers passed the "Tax Cuts and Jobs Act" in 2017 which substantially reduced the corporate income tax rate. The opportunity of the reform remains a controversial debate across the political spectrum. This dissertation explores the experience of U.S. state policy changes related to the taxation of corporations to study the extent to which the tax is passed onto workers in the form of lower wages and employment in the short-run.

The first chapter discusses the political economy of U.S. state corporate tax reforms. Using a unique dataset of state corporate tax rates, I observe that business tax changes are associated with tax competition, swings in economic cycles, and left-right political ideology. In contrast, long-term debt and budgetary pressures do not correlate with state corporate tax policies. Moreover, I document a regional heterogeneity and notice a slowdown in state tax changes after the Federal Reform Act of 1986. These findings matter for the empirics of corporate tax incidence, which is increasingly concerned with the endogeneity between tax reforms and other economic developments.

The second chapter studies the responsiveness of wages and employment to state corporate tax changes in the presence of market concentration and combined reporting legislation. I exploit policy discontinuities at state borders by pairing counties in states featuring a tax change with their contiguous counterparts in control states. I observe that corporate tax cuts do not boost employment or wages while tax hikes reduce the growth of both. When controlling for market competition, I notice that the wage sensitivity to a tax hike decreases with the number of establishments and depends on legislation regarding corporate profit reporting rules.

The third chapter evaluates the economic effects of a natural experiment created by the 2005 Ohio tax reform. The policy drastically reduced the corporate and personal income tax, as well as the property tax on machinery over the period 2006-2010. I observe several cross-sections of the Current Population Survey and compare groups of individuals in Ohio to similar individuals in (i) the Midwest and (ii) other U.S. states around the reform. Using a difference in difference identification approach, I conclude that the tax reform did not significantly boost wages and employment opportunities; but seems to have affected the reporting of self-employment earnings.

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TABLE OF CONTENTS

INTRODUCTORY CHAPTER

Over the last three decades, many OECD countries lowered their corporate tax rates. The average rate in a developed economy decreased by 20 percentage points between 1980 and 2014. In the US, the recently adopted "Tax Cuts and Jobs Act" of 2017 reduced the top marginal corporate tax rate from 35 percent to 21 percent. It also eliminated the worldwide nature of the taxable income and included a temporary tax holiday on repatriated corporate income and assets. The goal is to reduce inefficiencies, promote economic growth and remain competitive on the global stage.

Arguments in favor of the reform highlight a few facts. First, corporate tax collections as a share of federal revenue significantly dropped over the last few decades (from 23 percent in 1968 to 8 percent in 2010). Second, several controversies emerged recently following a series of corporate inversions[1] (Burger King recently) and the reluctance to repatriate corporate profits earned abroad. Third, to remain competitive on the global stage, proponents argue that the U.S. must respond to developments with respect to corporate taxation in its closest partners. In contrast, those who opposed the reform defend that it tilts the tax structure towards a more regressive scheme, given that corporate profits are predominantly earned by individuals at the higher end of the income distribution.

This research contributes to the debate by exploring the incidence of the corporate tax on labor market outcomes in the short-run. I exploit a unique dataset of state effective corporate tax rates in the U.S. over the period 1969-2014 and conclude the following: (i) the incidence of corporate tax changes is not symmetrical. Tax increases reduce wage and employment growth

[1] Process by which a corporation changes its country of residence to minimize tax liabilities.

while tax cuts do not seem to significantly boost these outcomes. (ii) market power affects the incidence of corporate tax increases on wages in the short-run. The more concentration there is, the higher the ability of firms to pass the burden of tax hikes onto workers in the form of lower wages. (iii) state corporate tax accounting rules affect the incidence of the tax. When firms are required to report consolidated profits for taxation, they react to rate increases by shifting income to other jurisdictions.

In a context of high sovereign debts and a growing need for social spending, many policymakers believed that an extensive overhaul of the treatment of capital earnings is needed to generate more fiscal resources to governments. This view contrasts with the theoretical literature which emphasizes the distortions that a corporate tax reform would create/reduce, along with the potential welfare loss/gain. Besides, the distribution of the burden of the tax between workers, capital owners, and consumers has proven to be a controversial undertaking, with a wide range of estimates.

In a globalized economy, tax competition compels countries to implement efficient corporate tax codes in order to not only reduce resource misallocations but also promote public tax collections while remaining attractive for private investments. These interrogations carry through to the regional level since most U.S. states tax corporate profits independently from the federal government. This heterogeneity across jurisdictions and over time offers a great opportunity for the empirical analysis of the corporate tax incidence. Each state could be treated as a small open economy and the effects of corporate tax reforms on labor market outcomes would certainly provide a useful insight for the expected economic benefits of the newly adopted "Tax Cuts and Jobs Act".

The first chapter examines the political economy of U.S. state corporate tax reforms and discusses the implications for the empirical literature on identification matters. It concludes that spatial correlation and time varying heterogeneity should be addressed when measuring the corporate tax effects on wages and employment. Building on this consideration, the second chapter studies the incidence of the tax by observing contiguous counties located on either side of a state borderline to capture unobserved dynamic factors that influence both labor outcomes and economic policies.

I also explore the importance of market concentration and state corporate tax accounting laws in the distribution of the burden. I observe that tax cuts do not affect wages and employment while tax increases reduce the growth of both. In addition, market concentration worsens the impact of corporate tax hikes on wages. This finding is consistent with microeconomic theory which defends that the deadweight loss of taxation is amplified when there are pre-existing market failures. I also notice that the presence of a legislation requiring corporations to report aggregate profits alters the effect of a corporate tax increase on employment and wages.

To further investigate the asymmetric effect of corporate tax increases vs. cuts, the third chapter explores the economic effects of the 2005 Ohio comprehensive tax reform using individual observations. The policy dramatically cuts both the personal and corporate income tax over the period 2006-2010. I observe that this comprehensive reform did not bear any significant impact on wages and employment. This result reinforces the asymmetric effect result found in Chapter 2.

CHAPTER 1: THE POLITICAL ECONOMY OF U.S. STATE CORPORATE TAX REFORMS

1.1 Introduction

Over the last few decades, several U.S. states have lowered their tax rates on business profits. If the incidence of such policies has received a great deal of attention, very few empirical works have explored the motivations behind such reforms. Though long-term growth, globalization, the national debt (Swank and Steinmo 2002, Tanzi 1995), tax competition, domestic business cycle, and regional shocks (Chirinko and Wilson 2017) have all been suggested as potential drivers of corporate tax reforms, no exhaustive investigation has been done at the state level. This consideration is critical for the consistency of empirical estimates of state corporate tax effects. I articulate in this paper that the major source of endogeneity when studying the incidence of state corporate tax changes in the U.S. results from spatial correlation due to tax competition, as well as political ideology that could itself affect other unobservables that determine economic outcomes.

Specifically, I conclude that the political affiliation of the party in control of a state's institutions correlates with corporate tax changes even after controlling for economic trends. I also observe that the average rate of neighboring states and pre-existing economic conditions affect incentives to change the business tax. In contrast, long-term debt and budgetary pressures do not seem to bear any significant relationship with state corporate tax reforms. I also document a heterogeneity in the frequency of tax changes across regions and notice a slowdown in state corporate tax increases after the federal tax reform of 1986.

Early classical economists have defended that the mobile nature of capital assets poses several challenges to public authorities seeking to raise taxes to finance social obligations. This

argument was later confirmed by Harberger (1962) and a number of public economists (Randolph 2006, Reveendra 1975, Gravelle 2006) who set to investigate the incidence of the corporate tax in a basic two-sector general equilibrium model. The mechanics at the source of these groundbreaking theoretical works suggest that the burden of a tax change would fall predominantly on the less mobile factor (i.e.) labor.

The empirical literature (Carroll et al. 2010, Hassett et al. 2006 and Vartia 2008) provided further evidence in support of this prediction. As a consequence, policymakers grew skeptical of the economic benefits of taxes on capital returns. However, many of the works seeking to measure the corporate tax incidence are plagued with identification issues due to the potential endogeneity between policy reforms and pre-existing trends in outcomes of interest. Understanding the political economy of state corporate tax reforms is critical for the design of an empirical strategy when estimating this causal effect.

The most cited sources of endogeneity between corporate tax policies and labor market outcomes originate from the increasing wave of globalization and the rise of social expenditures in advanced economies. The former increased the bargaining power of capital assets, which in turn exerted downward pressure on effective corporate tax rates around the globe. In contrast, the rise in social obligations and the growing presence of the public sector in rich economies tend to pull tax rates upward. This is compounded by the current debt-adverse environment which limits the ability of many governments to borrow on financial markets to offset the upheavals of the business cycle. These considerations highlight the trade-off at the center of the corporate tax debate, and the need to consider endogeneity when measuring the incidence of corporate taxation.

Alternatively, there is mounting evidence suggesting a rise of tax competition across U.S. jurisdictions to attract private investments and stimulate economic growth (Chirinko and Wilson

2017). Secular stagnation shifted the policy debate to low-productivity growth, inducing the adoption of tax expenditures by several state and local governments to attract corporations and promote capital investment. Corporate taxation is part of an arsenal of instruments at the disposal of policymakers to spur long-term economic growth. This implies that corporate tax rates in neighboring states could be useful predictors of policymakers' incentives to amend the business tax code.

Finally, ever-decreasing levels of corporate tax collections at the national and local levels contributed to the re-emergence of a widespread interest in tax reform. The main goal is to reduce inefficiencies and eliminate incentives to minimize tax liabilities through profit-shifting (Klassen et al. 2012, Mintz and Smart 2003). All ideologies across the political spectrum increasingly favor some changes to the corporate tax system. Liberal policymakers would like to close tax loopholes while conservatives defend that high corporate tax rates impede capital formation, employment, and growth.

In so far as the decreasing share of corporate income taxes in public revenue is driven by globalization and its implications on the fungibility of capital; one should expect the relationship between corporate tax rates and the usual outcomes of interest to be confounded by other forces. Also, since developments with regards to openness, trade and financial liberalization are generally set at the federal level; incentives to amend the corporate tax code in response to these forces should remain identical across states. I address this consideration with time specific effects in most regressions.

The goal of this paper is to provide a thorough description of the political economy of corporate tax reforms at the state level in the U.S. I exploit a unique historical record of state effective corporate tax rates over the period 1969-2014 which was initially compiled by Chirinko

and Wilson up until 2008. First, I extended this dataset to 2014 using information in the "Books of States" reports provided by the Council of State Governments. I then observed that state corporate tax changes are associated with tax rates in neighboring states, developments in tax reforms at the federal level, and the party affiliation of a state's political control. In contrast, movements in the business cycle and long-term debt do not predict corporate tax changes. I also observe a stark heterogeneity in the frequency of business tax reforms across regions.

The rest of the paper is organized as follows. Section 2 briefly reviews the relevant literature, with a focus on the potential determinants of tax policy reforms at the national level. Section 3 describes the structure of state corporate taxation in the U.S. and analyzes recent trends in state corporate tax hikes and cuts, while Section 4 presents the data sources. Section 5 discusses the methodology and section 6 analyzes the main findings. Section 7 highlights the implications for the empirical literature while section 8 concludes the analysis.

1.2 Relevant literature

The political economy of tax reforms has been the subject of a wealth of scholarly papers, books, and news articles. Though a great deal of this interest has captured the attention of political scientists, a growing body of development research seeks to understand the sociopolitical institutions at the source of tax reforms. Tax policy has always been suggested as a key instrument for development (Tanzi and Zee 2000, Bird R.M. 1992, Easterly and Rebelo 1993). The theoretical baseline underlying these analyses derives from the neoclassical Solow-Swan growth model and its implications for capital accumulation, growth, and development. These authors defend that taxes on certain goods or assets could impede investment and limit economic growth in the long-run.

The overwhelming consensus in this literature supports that taxes are vital for long-run economic prosperity. Personal and corporate income tax, value-added taxes on imported investment goods and investment tax credits can all be used to promote saving, investment, and growth (Tanzi and Zee 2000). Notwithstanding the fact that these predictions predominantly focus on developing nations, the same mechanics might be relevant for several advanced economies due to the current environment of low productivity growth and secular stagnation. The corporate tax, in particular, affects the user cost of capital and could alter investment incentives in the long-run. This implies that long-term economic growth could be an important motivation for corporate tax reforms both at the national and local level.

Other often cited determinants of tax policy relate to the business cycle. It is well documented that during periods of economic recession, automatic stabilizers would negatively affect public revenue and income tax collection while the opposite is likely to occur in periods of booms. In advanced economies, policymakers are generally prone to adopting countercyclical fiscal measures to offset the adverse effects of economic downturns. These include personal and business income tax relief but also investment credits and several provisions related to capital accumulation. The empirical evidence on the effectiveness of such Keynesian policies remains a longstanding controversial debate (Chodorow-Reich et al. 2012,). Several Neoclassical economists have challenged the notion that public expenditures could be used to smooth the upheavals of the business cycle. Even though the federal corporate tax code did not feature dramatic changes over time, on average five U.S. states amend their corporate tax structure every year and regional and local economic cycles might influence such decisions.

Another set of reasons why a state would alter its tax code relates to globalization and the growing competition between jurisdictions both within and across countries to attract businesses.

As evidenced by a series of papers (Altshuler et al. 2015, Devereux et al. 2008), countries are engaged in a race to the bottom with regards to business tax rates. Strategic tax competition has also been documented between states and counties in the U.S. (Brueckner 2003). The theoretical tax competition literature can be categorized by two sets of results. The generally accepted consensus of a positive tax reaction function – tax rates of a jurisdiction would move in the same direction as those of competing counterparts – has been recently challenged by a series of papers (Mintz and Tulkens 2006, Wilson and Janeba 2005) that explored the possibility of a negative or uncertain tax reaction slope. However, the empirical literature heavily favors the dominant view of an increasing "race to the bottom" between countries and states.

Alternatively, long-term debt has been suggested as a potential driver of corporate tax policy. How governments respond to debt accumulation remains a controversial subject within the macroeconomic literature. Some argue that in the face of a growing debt, policymakers should adopt corrective measures that involve meaningful increases in tax rates (Bohn 1998). This line of thought finds support in the literature of optimal government finance, which also recommends moderate budget deficits. Deviations from the stationary debt equilibrium should be offset by reduced social obligations or increases in tax rates; suggesting that state corporate tax reforms could be driven by debt considerations.

Finally, ideas such as economic theories influence policies and social outcomes. The wave of economic liberalism that emerged during the 1980s emphasized the importance of individual choice, limited government and private markets for prosperity. This shifted the political debate to the role and extent of government presence in the economy, especially in rich countries. A few empirical works (Campbell 1998) have suggested that the "Reagan Revolution" of the 1980s may have tilted all shades of the political spectrum towards market-friendly policies.

The sources of this expansion of free-market ideas during the early 1980s have also received a substantial wealth of research attention. If ideas influence policy agenda and the 1980s featured an ideological inclination in the direction of market deregulation and government retrenchment, one would expect a reduction in the frequency of state corporate tax increases after the mid-1980s. I test this hypothesis with a period dummy that equals one after 1986, year symbolizing the last major federal tax reform in the U.S. and a reference point during the Neoliberal takeover. I also explore the importance of left-right political ideology in state corporate tax reforms with a dummy measuring the party affiliation of policymakers. Figure 1.1 summarizes the potential sources of endogeneity present in the analysis of corporate tax policy reforms. The main goal of this paper is to explore the significance of these relationships which would cofound the channel of association that runs from corporate tax rates to econmic outcomes.

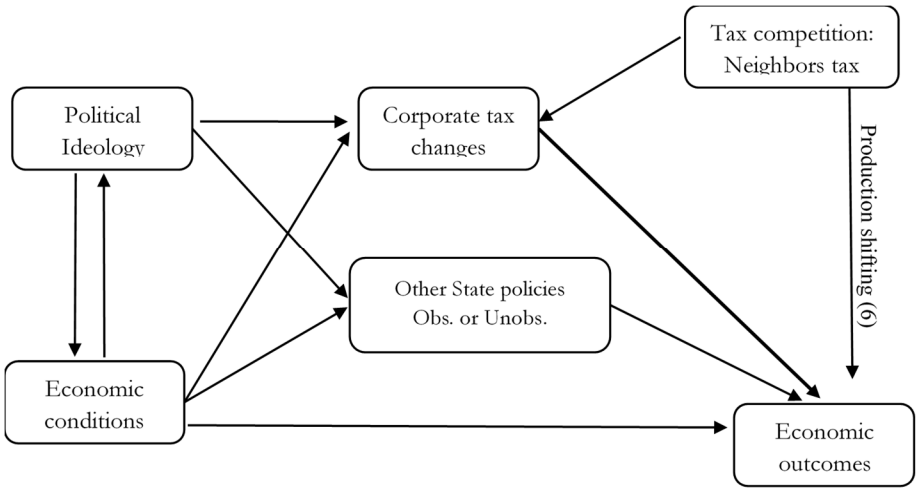

Figure 1.1: Potential sources of Endogeneity in corporate tax reforms

1.3 Background: Structure and trends of state corporate taxation in the U.S.

On top of the federal corporate income tax rate of 21 percent[2], most U.S. states impose a tax on the profits of businesses operating within their jurisdiction. Of the 50 states, only five –

[2] This represents the rate applied to the highest bracket of corporate profits after the tax Cuts and Jobs Act of 2017

Nevada, South Dakota, Texas, Washington and Wyoming – do not tax corporations as separate productive institutions in 2014. Texas and Washington impose a business sales tax on firms irrespective of their legal form of organization.

The tax schedule is not linear in most states[3], and provisions are made with regards to the deductibility of federal tax payments in some states. Also, the treatment of firms engaged in activities across several states varies widely. There is a combined reporting legislation in 23 of the 45 states with a corporate tax that requires corporations operating in multiple states to report aggregate profits for taxation. State tax liabilities are determined on the basis of an apportionment rule. This formula uses a combination of sales, property, and employment to estimate taxes due within each jurisdiction.

Among states that tax corporate profits, there is a substantial variation in the state corporate tax rate over time. To illustrate these dynamics, I plot the average statutory and effective top marginal tax rates over time (See Figure 1.2). Averaged across states, statutory tax rates increased from 3.7 percent in 1960 to a high of 7.0 percent in 1993 and have since fallen to 6.5 percent in 2014, the lowest it has been since 1981. Only sept states have lower tax rates in 2014 than they did in 1964; 36 have higher tax rates. Table 1.1 describes state corporate tax changes between 1970 and 2014.

[3] The literature treats it as a linear tax due to few number of brackets, most corporate profits usually lie in top bracket

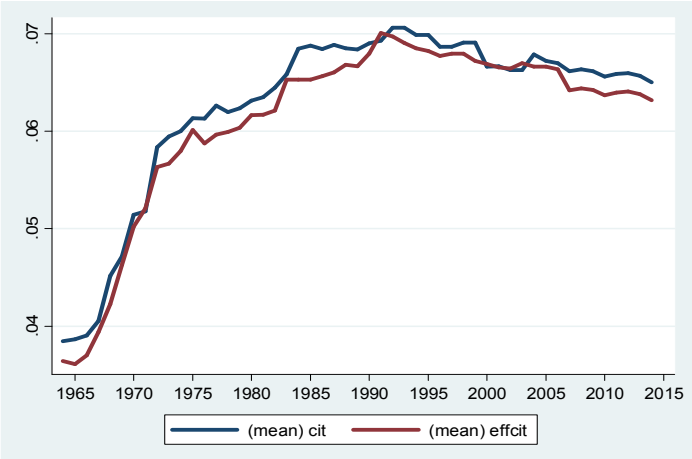

Figure 1.2: Average statutory and effective state corporate rates

The patterns of state corporate tax reforms over time could be broken down into two subperiods. 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 (See Figure 1.3). This disparity is also associated with substantial differences in rate changes. Prior to 1980, the regular state corporate tax cut averages 0.6 percent while the usual tax increase averages 1.1 percent. As a result, there is an upward trend in the average state corporate tax rates over the period 1964-1980 (See Figure 1.2). In contrast, following the two major tax legislations[4] 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 1.0 percent to 1.1 percent). This combination of factors explains the moderate downward trend observed on the average corporate tax rate curve over the period 1988-2014.

[4] Economic Recovery Tax Act in 1981 and Tax Reform Act in 1986

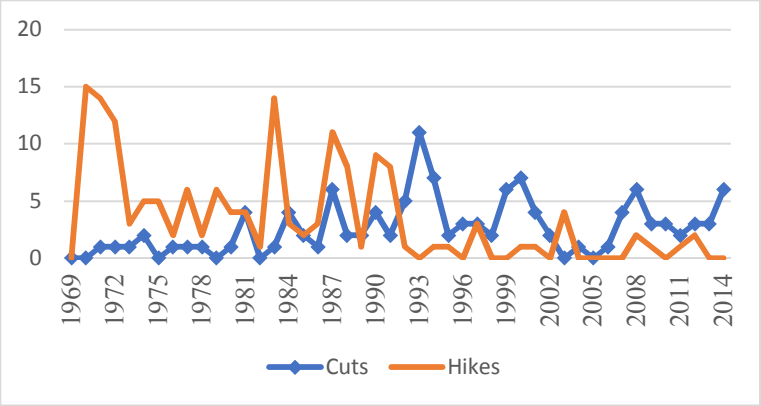

Figure 1.3: Number of state corporate tax cuts and hikes over 1969-2014

The dynamics described above could be linked to several developments. First, as discussed earlier, the 1980s were marked by a series of tax reforms at the federal level. These changes were introduced along with other policies that swayed the pendulum of political ideas towards market-friendly policies and institutions. This context may have contributed to the slowdown in policymakers' incentives to raise taxes on businesses. Plus, the growing nature of tax competition between states and localities as well as the deepening of globalization could be suggested as complementary forces that might have driven these state policies.

1.4 Data sources and variables

The subsequent analysis explores the political economy of state corporate tax reforms in the U.S. by analyzing data on state effective corporate income rates over the period 1970-2014. One recurrent issue in the empirical literature of corporate tax incidence relates to the difficulty of measuring corporate tax liabilities borne by firms. The top marginal statutory rate favored by investigators working on this topic features significant limitations. It does not capture provisions such as depreciation allowances, deductibility of some costs, and rules of expensing that would lower the effective burden of the tax on corporations.

Several U.S. states provide investment tax credits and allow firms to deduct federal corporate taxes paid from state taxes owed. These incentives reduce the effective tax rate on corporations. Using information in the "Books of the States" reports, Chirinko and Wilson constructed a series of state effective corporate tax rates over the period 1964-2006. I extended the dataset to 2014 using information available on state websites and statutory corporate rates provided by the Tax Foundation. The formula used to construct this variable is described in the appendix. Variations in this variable might result from statutory corporate rate changes but can also be inherent to developments with respect to other provisions that affect the user cost of capital. I consider both the statutory and the effective tax rates in all regressions featured in this paper.

Tax reform at the state level undergoes a process identical to the design at the federal level. Changes to the tax code usually originate from the executive branch of the government which submits a new tax legislation to both houses of the legislative branch through the budget process. The proposal would thereafter be studied by appropriate committees and submitted for voting after potential amendments. The economic literature generally assumes that policy changes reflect voter preferences. However, a recent series of works drawing on public choice theory emphasize that policymakers could be motivated by other considerations ranging from self-interest to satisfying big donors.

In this paper, I adopt the traditional view of policy changes reflecting voter choices for a few reasons. First, to the best of my knowledge, there is no organized time series of campaign financing and contributions by source for each political candidate at the state level. This information is certainly available, but it has not been compiled in a fashion that would enable its use in an empirical set-up. Second, even though the literature of public choice analysis has for long

examined the agency problem involved in public policy decisions, few works empirically tested the theoretical predictions due to the lack of an extensive dataset.

To capture the political control of a state, I use a combination of dummies that measure the ideological affiliation of the party controlling the executive branch and both state houses. This information is collected online from the National Conference and State Legislature (NCSL) website. This bipartisan organization follows changes in the political landscape, policy reforms and budget trends across U.S. states over time. I assume that a state is under Republican control when both the Executive and the Legislative Houses are controlled by the Republican party. In contrast, when both institutions are dominated by Democrats, I consider the state to be under Democratic Control. The "No control" dummy refers to a situation where no party controls all institutions simultaneously.

The Bureau of Economic Analysis provides annual statistics on employment and output by industry for every U.S. state going back to 1969. The Bureau also releases aggregates of this information for the eight economic subdivisions commonly used for regional analysis**.** I measure the growth of employment and output (Gross State Product) as relative annual changes at the end of the year. Statistics on state unemployment rates are produced by the Bureau of Labor Statistics (BLS) through the Local Area Unemployment Statistics (LAUS) program going back to 1976. This data is derived from the Current Population Survey (CPS) which follows labor market participation trends across households monthly. I use these variables to control for business cycle fluctuations at the state or regional level.

Finally, I also explore the extent to which a state's fiscal position affects corporate tax policies. Even in the presence of a Balanced Budget Requirement (BBR), a state can still run deficits over a sustained period especially with *ex-ante* BBR rules. The information on state

government debt, revenues, expenditures and fiscal position is collected by the Census Bureau going back to 1992 through the state and local government finance report. The dataset has been extended all the way back to 1967 by Pierson K., Hand M., and Thompson F. (2015) of the University of Willamette through the government finance database. Using this information, I constructed a "deficit" dummy that equals one if a state runs a fiscal deficit in any given year and control for a state's debt to output ratio in some specifications. I describe the summary statistics of the variables used in all regressions in Table 1.6 in the Appendix.

1.5 Estimation

The empirical work carried out in this paper intends to identify factors associated with a state's decision to change its business tax code. I am not seeking to uncover the causal relationships between the variables of interest and a state's decision to amend its corporate tax code. The ideal experiment that will enable the measurement of such causal effects, would require randomly assigning exogenous business cycles or public fiscal position to a state or alternative corporate tax schemes to its neighbors, and observe how otherwise similar states react to these developments. This is hard to implement, and quasi-experimental settings are cofounded by time-varying unobservables. The main objective of this analysis is to identify the set of variables that predict corporate tax reforms at the state level, hence exposing the main identification challenges that should be a matter of concern to the empirical literature.

I estimate a model with a panel of 48 U.S. states over the period 1969-2014. I excluded Hawaii and Alaska which are not located on the mainland territory and do not face the same degree of tax competition from contiguous neighbors. The design of this paper presents a variety of empirical issues including spatial correlation in tax policy but also serial correlation in tax changes

over time at the state level[5]. I address these considerations by clustering the error terms at the state and regional level. In most specifications, I included time-invariant fixed effects to capture unobserved differences in the preference for policy reforms in a state. I also included a dummy that equals one after the year 1986, which marks the last major federal corporate tax reform in the U.S. I estimate the following specifications:

$$\Delta\tau_{it} = \alpha_i + \beta_1 X_{it-1} + \theta_{11} d_{1986} + u_{it} \quad (1.1)$$

$$1_{\{\Delta\tau_{it}<0\}} = \alpha_i + \beta_2 X_{it-1} + \theta_{21} d_{1986} + u_{it} \quad (1.2)$$

$$1_{\{\Delta\tau_{it}>0\}} = \alpha_i + \beta_3 X_{it-1} + \theta_{31} d_{1986} + u_{it} \quad (1.3)$$

$\Delta\tau_{it}$ refers to the change of corporate statutory or effective tax change in state *i* during year *t* while X_{it-1} represents a set of covariates measured at *i* and *t-1* and likely to influence state corporate tax policies. This set includes the following variables: (i) debt to output ratio, (ii) the difference between a state's corporate rate and the average of its contiguous neighbors, (iii) the growth of output, (iv) a dummy for the presence of a fiscal deficit and (v) dummies for the party affiliation of the political control of a state's institutions. α_i represents state time-invariant fixed effects while θ_{k1} in equation *k* captures the post-TRA effect on state corporate tax reforms. Notice that this parameter is not identified when including year effects.

The control variables are lagged to account for the general design of tax policy reforms, which are usually announced with a one-year lead period. Plus, there is a strong contemporaneous association between corporate tax rates and several of the control variables such as budget deficit, debt or output growth; and this relationship is not of primary interest in this analysis. I am also

[5] This occurs due to several reasons such as the adoption of successive corporate tax changes within the same reform.

aware of the consideration that these control variables are endogenous, nonetheless, the lagged controls are predetermined with respect to state corporate tax reforms.

Specification (1.1) explores the determinants of the magnitude of a state corporate tax change. In contrast, specifications (1.2) and (1.3) investigate the factors driving incentives to cut or increase the rate in the first place. These last two specifications are estimated using a Linear Probability Model (LPM) with robust standard errors. Using this approach, I intend to distinguish between a rather "intensive margin" associated with the magnitude of state rate changes from a more "extensive margin" relative to the decision to cut or increase the corporate rate.

1.6 Empirical findings

Table 1.1 presents the details of the political economy of a state's statutory and effective corporate tax changes. Panel (1) describes the association between the control variables and the magnitude of a state's corporate tax change. Panels (2) and (3) distinguish between incentives driving corporate rate cuts and increases. The results exposed in the table suggest that tax competition with neighbors, economic cycles and Republican control of a state correlate with corporate tax reforms. In contrast, long-term debt and budgetary constraints do not seem to significantly influence these policy incentives. I also notice a shift in the pattern of corporate rate changes after the federal Tax Reform Act of 1986.

Table 1.1: Effects of domestic political economy on statutory and effective corporate tax reforms

Dependent variable	*Effective rate*[(a)]			*Statutory rate*[(a)]		
Specification	(1)	(2)	(3)	(1)	(2)	(3)
	Change	Cut	Hike	Change	Cut	Hike
Tax competition						
[(a)]*Diff. with avg. rate of contiguous states* $_{t-1}$	-0.047***	1.467*	-2.575***	-0.049***	1.387*	-2.083***
	(0.016)	(0.912)	(0.946)	(0.017)	(0.848)	(0.845)
Domestic economy						
[(a)]*Growth output* $_{t-1}$	-0.009***	-0.117	-0.724***	-0.006***	-0.132	-0.391***
	(0.002)	(0.144)	(0.175)	(0.002)	(0.143)	(0.148)
Political Control						
Republican Control $_{t-1}$	-0.007***	0.013	-0.048***	0.007***	0.052**	-0.029**
	(0.001)	(0.023)	(0.014)	(0.001)	(0.026)	(0.013)
Democratic Control $_{t-1}$	0.000	-0.001	0.014	0.000	0.017	0.006
	(0.001)	(0.019)	(0.015)	(0.002)	(0.014)	(0.016)
Budgetary pressures						
Budget Deficit $_{t-1}$	-0.001	-0.011	-0.030**	-0.000	-0.016	-0.005
	(0.001)	(0.013)	(0.013)	(0.001)	(0.014)	(0.012)
Debt to output ratio $_{t-1}$	-0.001*	-0.036	-0.009	-0.005	0.020	-0.041
	(0.000)	(0.046)	(0.024)	(0.004)	(0.037)	(0.019)
Post-1986 dummy	-0.006***	0.033**	-0.036***	-0.006***	0.034**	0.038***
	(0.001)	(0.017)	(0.001)	(0.001)	(0.017)	(0.012)
Constant	0.001***	0.008	0.116***	0.001***	0.002	0.112***
	(0.000)	(0.027)	(0.021)	(0.000)	(0.026)	(0.018)
Observations	1824	1824	1824	1824	1824	1824
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Region Business Cycle	No	No	No	No	No	No
Year fixed effects	No	No	No	No	No	No
R-squared	0.05	0.03	0.05	0.04	0.02	0.04

Each model is estimated by OLS with a panel of 48 U.S. states over the period 1969-2014.
All standard errors are robust and clustered at the state level. [(a)] Effective and statutory tax changes are measured in units (5% is equivalent to 0.05). ** significant at 10%; ** significant at 5%; *** significant at 1%*

1.6.1 The role of tax competition

It appears that a state is more likely to cut its corporate rate when it is above the average of its closest competitors but is more likely to increase it when it is below this same average. A one percent point above the average of a state's contiguous neighbors' rates increases the probability of a corporate effective rate cut by 1.46 percent and reduces the probability of a corporate rate increase by 2.52 percentage points. Alternatively, a one percent business point above neighbors' rate associates with an average corporate change of -0.04 percentage points.

The symmetrical interpretation of this finding implies that the standard error of this estimate might be biased if all states react to the same average target. I address this consideration by clustering the error term at the region-year level. These patterns support the hypothesis of a positive reaction curve and some degree of convergence towards a regional average, though the reaction to neighbors' policies might take a while to manifest. The literature on tax competition has generally defended that states positively react to the tax rates of competitors when designing domestic tax policy (Altshuler & Goodspeed 2003) in a game-theoretic set-up.

This consensus has been recently challenged by a series of works (Chirinko & Wilson 2017) which emphasize that the slope of the reaction curve could be negative or uncertain in the presence of heterogeneous preferences for public goods across regions. In equilibrium, voters would choose a combination of tax rates and public goods to maximize welfare. Corporate tax hikes in neighboring states will increase domestic income and might induce a business tax cut if there is a weak preference for public goods relative to private goods. The results in Table 1.1 support the traditional view of a positive reaction function, with states looking to emulate neighbors to remain competitive. Though the response to neighbors' rate changes could take a while to set in, the average rate of neighbors in the year preceding a reform is quite informative. This variable is pre-determined at the moment of a tax reform and summarizes previous corporate tax policies of a state's neighbors.

1.6.2 The importance of ideology and political control

Political ideology not only reflects voters' preferences but also affects policy developments. Right to center affiliation generally correlates with market-friendly and government retrenchment ideas. This usually translates into Republican elected officials supporting cuts to business taxation, while Democrats are more likely to undertake tax increases to finance social

obligations. The results in Table 1.1 confirm this prediction, suggesting that a Republican control of a state's institutions is more likely to induce a statutory corporate tax cut and less likely to result in a business tax hike.

Specifically, when both houses and the government of a state are under Republican control, the probability of a corporate tax cut increases by 5.20 percentage points. In contrast, under the same circumstances, the probability of a corporate rate hike shrinks by 2.92 percentage points. These effects are measured relative to the "No control" scenario when neither party controls all political institutions. This finding is interesting if one considers the fact it has been established after controlling for economic trends.

Voters' preferences for one shade of the political spectrum is often driven by economic conditions but also influences the taxation of capital. Alternatively, political ideology might associate with several other policies (such as investment tax credits, personal income tax, and other supply-side incentives) that in turn shape economic outcomes. The results presented in Table 1.1 suggest that political ideas determine corporate tax policies even when states are exposed to similar economic conditions. All else equal, a Republican control of institutions predicts a higher likelihood of a business tax cut and a lower probability of a tax increase. In contrast, democratic control does not seem to significantly affect corporate tax policies.

1.6.3 The influence of the "Reagan Revolution"

The economic liberal wave that emerged in the 1980s resulted in the adoption of several market-friendly policies. The theoretical justification for this political revolution can be traced back to the Laffer Curve, which suggests that tax cuts might translate into higher tax collections under a set of circumstances. The landmark policy that embodies this intellectual school of thought took effect through the Tax Reform Act (TRA) of 1986 that considerably reduced tax rates on

corporate and personal income. The reform was followed by a series of market deregulations set to limit the role of the public sector in the economy. This philosophical takeover seems to have influenced the frequency and direction of state corporate rate changes.

I notice that after the federal tax reform of 1986, the probability of a state corporate tax cut increases by 3.32 percentage points while the likelihood of a state corporate rate hike decreases by 3.60 percentage points. Plus, the average tax change in a state is 0.62 percentage point lower than it was prior to the reform. These results are robust to falsification tests that consider alternative structural breakpoints. The change appears to have begun around the aftermath of the TRA and remains significant regardless of the party affiliation of state policymakers.

A few factors can explain this development. First, the interdependency between states and the federal government particularly on corporate taxation sets-up a strategic interaction with respect to tax rates. Many states allow corporations to deduct federal corporate taxes paid from their state liabilities. This implies that a rate change at the federal level will alter state corporate tax revenues if nothing is done. Second, the intensification of neoliberal policies at the national level probably affected federal grants and other resources available to states, which might in turn, contribute to the adoption of business-friendly policies to promote growth and make up for the lost revenue. Third and importantly, the wave of market-oriented reforms that marked the Reagan administration may have carried through local politics and influenced voters and policymakers' preferences with respect to the size of government in the economy (Campbell, 1998**)**.

1.6.4 The impact of economic conditions and regional heterogeneity

A major driver of state corporate tax reforms originates from pre-existing economic conditions. During regional and local recessions, states are likely to provide tax breaks and several incentives to offset the adverse effects of contraction on employment. Corporate tax rates are part

of the set of instruments that can be manipulated to spur economic growth during downturns. Alternatively, states might increase corporate tax rates or borrow to finance the uptake in social obligations (increase in unemployment insurance for example) during recessions. Corporate tax reforms can also be motivated by fiscal constraints and public debt. States could increase tax rates to finance budget deficits or pay off the public debt.

The results described in Table 1.1 indicate that states are more likely to undertake corporate tax changes when economic growth is below average. In contrast, running a budget deficit or high levels of public debt does not seem to significantly affect the probability of a corporate tax change. Specifically, statutory corporate tax increase and decrease occur respectively when output growth is 0.39 percentage point and 0.13 percentage point below average, though the latter estimate is not statistically significant. I have also explored an alternative specification which controls for the average growth of output over a three-year period leading to the corporate tax change and did not notice any significant association between pre-existing economic conditions and the propensity to cut or increase corporate tax rates. The negative correlation between economic growth and increases in corporate tax rates is counterintuitive but offer some support to the hypothesis that states raise additional corporate tax revenue to finance expenditures during downturns. The empirical evidence on the political economy of tax reforms at the country-level (Swank and Steinmo 2002) favors the opposite conclusion (i.e.) a positive correlation between growth and the direction of corporate tax changes).

I also document a heterogeneity of corporate tax reforms across economic regions. As evidenced in Table 1.4 (see Appendix), states in "New England" and the "Great Lakes" are more likely to revert to statutory corporate rate increases than those in the West (Far West, Rocky Mountain, and Southwest) and the "Southeast". This result holds with the inclusion of the set of

control variables used above as well as regional business cycle trends, suggesting that when exposed to similar circumstances, states in certain regions are more likely to increase their tax rates than others. I do not notice this regional heterogeneity with regards to corporate tax cuts. This finding likely reflects unobserved heterogeneity related to the structure of economic activities in a region or the strength of regional integration.

1.7 Implications for the empirical literature

The political economy of state corporate tax reforms matters for the empirical literature which is increasingly concerned with the identification of corporate tax elasticities. The dominant strategy that exploits exogenous variations in corporate tax rates to capture these causal elasticities, though flawed (Kahn and Whited 2017), remains valuable. Figure 1.4 presents a schematic description of the complexities of corporate tax reforms. The causal effect of interest that runs from tax reforms to economic outcomes could be identified with *ad hoc* empirical approaches.

As illustrated on the graph, there are several backdoor channels that correlate with both business tax reforms and economic outcomes. Some of those are unobserved and several others are just hard to measure. An empirical investigation of the short-run corporate tax incidence would consider all associations between rate changes and economic outcomes. The figure below describes the evidence exposed in this paper. It also suggests that a reliable account of the elasticity of interest – relationship (1) – would have to block all other cofounding channels.

For instance, some empirical works on this topic have used the average rate of neighboring states/countries as an instrument for domestic corporate tax policies (Lee and Gordon 2004). This method is flawed as evidenced by relationship (6) on the graph. Though neighbors' rates influence domestic corporate tax policies, they also affect domestic production, employment, and wages through activity shifting. Businesses in neighboring states would likely react to tax changes by

moving part of their production into the domestic economy. This clearly violates the exclusion condition. Other papers have compared contiguous counties around state corporate rate changes (Ljungqvist and Smolyansky 2014), which might be effective if appropriately designed to block all sources of endogeneity.

Most empirical studies of the corporate tax incidence do not consider the possibility of tax competition (Hassett, and Mathur 2006, Hassett et al. 1996) and the implications this will bear on the treatment of standard errors. When states (or countries) react to corporate tax policy developments in neighboring economies, one classic assumption in regression analysis – $E(\varepsilon_{it}\varepsilon_{js})=0$ – will not hold true for a pair of contiguous jurisdictions (i,j) over two consecutive years (t,s). This suggests that the standard errors should be adjusted for spatial correlation using traditional GLS methods.

I also argue in this paper that an effective identification strategy is one that blocks all backdoors especially those that are unobserved or hard to capture such as the effects of neighbors' policies on domestic economic outcomes. Instruments like the average rate of contiguous states/countries are only effective when supplemented with additional controls which shut down channels like relationship (6). Variables such as distance from neighbors or the existence of regional trade agreements could mediate incentives to shift activity across the border – relationship (6) –.

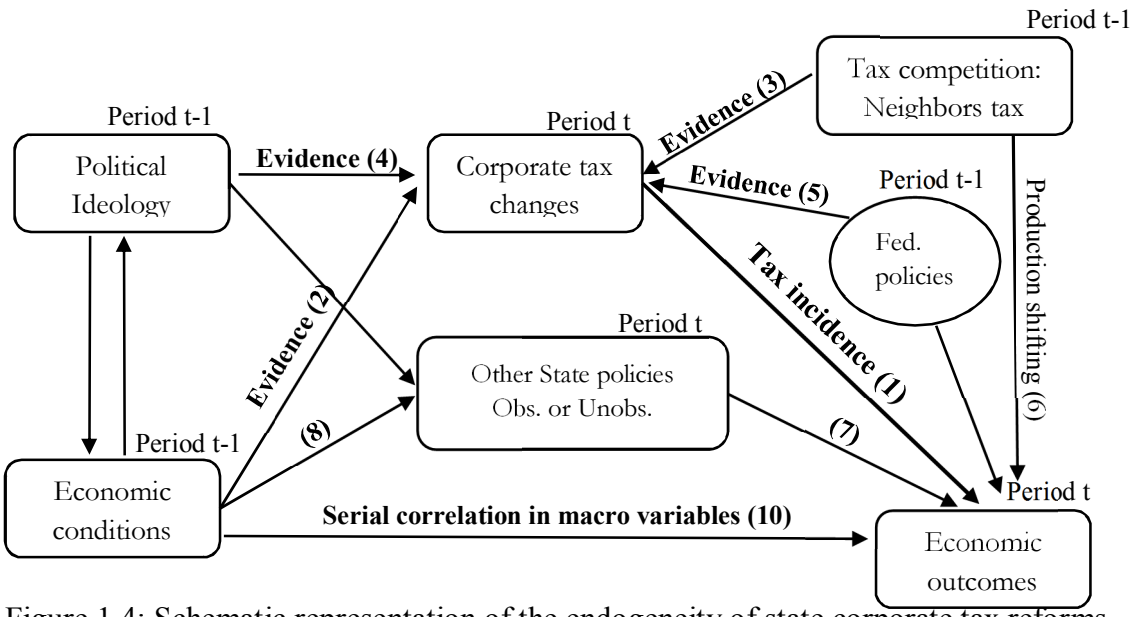

Figure 1.4: Schematic representation of the endogeneity of state corporate tax reforms

1.8. Conclusion

Corporate tax policy is the subject of numerous controversies in political circles, as evidenced by the ongoing debate on the opportunity of the "Tax Cuts and Jobs Act" of 2017. Unfortunately, the empirical literature remains unsettled on the magnitude of corporate tax effects on the economy. Part of this uncertainty is methodological and relates to the difficulty of designing an appropriate identification strategy. This limitation cannot be addressed without clearly understanding the motivations (the political economy) of corporate tax reforms. I argue in this paper that U.S. state corporate tax reforms are essentially driven by tax competition, left-right political ideology and corporate policy developments at the federal level.

Using a unique historical record of state effective corporate tax rates over the period 1969-2014, I observe that state corporate tax cuts and increases are associated with tax rates in neighboring states. First, a one percent rate point above the average of a state's contiguous neighbors' rates increases the probability of a corporate effective rate cut by 1.46 percentage points

and reduces the probability of a corporate rate increase by 2.52 percentage points. Second, Republican control of a state's political institutions increases the probability of a corporate tax cut by 5.20 percentage points and shrinks the probability of a corporate rate increase by 2.92 percentage points. Third, in the aftermath of the federal tax reform (TRA) of 1986, the probability of a state corporate tax cut increases by 3.32 percentage points while the likelihood of a state corporate rate hike decreases by 3.6 percentage points.

In contrast, the presence of a budget deficit and long-term debt do not predict state corporate tax changes. This finding differs from Swank and Steinmo (2002) who observed that budgetary pressures and debt are associated with corporate rate increases across countries. Differences in the ability to borrow on financial markets could offer a plausible explanation to this constrast. U.S. states are more limited in their ability to resort to financial markets to raise revenue compared to the federal government and many other governments around the world. As a result, public debt is generally modest at the state level and policymakers do not usually have to offset a recurrent negative fiscal position or high public debts through changes in tax rates. Plus, several U.S. states have an *ex-post* Balanced Budget Requirement (BBR) rule which constraints the propensity of sustaining fiscal deficits over a long time period.

These results have implications for the empirical literature, specifically regarding identification strategies. Empirical studies on the incidence of corporate taxation should address the presence of time varying unobservables along with spatial correlation due to tax competition. Traditional panel studies that observe countries (Vartia 2008, Felix 2007) or states (Carroll 2009) might be limited because they do not control for heterogeneous developments in economic outcomes across regions. Other papers exploit national tax reforms and observe panels of firms (Hassett and Hubbard 1996 and Hassett and Mathur 2010) around these presumably exogenous

policies. The main challenge, in this case, is to account for other policies that affect the control units.

A few empirical works on this topic have used the average rate of neighboring states/countries as an instrument for domestic corporate tax policies (Lee and Gordon 2004). This method is flawed as evidenced by relationship (6) on figure 1.4. Though neighbors' rates influence domestic corporate tax policies, they also affect domestic production, employment, and wages through activity shifting. Businesses in neighboring states would likely react to tax changes by moving part of their production into the domestic economy. This clearly violates the exclusion condition. Alternatively, a number of authors compared contiguous counties straddling state borderlines around corporate rate changes (Ljungqvist and Smolyansky 2014), which might be effective if appropriately designed to block all sources of endogeneity.

Most empirical studies of the state corporate tax incidence do not consider the possibility of tax competition (See for instance Hassett et al. 1996, Carroll 2010) and the implications this would bear on the treatment of standard errors. When states (or countries) react to corporate tax policy developments in neighboring economies, the following classic assumption of regression analysis – $E(\varepsilon_{it}\varepsilon_{js})=0$ – will not hold true for a pair of contiguous jurisdictions (i,j) over two consecutive years (t,s). This suggests that the standard errors should be adjusted for spatial correlation using traditional GLS methods.

This paper recommends using identification strategies that block all backdoors especially those that are unobserved or hard to capture. Instruments like the average rate of contiguous states/countries are only effective when supplemented with additional controls which shut down the possibility of activity shifting practices. Variables such as distance from neighbors or the existence of regional trade agreements could mediate such incentives at the firm level.

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CHAPTER 2: THE CORPORATE TAX, WAGES, MARKET POWER AND PROFIT-SHIFTING: EVIDENCE USING POLICY DISCONTINUITY AT U.S. STATE BORDERS

2.1 Introduction

The theory of public finance has contributed a substantial amount of analysis to the incidence of corporate taxation on labor market outcomes. Several empirical studies have questioned the extent to which the theoretical general equilibrium predictions hold true in the presence of market imperfection. Also, the rise of international tax evasion over the last three decades re-centered the corporate tax literature on the role of profit-shifting (Huizinga & Laeven 2008, Bartelsman et al. 2003, Kimberley 2003, Bond et al. 1989). This paper uses policy discontinuities along U.S state borderlines to examine how the degree of market power and state combined reporting legislation on aggregated profits, affect the impact of the corporate tax on wages and employment.

I conclude that the incidence of a tax change features some asymmetry. Specifically, a one percent increase in the state effective corporate tax rate significantly reduces the growth of wages paid to workers by 0.17 percentage points, and employment growth by 0.05 percentage points. However, state corporate tax cuts do not bear any significant impact on employment and wage growth. The magnitude of the incidence of a tax hike on employment strengthens along border segments with different rules of aggregate corporate profits accounting. Also, the wage sensitivity to a tax hike strengthens when there is no combined reporting agreement suggesting the potential existence of profit-shifting. When controlling for market power, I find that the wage response to a tax hike shrinks with the degree of market competition. This result is consistent with the theoretical general equilibrium analysis of Davidson and Martin (1985).

The empirical literature on the effects of the corporate income tax on labor market outcomes could be characterized by two different lines of argument. Contributions of the likes of Djankov et al. (2006) or Carroll (2009), build on the neoclassical tradition and focus on the indirect effects of the tax, which operate through factor reallocations between corporate and non-corporate sectors. In contrast, a rather heterodox approach emerged more recently and measure the so-called "direct effect" of the tax. The proponents of this school emphasize the importance of labor market institutions and other 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 & Devereux et al. 2010).

Econometric works adopting both approaches are generally plagued by methodological limitations. These relate to the violation of the parallel trend assumption across countries, industries or states in traditional panel studies (Djankov et al. 2009) as well as the potential reverse causality between labor market outcomes and corporate tax reforms (Hassett et al. 1996 and Vartia 2008). Empirically, it is hard to capture unobserved time variant heterogeneity at the country or state level.

Also, even though the literature on international profit-shifting and transfer pricing by multinational corporations has been studied extensively (Huizinga & Laeven 2008, Kimberley 2003 and Bartelsman et al. 2003), very few works have explored the importance of business profit-shifting across states in the U.S. As evidenced by Klassen (1998) and Mintz & Smart (2003), firms operating across several jurisdictions within the same country could embark on financial accounting practices designed to minimize corporate tax liabilities by shifting profits to regions or states with the lowest rates. This consideration has serious implications with regards to the analysis

of the tax impact. If wages are set up within a bargaining framework, lower reported profits might reduce labor compensation.

This paper utilizes differentials in corporate income taxation across the U.S. states to investigate the relationship between corporate tax rates on the one side and wages and employment on the other side. More specifically, I analyze the wage sensitivity to the tax in the presence of market power and explore the importance of state financial accounting laws through the combined reporting legislation. I restrict the analysis to contiguous counties (straddling across state borders) which share similar economic conditions and are more likely to provide ideal control groups for tax changes on either side of the border.

While this approach has been used extensively in labor economics to study the effects of minimum wage legislation on low-skilled labor employment (Card & Krueger, 1994, 2000 and Dube et al. 2010), only Ljungqvist et al. (2014) have tried to adopt the same identification strategy in the context of corporate taxation with noticeable limitations[6]. In this context, the dual mobility of capital and labor across state borderlines suggests that the extent of the tax incidence inherent to the indirect factor reallocation would be less important than the direct effect arising from a pre-existing market imperfection.

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 and wages across different geographic entities. Previous works using state-level differences (Caroll 2009 & Felix and Hines 2009) in corporate tax rates were only able to control for the time-invariant source

[6] In such a context, the general equilibrium predictions of the incidence of the CIT on wages are more complicated due to the dual mobility of labor and capital across state lines.

of heterogeneity in employment and wages across states. By focusing on neighboring counties (like Ljungqvist et al. 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, I consider the role of legislation on combined reporting agreement on the incidence of a corporate tax change. To the best of my knowledge, this study is the first to consider this aspect in the literature of the state corporate tax incidence. The majority of U.S corporations operate across multiple states and are therefore subject to a differential treatment of profits earned in different jurisdictions. The combined reporting agreement first adopted in 1983 by 16 states treats the parent corporation and most subsidiaries as one corporation for state income tax purposes.

Third, and importantly I expand the literature on the importance of market imperfection on corporate taxation by exploring the importance of local market concentration on the wage sensitivity of corporate taxes. Previous empirical works that studied the corporate tax incidence on wages in the presence of market failure, usually exploit national sectoral data on business concentration and federal corporate tax rates, coupled with individual wage records from population surveys (Liu and Altshuler 2013). This research improves upon that approach by using the number of establishments at the county level as a concentration measure.

The rest of the paper is organized as follows. Section 2 briefly reviews the relevant theoretical and empirical literature, with a focus on the challenges in estimating the impact of the corporate tax on wages and employment. Section 3 explores the policy context with a description of the legislation on state combined reporting agreement. 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 several alternative methodological considerations. Section 8 highlights the limitations of the study and explores the avenues for future research.

2.2 Relevant literature

Harberger's groundbreaking contribution (1962) remains a valuable reference for the analysis of the incidence of the corporate income tax. In this model, Harberger considers a neoclassical closed economy with two sectors and treats the tax as a partial factor tax on capital in the corporate sector. He concludes that labor could bear a significant share of the long-run incidence of the tax depending on a set of elasticities. As capital tries to escape the corporate sector, the reallocation of resources across industries that ensue in the domestic economy would alter the relative returns to capital and labor. An extensive review of the literature of general equilibrium corporate tax incidence is provided in Appendix 2.D.

The rise in international flows of capital, goods, and services over the last three decades, underscores the need to adopt open-economy models when analyzing the incidence of the corporate tax. A series of recent works have addressed this challenge (Grubert & Mutti 1985, Gravelle & Smetters 2006, Randolph 2006, Gravelle 2008). These extensions of the classic Harberger benchmark emphasize that labor is even more vulnerable in a global economy as capital could move abroad to avoid the tax.

The importance of market concentration has made some scholars question the absence of imperfect competition in the benchmark Harberger model. There have been multiple attempts to extend the original two-sector model by accounting for imperfect labor market (Lockwood 1990),

monopolistic competitive corporate industries (Atkinson and Stiglitz 1980) and oligopolistic sectors (Davidson and Martin 1985).

The theoretical model that underlies the empirical work carried out this paper is taken from Davidson and Martin[7] (1985). The authors adopt a closed economy with an oligopolistic corporate sector. Considering that firms operate within an infinitely repeated Cournot game, they suggest that a tacit collusion could emerge, whereby each firm produces below the perfectly competitive quantity, resulting in a higher labor share of the burden of the tax. This implies that the response of wages to the corporate tax depends on the degree of market competition, which the analysis captures by the number of firms.

The empirical literature on the corporate tax incidence could be divided into two broad categories. The first strand builds on the neoclassical tradition and focuses primarily on the indirect effect of the taxation. These papers usually observe panels of firms (Hassett & Hubbard 1996 and Hassett & Mathur 2010), countries (Laura Vartia 2008, Djankov et al. 2006 and Felix 2007) or regions (Carroll 2009 for U.S states) over an adequate period to study the impacts of corporate tax changes on wages, employment, or investment. But the use of fixed effects only controls for time-invariant heterogeneity at the country, state or industry level. Many other unobserved determinants of labor market outcomes vary over time and cannot be captured by a traditional panel structure.

Ljungqvist and Smolyansky (2014) addressed this shortcoming by considering contiguous counties that straddle state borderlines. Even though the authors consider time variant heterogeneity and address the potential reverse causality between taxes and employment or income on the other side, the mechanics according to which the tax impacts labor are not clearly identified.

[7] The model is presented in the appendix.

The dual mobility of labor and capital across contiguous counties, suggests that in the long-run, the extent of the tax incidence related to capital movement may not be as severe in this set-up. I use the same identification strategy in this paper but explore the tax effects in the short-run.

The other school of the empirical literature focuses on the importance of market failures or imperfections in the corporate tax incidence. It builds on a rather heterodox tradition which emphasizes the role played by labor and product market institutions in the analysis of tax incidence. Most of this literature assess the impact of the tax in the presence of a bargaining arrangement between workers and firm owners (Arulampalam et al. 2010 and Felix & Hines 2009) or highly concentrated productive sectors that enable the shifting of the tax to consumers and workers (Liu & Altshuler 2013).

Arulampalam et al. (2010) compare earnings of union and non-union workers to appreciate the role of wage bargaining while Liu and Altshuler use the share of the revenue made by the four biggest firms in each sector to capture the importance of market power. I consider the number of establishments at the local level as suggested by the theoretical model of Davidson and Martin, to capture the extent of market competition.

Besides, measuring the corporate tax rate has been a challenging undertaking. Most scholars consider the statutory tax rate (Hassett & Hubbard 1996, Carroll 2009, Ljungqvist & Smolyansky 2014 and Hassett & Mathur 2010) which features several limitations. Provisions available at the country or state levels such as the deductibility of some costs or depreciation allowance rules could substantially lower the effective tax burden on firms. As a result, other papers preferred an effective tax rate (Laura Vartia 2008, Djankov et al. 2006 and Liu & Altshuler

2013) measured as a user cost of capital investment. I use an effective state tax rate that was initially compiled by Chirinko and Wilson (2008).

Finally, even though profit-shifting across national borders has attracted a great amount of empirical works (Huizinga & 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 & Smart (2003) noticed that Canadian firms minimize provincial corporate tax liabilities through a variety of financial techniques, such as lending among affiliates; while Klassen & 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 favorably taxed jurisdictions. I exploit differentials in corporate financial accounting rules across U.S states through the combined reporting agreement to assess how alternative tax filing requirements affect the corporate tax incidence.

2.3 Policy background

2.3.1 Why study state corporate tax changes?

Given that the federal corporate tax has not changed much since the Tax Reform Act of 1986, 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 (figure 2.1). 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.

The time variation in tax rates across states could be summarized in two major episodes. Up until 1980, there is an upward trend in the variability of rates between states. Since 1981 and after the two major tax reform acts passed in the early 1980s, there has been a convergence in corporate tax rates between states coupled with a decrease in the average tax rate over time. This could 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.

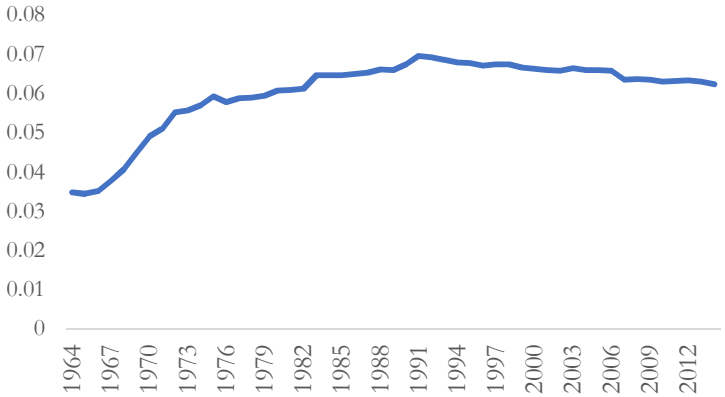

Figure 2.1: Average effective state corporate rates

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 1970s. 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.

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

2.3.2 A note on state combined reporting

U.S corporations operate in multiple states with different rules with regards to the definition of taxable income. From the perspective of tax authorities, this posits a challenge, particularly considering tax optimization incentives. The literature on corporate tax avoidance has considerably studied how multinational businesses take advantage of existing differentials in tax rates and rules relative to the treatment of income to maximize aggregate profits.

The same consideration carries to the regional level since different localities in the same country might treat business income differently. The combined reporting agreement first adopted in 1983 by 16 U.S states, requires corporations to report aggregate parent and dependent subsidiary earnings for taxation. In this paper, I study how these differences in corporate tax filing rules influence the impact of a tax reform. The variation in profit reporting laws provides an additional dimension along which activity or income shifting incentives differ.

U.S corporations are generally comprised of a “parent” and several “subsidiaries” owned by the parent. Combined reporting essentially treats the parent and most subsidiaries as one corporation for state income tax purposes. This law has grown remarkably over the last three

decades and is now present in the majority of U.S states that tax corporate income (23 of the 45 states with a corporate tax).

This consideration matters when analyzing the incidence of U.S state tax changes and even more so given my identification strategy which compares contiguous counties that straddle state borderlines. In the absence of the agreement, a firm might react to a corporate tax change by shifting income to the other side of the border. As a result, the decline in reported profits could be more harmful to wages if earnings are determined by a bargaining set-up. Similarly, different rules in the treatment of aggregated profits along a borderline could provide an incentive to shift production to the most favorable jurisdiction, while servicing the same market. To the best of my knowledge, this paper is the first to examine how these behavioral patterns influence the impact of state corporate tax changes on wages and employment.

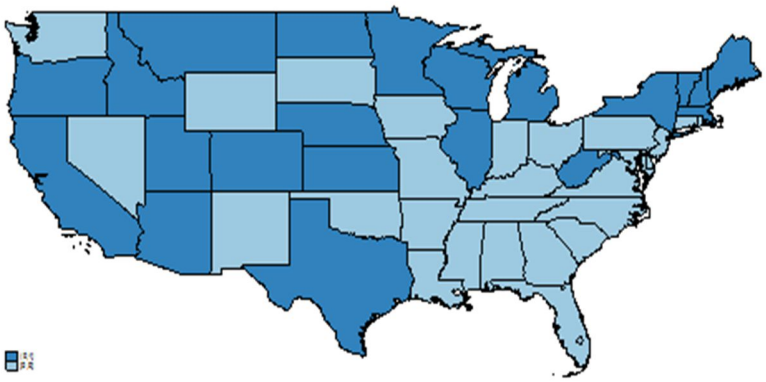

Figure 2.2: States with a combined reporting agreement (dark shade)

2.4 Methodology

2.4.1 Identification strategy: spatial discontinuity

One of the critical challenges of many econometric studies relates to the inability 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, most studies consider non-treated states as controls (Felix & Hines 2009 and Djankov et al. 2006). This approach would lead to biased estimates if as expected, the parallel trend assumption is violated across states. To circumvent that potential endogeneity, I focus exclusively on contiguous counties because state tax reforms are exogenous from the perspective of a county although I also check the robustness of this hypothesis by excluding large economic counties.

Unobservable time-varying determinants of employment and wages 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, suggesting 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 quite extensive 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 entity with its closest 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 and wage 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 this identification approach rests critically on the assumption that contiguous counties are homogenous due to their geographic proximity, I explore the validity of that hypothesis by considering counties which are much closer

and as a result are more likely to serve as controls for tax changes on either side of the border. I do so by restricting the analysis to contiguous counties located within a hundred miles of each other's respective center of gravity.

A growing body of empirical works exploits discontinuity at state borders to study the effects of policy changes on growth opportunities and labor market outcomes. These include (Card & Krueger 1994 & Dube et al. 2010) on the minimum wage impact on labor market outcomes, Chirinko & Wilson (2008) on tax-induced changes in the user cost of capital on business location, Ljungqvist & Smolyansky (2014) on corporate tax changes on employment and income and on deregulation in the financial sector on economic growth. Even though the identification strategy is the same, these papers adopt different econometric specifications ranging from county group effects in panel regressions (Ljungqvist & Smolyansky 2014) to contiguous matching (Huang 2008 & Chirinko & Wilson 2008).

I use a methodology identical to Huang (2008) and compare the average growth of employment and wages between a pair of contiguous counties before and after a state corporate tax change. I only consider isolated tax changes in order to precisely 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 or after the considered event. Therefore, this analysis compares average growth rates of employment or wages 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.

2.4.2 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 (Ljungqvist & Smolyansky 2014) in a difference in difference identification approach underestimates standard errors in the presence of serially correlated dependent variables like employment or wages. 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 and wage growth before and after a set of corporate tax changes between pairs of contiguous counties. Like 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 variables 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, wages and income over this treatment period will help to capture the tax incidence.

2.4.3 Are contiguous counties good controls?

I investigate the validity of the identification strategy used in this paper 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 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 easily 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 U.S. counties. The results in table 2.1 suggest that contiguous counties are much closer to their neighboring counties than they are to other counties in terms of economic and demographic growth opportunities. I also report the standard deviations to emphasize the variation of the differences between the two groups.

Table 2.1: Growth differentials between contiguous counties vs. all counties (%)

	Avg. Diff. Growth	Mean	Std. Dev.	Min	Max
Employment	Cont. Counties	**0.006**	0.009	0.0003	0.0439
	All other Counties	0.012	0.012	0.0004	0.0431
Population	Cont. Counties	**0.003**	0.003	0.0003	0.0148
	All other Counties	0.009	0.005	0.0015	0.0209
Income	Cont. Counties	**0.010**	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

2.4.4 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.

2.4.4.1 Estimation method

The primary identification assumption of this paper consists of comparing contiguous counties which are identical in all respects 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, real average wages or personal income while $X_{ipt}(X_{jpt})$ represents a set of covariates measured over the period *t* for a county *i*(*j*) belonging to the contiguous pair *p*. τ refers to the corporate tax variable which is the effective tax rate obtained from Chirinko and Wilson (2008).

The term λ_{pt} is critical in this specification as it ensures that a pair of contiguous counties shares a similar time variant unobserved heterogeneity. θ_i represents the time-invariant county

specific effect. Period *t* in this specification refers to a three-year span and the growth variables measure average growth rates over this time span.

$$\begin{aligned} g_{ipt} &= \alpha + \theta_i + \beta\tau_{it} + \lambda_{pt} + \delta X_{ipt} + \varepsilon_{ipt} \\ g_{jpt} &= \alpha + \theta_j + \beta\tau_{jt} + \lambda_{pt} + \delta X_{jpt} + \varepsilon_{jpt} \end{aligned} \tag{2.1}$$

To identify the employment and wage 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 (2.1) around a tax change. This results in the set of equations (2.2) which compares the average growth of employment and wages three years prior to the tax reform to the same average three years after. The first difference wipes out any time-invariant heterogeneity at the county or state level. Counties feature different averages of employment or wage growth due to county-specific characteristics which do not vary much over time such as climate, the presence of natural resources or initial per capita income. 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 heterogenous state characteristics.

$$\begin{aligned} \Delta g_{ipt} &= \beta\Delta\tau_{it} + \Delta\lambda_{pt} + \delta\Delta X_{ipt} + \Delta\varepsilon_{ipt} \\ \Delta g_{jpt} &= \beta\Delta\tau_{jt} + \Delta\lambda_{pt} + \delta\Delta X_{jpt} + \Delta\varepsilon_{jpt} \end{aligned} \tag{2.2}$$

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 (2.2). This results in equation (2.3) 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, through the inclusion of fixed specific effects, they fail to consider the differential in

wage and employment dynamics across regions. Differencing twice (resulting in Δ_t^p) allows us to eliminate the two sources of endogeneity that arise in this type of analysis.

$(\Delta g_{ipt} - \Delta g_{jpt}) = \beta(\Delta\tau_{it} - \Delta\tau_{jt}) + \delta(\Delta X_{ipt} - \Delta X_{jpt}) + (\Delta\varepsilon_{ipt} - \Delta\varepsilon_{jpt})$ or equivalently

$$\Delta_t^p y_{ipt} = \beta\Delta_t^p\tau_{it} + \delta\Delta_t^p X_{ipt} + \Delta_t^p\varepsilon_{ipt} \quad (2.3)$$

2.4.4.2 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

The third 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 correlations 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, this paper explores the existence of spatial correlations 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.

2.4.4.3 Regression specifications

The main dependent variables used in all the specifications are the growth of employment and the growth of real weekly average wages. As control variables, the paper includes the traditional determinants of wages and employment in the literature, for which I could gather time series data at the county level over the period of analysis. Throughout all specifications, the analysis controls for the following county-level variables: the growth of population, the number of establishments and the state effective corporate tax rate. To capture time variant heterogeneity at the state level, I also include the average growth of employment in the interior contiguous counties.

If as suggested by Ljungqvist & Smolyansky 2014, the behavioral response of businesses to the taxation of profits is asymmetric, then it is necessary to consider the possibility of an asymmetric incidence of tax cuts and tax hikes separately in the analysis of the incidence. I adopt several panels of regressions. The first set of regressions described in panel (2.4) combines all tax events without distinction. The second set highlighted in panel (2.5) differentiates between tax hikes and cuts in both the wage and employment regressions.

$$\begin{aligned}\Delta_t^p gemp_{pt} &= \beta_1 \Delta_t^p effcit_{pt} + \delta_1 \Delta_t^p gpop_{pt} + \delta_2 \Delta_t^p gemp_{cpt} + \delta_3 \Delta_t^p firms_{pt} + \Delta_t^p \varepsilon_{pt} \\ \Delta_t^p gwage_{pt} &= \beta_1 \Delta_t^p effcit_{pt} + \delta_1 \Delta_t^p gpop_{pt} + \delta_2 \Delta_t^p gemp_{cpt} + \delta_3 \Delta_t^p firms_{pt} + \Delta_t^p \varepsilon_{pt}\end{aligned} \tag{2.4}$$

$$\begin{aligned}\Delta_t^p gemp_{pt} &= \beta_1 \Delta_t^p effcit^+_{pt} + \beta_2 \Delta_t^p effcit^-_{pt} + \delta_1 \Delta_t^p gpop_{pt} + \delta_2 \Delta_t^p gemp_{cpt} + \delta_3 \Delta_t^p firms_{pt} + \Delta_t^p \varepsilon_{pt} \\ \Delta_t^p gwage_{pt} &= \beta_1 \Delta_t^p effcit^+_{pt} + \beta_2 \Delta_t^p effcit^-_{pt} + \delta_1 \Delta_t^p gpop_{pt} + \delta_2 \Delta_t^p gemp_{cpt} + \delta_3 \Delta_t^p firms_{pt} + \Delta_t^p \varepsilon_{pt}\end{aligned} \tag{2.5}$$

Panel 6 improves upon the previous equations and considers the extent to which market competition affects the impact of the tax. Given the results of the first two sets of regressions and since Davidson and Martin (1985) suggest that market concentration alters the incidence of the corporate tax on wages, the analysis only studies the impact of market power in the wage regressions and focus particularly on tax hikes. As could be noticed, the tax effects measure short-run effects in all our specifications.

$$\Delta_t^p gwage_{pt} = \beta_1 \Delta_t^p effcit^+_{pt} + \beta_2 \Delta_t^p effcit^-_{pt} + \delta_1 \Delta_t^p gpop_{pt} + \delta_2 \Delta_t^p ginc_{cpt} + \delta_3 \Delta_t^p firms_{pt} + \delta_4 \Delta_t^p firms.\Delta_t^p effcit_{pt} + \Delta_t^p \varepsilon_{pt} \quad (2.6)$$

In addition, each panel of regressions is estimated on different samples of border segments. I respectively explore the tax effects on (a) all pairs of contiguous counties, (b) pairs of contiguous counties with no combined reporting on either side of the border, (c) pairs of contiguous counties with different combined reporting agreements and (d) pairs of contiguous counties with a combined reporting legislation. For sub-sample (c), the analysis focuses on tax hikes that occur in a combined reporting state while the contiguous county is in a state with no such agreement.

2.5 Data Sources and samples

In this section, I describe the data used as well as 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.

2.5.1 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 businesses operating within their jurisdiction. Of the 50 states, only Nevada, South Dakota, Texas, Washington and Wyoming do not tax corporations as separate productive institutions in 2014 but

treat them as pass-through entities. Texas and Washington impose a business turnover tax, irrespective of the legal form of organization.

Given that this paper focuses on U.S contiguous counties, I exclude Hawaii and Alaska which are not located on the Mainland U.S territory. Also, I include all other states in this analysis even those that do not tax corporations separately. The identification strategy of this paper compares differences in growth rates of contiguous counties around tax changes, so 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. Also, the treatment of firms engaged in activities across several states varies substantially. Most of the literature on corporate tax incidence utilizes the highest bracket of the schedule to capture the burden of the tax on firms (Ljungqvist et al. 2014 and Felix & Hines 2009). I follow the same approach in this paper but use an effective state corporate tax rate that was initially compiled by Chirinko & Wilson (2008). A description of the construction of this effective tax rate is provided in the Appendix 1.B.

2.5.2 Data sources

This paper relies on the comparison of employment and wage variations between contiguous counties which feature identical economic trends except for differentials in corporate tax treatment. 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 and wages going back to 1970. The dataset is published on a quarterly basis and covers 98% of employers surveyed by the Quarterly Census of Employment 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 and the growth of real wage as the relative annual change of weekly average wages at the end of the first quarter.

Chirinko & 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 state. 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 1.B.

I proxy market competition with the number of establishments located in the county. The Census Bureau through the County Business Patterns (CBP) program releases statistics on the number of production units in each county going back to 1975. The higher this metric gets in a locality; the less local firms hold a sizable market and the smaller the incidence of a tax hike on average wages. Though this proxy features some limitations, it does measure in a way the extent

of tacit collusion between firms in a given geography. Irrespective of market size, a limited number of firms would generally increase the incentive to collude and sustain the same level of profits even after a tax increase[8].

2.5.3 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.

To capture the importance of combined reporting legislation, I analyze different samples of border segments. I consider border segments featuring pairs of states with a combined reporting ruling, no combined reporting ruling, and different combined reporting rulings. This allows me to assess the extent to which state corporate tax accounting rules influence the incidence of a tax change.

2.6 Analysis

2.6.1 The effects of the corporate tax on wages and employment

Table 2.2 reports the main impacts for the wage, and employment responses to corporate tax reforms. Panel 1 relates to the effects of tax changes in general. In panel 2, I distinguish between tax cuts and hikes. The results described in table 2.2 indicate that tax changes affect employment and wage growth in the short run. These growth effects are measured relative to control contiguous counties. However, the findings in panel 2 suggest that these effects are driven primarily by tax

[8] This is the channel described in Davidson and Martin (1985)

hikes, as that cuts do not significantly boost employment or wage growth while tax hikes reduce both.

The employment growth reduction for a one percentage point corporate change equals 0.05 percentage point and is significant at 1%. This effect ranges from -0.10 to -0.01 with a 95% confidence interval. This cumulative effect is mostly driven by tax hikes which reduce employment growth in a county by 0.17 percentage point on average, while tax cuts do not seem to bear any significant effect on jobs. The wage growth response to a one percentage point tax change equals 0.17 percentage point which is significant at 10% and is mostly driven by tax hikes (-0.14 percentage point).

The asymmetry of the tax impact could be explained by the unequal bargaining power between workers and firm owners among other theoretical considerations. If business owners hold a higher bargaining power because of market institutions such as union and antitrust laws or market concentration in a given region which limits the availability of alternative sources of employment to workers, one would expect the incidence of corporate tax reforms to differ in function of the direction of the change. There is a wealth of research in contract theory that has documented the unequal bargaining power between labor and capital (Bagchi 2009). If this hypothesis holds true, one should expect businesses to pass part of the costs of tax hikes onto workers in the form of lower wages, while retaining a significant portion of the benefits induced by corporate tax cuts. Unfortunately, I am not able to disentangle the part of the average wage decrease inherent to a reduction in hours demanded from the share related to a cut of wages *per se*.

Table 2.2: Incidence of corporate tax changes

	growth relative to Contiguous	
	Employment	**Wage**
	Panel 1	
Tax change (%)	-0.05***	-0.17*
	(0.02)	(0.09)
	Panel 2	
Tax hike (%)	-0.17***	-0.14*
	(0.06)	(0.08)
Tax cut (%)	-0.006	0.02
	(0.02)	(0.06)

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

2.6.2 The importance of combined reporting

To appreciate the importance of state corporate tax accounting rules, I explore the tax effects within sub-samples of border segments. First, I examine the tax impacts along borders with no combined reporting legislation on either side. The results exposed in Table 2.3 suggest that the wage response to a one percentage point tax change strengthens (-0.43 percentage point vs -0.17 percentage point in general). Specifically, I observe that the effect of a tax hike on wages increases in this sample while tax cuts boost average wages (0.53 percentage point) albeit not significantly.

In the absence of a combined reporting rule, a parent corporation could shift profits to a subsidiary in a state without such requirement, to escape the tax hike. This shifting occurs in a variety of ways such as within-firm sales or lending (Mintz & Smart 2001). The results in Table 2.3 suggest that corporations might engage such practices. The evidence here indicates that profit-shifting prevails since no disemployment effect, reflective of a dislocation of production is observed.

The wage reduction could be explained in a bargaining set-up, where fewer profits in an area would translate into fewer wages for employees. Unfortunately, due to the data structure, this study cannot observe wages paid to full time and part time workers at the county level. It would

have been instructive to compare the tax impacts on these two groups to further investigate the validity of that hypothesis.

Table 2.3: Incidence of tax changes (No combined reporting)

	growth relative to Contiguous	
	Employment	**Wage**
	Panel 1	
Tax change (%)	-0.07	-0.43***
	(0.05)	(0.05)
	Panel 2	
Tax hike (%)	-0.13	-0.28***
	(0.10)	(0.07)
Tax cut (%)	0.06	0.53
	(0.10)	(0.41)

** significant at 10%; ** significant at 5%; *** significant at 1%*
Growth measured as a relative change, N=955

Next, I restrict the analysis to the sample of border segments with a differential combined reporting ruling on either side of the border. I focus on border segments where the change occurs in the state with a combined reporting. As reported in Table 2.4, I notice that the employment growth effect of a tax hike significantly increases (-0.23 percentage point vs -0.17 percentage point) suggesting that businesses might engage in some forms of activity shifting across borders to minimize tax liabilities.

In the face of a tax hike involving a state with a combined reporting, businesses might have an incentive to transfer activities to the side of the border with no such requirement to maximize profits. The absence of a significant wage response to a tax hike in this sub-sample, coupled with an employment decrease reinforces the activity shifting hypothesis.

Table 2.4: Incidence of tax changes (Different combined reporting)

	growth relative to contiguous	
	Employment	**Wage**
	Panel 1	
Tax change (%)	-0.06**	-0.005
	(0.03)	(0.05)
	Panel 2	
Tax hike (%)	-0.23***	-0.008
	(0.06)	(0.08)
Tax cut (%)	0.01	-0.01
	(0.03)	(0.06)

** significant at 10%; ** significant at 5%; *** significant at 1%*
Growth measured as a relative change, N=669

Last, I consider the incidence along border segments with combined reporting laws on both sides. As evidenced in Table 2.5, I do not observe a substantial effect of the corporate tax change on employment and wages, since the existence of such rulings limits the ability to shift profits and escape the tax in the short-run. The behavioral response, in this case, is likely to be observed in the long-run when factor reallocations take place. The dual mobility of labor and capital across states implies that the wage effects would not be as important. I should highlight that the limited size of the sample of contiguous states with a similar treatment of consolidated profits, considerably limits any inference given that the standard errors are clustered at the state level.

Table 2.5: Incidence of tax changes (with combined reporting)

	growth of relative to contiguous	
	Employment	**Wage**
	Panel 1	
Tax change (%)	-0.15	-0.18
	(0.12)	(0.37)
	Panel 2	
Tax hike (%)	-0.24	-0.25
	(0.67)	(0.77)
Tax cut (%)	0.15	0.18
	(0.12)	(0.37)

** significant at 10%; ** significant at 5%; *** significant at 1%*
growth measured relative to contiguous counties, N=255

2.6.3 The role of market power

Considering that the results presented above found significant impacts of tax hikes on wages, this paper analyzes how the existence of market concentration affects this effect. When controlling for the degree of market competition proxied in this study by the number of establishments, I observe that the wage response to a tax hike is sensitive to the degree of market concentration as suggested by the general equilibrium model of Davidson & Martin (1985). The results described in Table 2.6 indicate that the marginal effect of the corporate tax on wages varies substantially at different quartiles of market competition. Overall, the paper notices that a one percent increase in the state corporate tax reduces the growth of average wages paid to employees by -0.16 percentage point at the mean of the distribution of the number of establishments.

Table 2.6: Incidence of tax changes (with market competition)

	growth of relative to cont.	
	Employment	**Wage**
Hike (%)	-0.16***	-0.14**
	(0.06)	(0.07)
Hike*Firms	-0.0005	0.0018*
	(0.0008)	(0.001)

	1st Quart	Median	Avg.	3rd Quart
Nber of Firms (hundreds)	15	24	42	45
Marginal Effect (Wages)	-0.17	-0.13	-0.16	-0.10

** significant at 10%; ** significant at 5%; *** significant at 1%*
Number of establishments measured in hundreds, N=1585

This marginal effect intensifies with the degree of concentration and reaches a peak of -0.17 percentage point at the first quartile of the market competition metric. These findings corroborate the bargaining power story, as the presence of more firms in a locality limits the bargaining power and the extent of collusive behavior by firms. Thus, it impedes their ability to pass the cost of a tax hike onto workers. Of interests are the marginal effects at the first and third quartiles of the distribution of the number of establishments at the county level.

These results suggest that for a county with the number of establishments equivalent to the median of the distribution, the growth of labor compensation reduces by 0.16 percentage point for a one percentage point increase in the corporate tax rate in the short-run. However, medium and long-term effects would likely be different as migration flows, and other business decisions respond to the tax.

2.7 Robustness checks

This section presents several checks to highlight the robustness of the measured tax effects. I investigate the validity of the assumptions made with regards to the exogenous nature of tax changes from the perspective of a county, the parallel trend between contiguous counties, and other econometric pitfalls inherent to the methodology of this analysis.

2.7.1 Do counties react to state tax changes?

The empirical strategy of this paper compares contiguous counties around exogenous corporate tax reforms. If treated counties react to state policy changes by altering local policy instruments that influence business activities and the demand for labor, this methodology will likely underestimate the tax impact on wages and employment. I investigate this possibility by regressing the growth rate of county public property and sales tax collections, expenditures, and revenue on current and previous tax changes. The results summarized in Table 2.7 suggest that such local government interventions may have occurred given that all these local public finance aggregates decreased during years of state tax reforms. Counties may be lowering tax rates to offset the adverse effect of the tax on their economies. As a result, the tax impacts measured in this analysis might just provide a lower bound of the true response to corporate tax changes.

Table 2.7: Corporate tax change and county public finance

	δ(Protax)	**δ(Saltax)**	**δ(Expend)**	**δ(Revenue)**
δ(effcit)	-0.037***	-0.06***	-0.079***	-0.079***
	(-0.01)	(-0.02)	(-0.02)	(-0.02)
l.δ(effcit)	0.004	0.05	0.06**	0.05*
	(0.03)	(0.05)	(0.03)	(0.03)
l2.δ(effcit)	0.04	0.04	0.03	0.04
	(0.03)	(0.03)	(0.03)	(0.03)
l3.δ(effcit)	0.005	0.005	0.02	0.01
	(0.04)	(0.06)	(0.03)	(0.03)

** significant at 10%; ** significant at 5%; *** significant at 1%*
Number of establishments measured in hundreds, N=1585

2.7.2 Results with closer contiguous pairs

The spatial discontinuity approach used in this paper implies that contiguous counties share similar economic conditions in the long-run. This draws on the importance of proximity which favors the development of integrated local economies. I explore the robustness of that hypothesis by analyzing the incidence in a sample of much closer contiguous counties. I exploit geographic coordinates of county centers to calculate distances between contiguous counties. The results exposed in Table 2.8 focus on adjacent county pairs located within a hundred miles of each other. The tax effects on this sample are barely different from the initial impacts, suggesting that the homogeneity of contiguous counties is not necessarily driven by their geographic distance.

Table 2.8: Tax effects with closer counties (less than 100 miles)

	growth of relative to contiguous	
	Employment	**Wage**
	Panel 1	
Tax change (%)	-0.06***	-0.18*
	(0.02)	(0.10)
	Panel 2	
Tax hike (%)	-0.17***	-0.14*
	(0.06)	(0.08)
Tax cut (%)	-0.002	0.01
	(0.02)	(0.06)

** significant at 10%; ** significant at 5%; *** significant at 1%*
Number of establishments measured in hundreds, N=1585

2.7.3 Exclusion of big economic counties

The treatment of state corporate tax reforms as exogenous policy changes to counties located along borderlines would be questionable if contiguous counties are economically large enough to influence policy changes at the state level. In such a scenario, corporate tax reforms cannot be treated as exogenous events from the perspective of a contiguous county. To test this possibility, this paper excludes contiguous counties that represent more than 10% of a state's personal income. The results of this set of regressions presented in Table 2.9 do not suggest any significant changes in our estimates.

Table 2.9: Tax effects without large economic counties

	growth of relative to Contiguous.	
	Employment	Wage
	Panel 1	
Tax change (%)	-0.05***	-0.16*
	(0.02)	(0.09)
	Panel 2	
Tax hike (%)	-0.17***	-0.15*
	(0.06)	(0.08)
Tax cut (%)	-0.006	0.02
	(0.02)	(0.06)

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

2.7.4 Presence of spatial correlations

One critical limitation of using contiguous counties to study the impact of state policy changes on employment and wages relates to the potential bias inherent to cross-border spillovers. This type of spatial interactions is concerning in studies that measure long-run effects. However, it is also useful to investigate the presence of spatial correlation in empirical works that focus on short-run impacts.

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 these estimates will be biased. I study this possibility by regressing the growth rates of employment, wages and income in a county on current and previous corporate tax rates in the contiguous county.

The results in Table 2.10 do no highlight any significant spatial cross-border spillovers on employment and wage. Even though county personal income which proxies the gross county product features a potential response to past corporate policy changes on the other side of the border, the employment and wage results do not warrant any spatial interactions that would possibly undermine the consistency of our estimates.

Table 2.10: Spillovers of tax changes on contiguous counties

	δ(lcemp)	**δ(lcwage)**	**δ(lcpinc)**
effcit	-1.09	0.29	0.33
	(0.97)	(0.30)	(0.22)
l.effcit	0.76	-1.35	-1.06**
	(2.04)	(1.41)	(0.5)
l2.effcit	0.96	2.02	1.37***
	(1.53)	(2.00)	(0.32)
l3.effcit	-0.9	-1.00	-0.63***
	(1.14)	(0.76)	(0.27)

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

2.7.5 Simultaneity of CIT changes with other state changes

State corporate tax changes are not random and could be related to other state policies. Even though this paper captures state-level heterogeneity through the average growth of interior contiguous counties, it is useful to study the endogeneity between corporate tax reforms and other policies. I regress corporate tax changes on changes in personal income tax, sales tax and investment tax credits at the state level. The results in Table 2.11 suggest that CIT reforms are

correlated with personal income tax and sales tax changes in subsequent years. This finding reinforces the approach used in this paper to control for state-level heterogeneity. Otherwise, the observed difference in growth between contiguous counties around state corporate changes would be contaminated by other state policies. By including the average growth of interior contiguous counties, I capture other policy changes related to the personal income and sales taxes that might be correlated with both corporate reforms and labor market outcomes.

Table 2.11: Relationships between state policy changes

	δ (effcit)	**l.δ (effcit)**	**l2.δ (effcit)**
δ(itr)	0.0004	0.0003***	0.00002
	(-0.0003)	(-0.0001)	(-0.0001)
δ(str)	0.0006	0.001***	0.0003
	(0.0004)	(0.0004)	(0.0003)
δ(itc)	-0.0006	0.01	-0.00009
	(0.007)	(0.01)	(0.005)
Obs.	1582	1582	1582
R-squared	0.004	0.009	0.004

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

2.7.6 Results with economically similar counties

An alternative way of checking the robustness of my results, is to restrict the analysis to contiguous counties that are alike based on observable economic characteristics. I compare the growth of employment, personal income and population in contiguous counties during years preceding a corporate tax change. I construct a "similarity" index that aggregates the absolute difference in the growth of employment, population and personal income between a pair of contiguous counties prior to a state tax reform. I later exclude the pairs of counties for which the index is above the top decile of the similarity distribution. The results in table 2.12 suggest that restricting the analysis to counties that are similar with regards to observable economic characteristics, does not alter the main tax effects.

Table 2.12: Tax effects with economically similar counties

	growth of relative to Contiguous.	
	Employment	Wage
	Panel 1	
Tax change (%)	-0.04**	-0.18*
	(0.02)	(0.10)
	Panel 2	
Tax hike (%)	-0.15***	-0.14*
	(0.06)	(0.08)
Tax cut (%)	0.005	0.005
	(0.02)	(0.06)

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

2.8 Limitations and perspective for future works

2.8.1 Limitations

In this study, I exploit county-level data on employment and wages over the period 1969-2014 with significant gaps in some states. However, I could not compile a dataset on employment and wages by organizational form and employee types over the same period. This would have been ideal considering the corporate tax usually applies to big firms and bargaining power differs by employment status (part-time vs. full-time). The incidence measured in this paper could understate the tax effect and only the use of data by legal form of organization or type of workers would settle this issue.

Also, I only estimate short-term effects which barely account for the reallocation of factors across different sectors. By comparing average growth rates over a period of three years, this paper assesses the change in employment and wages 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.

Future works aiming to expand the empirical literature on the corporate tax effects on wages and employment in the presence of market concentration should be directed at estimating long-run estimates by gathering substantial time series data on the activities of firms over time. This type of study will be much more informative for the overall debate on the reform of the federal corporate tax code.

Measuring the degree of market power in the context of an empirical investigation has always been a remarkable challenge. This paper draws on the number of establishments in a county to proxy for the degree of market competition. An interesting path for future research on this topic relates to the use of alternative concentration metrics.

2.8.2 Implications for corporate tax policy

This paper exploits changes in corporate taxation across U.S. states to study the extent to which the degree of market competition and state corporate financial reporting rules alters the way the tax is passed onto workers in the form of lower wages. Even though the paper focuses exclusively on short-run effects, these estimates provide a valuable empirical insight regarding the importance of market power in the incidence of the corporate tax.

The investigation carried out in this paper contributes to the debate on the reform of the U.S. federal corporate tax system in multiple ways. Considering that the national conversation around the federal corporate tax code has been framed around the opportunity of a corporate tax cut, the evidence exposed here does not highlight any benefits for wages and employment in the short-run. State corporate tax cuts do not significantly boost employment and wage growth in the

short-run. This observation may not necessarily carry over the long-run since factor reallocations across sectors will likely affect labor outcomes.

When controlling for market competition the analysis notices that the wage sensitivity to a tax hike shrinks with the number of establishments in a county. The numerous provisions already existing in the current federal corporate tax code should be revised in function of the incentives they provide to firms operating in imperfectly competitive sectors.

Also, this analysis offers a slight glimpse into the incidence of a tax reform when multinational firms could use profit-shifting practices to minimize tax payments. I found that the absence of a combined reporting agreement at the state level strengthens the wage sensitivity of the tax suggesting that businesses might shift profits across jurisdictions. If these behavioral considerations carry to the federal level, a tax cut might not actually affect average wages and employment in the short-run, and the forgone public tax revenue will not be offset by more economic activities or a higher pay to workers.

Even though, from the perspective of a multinational corporation, maximizing profits in the U.S might become more attractive, my results indicate that these benefits may not be passed onto workers in the form of higher wages, but could simply reflect a transfer of profits made overseas to U.S jurisdictions. I am also aware of the fact that the challenges at stake in a federal tax reform are much broader than the implications state corporate tax changes may indicate. Although tax competition across U.S. states could be compared to international competition between jurisdictions to attract multinational firms, the difference in terms of scope and depth of the federal tax code limits any immediate extrapolations.

2.9 Conclusion

In this paper, I study how the presence of market power and state corporate tax accounting rules affect the responsiveness of wages and employment to changes in corporate tax rates. I measure the degree of market competition by the number establishments and use legislation on state combined reporting agreement to proxy differences in corporate income shifting incentives across states.

I found that corporate tax cuts do not have a significant effect on employment and wages, while corporate tax hikes reduce the growth of both wages and employment in the short-run. This result is consistent with a series of papers (Ljungqvist and Smolyansky 2014, Fuest et al. 2017) which also noticed this asymmetric effect. My findings indicate that a one percentage point corporate tax hike reduces average wage growth by 0.17 percentage point and employment growth by 0.05 percentage point. Unlike Ljungqvist and Smolyansky 2014, who noticed that corporate tax cuts are only effective when implemented during recessions, I contend that the asymmetry of the tax effect could be explained by the difference in bargaining power between capital and labor.

As the tax increases, corporations are more likely to pass the burden onto workers in a classic wage setting through bargaining. Alternatively, they would also retain a significant share of the benefits generated by tax cuts, implying no sizable wage or employment gains. This supports the belief that market power influences the wage sensitivity of the corporate tax. To explore this possibility, I controlled for the degree of local concentration through the number of establishments in a locality and observed that the wage sensitivity to the tax tends to decrease with more market competition ranging from -0.17 percentage point at the lowest quartile to -0.10 percentage point at the third quartile of the concentration measure. This result also provides evidence in support of the predictions of the general equilibrium model of Davidson and Martin (1985).

Another contribution of this study relates to the importance of state legislation on combined reporting in the analysis of the corporate tax. When businesses are required to report combined profits in their tax filings, it is found that the employment sensitivity to a corporate tax hike amplifies and labor demand shrinks even more. This is consistent with a production shifting hypothesis. As the presence of combined reporting limits the ability of firms to shift profits across jurisdictions to cushion the losses entailed by a corporate rate increase, this result implies that businesses are more prone to moving production to other states in this case. This finding also lends support to results found by other papers (Mintz and smart 2004, Grubert and Slemrod 1998, Gordon et al. 1995) which emphasized that profit-shifting incentives affect corporate investment and employment.

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 primarily on measuring long-run impacts in the presence of market power. One vital constraint with regards to this challenge remains the length of the time series data required for such an analysis. As longer data structures emerge, we should be able to draw a more accurate picture of the importance of market imperfection in the analysis of the corporate tax incidence. Second, future work on this subject should examine the opportunity of expanding the combined reporting agreement to other states and countries if possible through multilateral arrangements.

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CHAPTER 3: TAX REFORM, WAGES, AND EMPLOYMENT: EVIDENCE FROM OHIO

3.1. Introduction

Over the last few decades, the integration of the world economy enabled an unprecedented deepening of trade and international financial flows. One direct implication of this expansion of globalization has been the rise of the bargaining power of capital owners at the expense of public governments. To reduce tax inefficiencies while remaining competitive on the global stage, many jurisdictions lower their tax rates (Tanzi 1995). This paper observes individual records of the Current Population Survey to study the impact of the 2005 Ohio tax reform on labor market outcomes. The reform significantly reduced the corporate tax, the personal income tax and the property tax on machinery investment over the period 2006-2010. I found that the change did not significantly boost individual wages, employment opportunities or participation in the labor force, but seems to have affected the reported self-employment earnings. This result is consistent with income shifting behavior implying that self-employment taxable income responds to marginal tax rates. These findings are robust to several specifications, one of which controls for pre-existing heterogenous patterns in earnings and employment growth across states.

Like national tax jurisdictions, local public governments engaged in a race to attract private firms. In the U.S., this competition features a decrease in corporate and personal income tax rates as well as the adoption of investment tax credits and other instruments to reduce production costs to businesses. Average state corporate income tax rates increased from 3.7 percent in 1960 to a high of 7.0 percent in 1992 and have since fallen to 6.2 percent in 2014, the lowest it has been since 1981. The average state personal income tax rate increased from 5.3 percent in 1970 to a maximum of 6.2 percent in the mid-1980s before hitting an all-time low of 5.1 percent in 2010.

Additionally, many U.S. states introduce several incentives to promote a more attractive business environment. The average state investment tax credits rate rose from 0.2 percent in 1970 to 2.0 percent by 2010.

One of the most recent comprehensive state tax reforms designed to spur growth and employment was adopted by the state of Ohio in 2005. The magnitudes of the cuts are staggering relative to other state tax changes. The reform eliminated property taxes on capital investment and machinery and abolished taxes on the sales of goods and services to customers outside of Ohio. It also set up a 21 percent reduction in personal income taxes and replaced the pre-existing Corporate Franchise Tax (CFT) of 8.6 percent by a Commercial Activity Tax (CAT) of 0.26 percent. These changes were gradually phased-in over the period 2006-2010.

There is an abundance of empirical studies that have explored the incidence of tax reforms on economic activity and labor market outcomes. Most of this literature focus on specific taxes such as the payroll or the personal income tax (Gruber 1997, Cruces et al. 2009). Few papers have looked at the incidence of extensive reforms that involve several simultaneously implemented tax changes. These works generally consider national tax reforms like the U.S Federal Tax Reform Act of 1986 and use general equilibrium models (Altig et al. 2001, Auerbach & Kotlikoff 1987, Fullerton et al. 1993) to simulate the incidence of several tax schemes on wages and employment. Alternatively, other scholars measure this incidence with econometric models (Kubik 2006).

Unfortunately, no such empirical work has been done at the U.S. state level. Unlike federal taxes, state tax systems which are more likely to be influenced by political or business cycles, are overhauled regularly. This offers a unique opportunity to investigate the benefits of such policies in the short-run considering the forgone revenue for state governments. One should expect the

incidence to differ from single tax changes, due to the interrelationships between several aspects of the tax code.

This could also serve as a valuable benchmark for the incidence of the newly adopted "Tax Cuts and Jobs Act" of 2017. Even though the rate changes differ in magnitude, the reform in Ohio is comparable to the new federal tax code along one dimension. Both policies reduced the corporate and personal income tax rates simultaneously. However, the "Tax Cuts and Jobs Act" adopted a provision that features a different treatment for income earned by pass-through entities like self-employed individuals.

This paper studies the impact of the 2005 Ohio tax reform. Specifically, I exploit the Public Use Microdata Survey from the Current Population Survey (CPS) to explore the effects of these substantial rate cuts on the Ohio workforce. I observe separate cross-sections of individuals across all U.S states over the period 2000-2015. The identification strategy is akin to a difference in difference approach which compares wages, employment status and labor force participation for individuals living in Ohio to similar individuals in (i) the Midwestern region, and (ii) the U.S. before and after the reform. I consider several specifications, one of which controls for pre-existing economic conditions in the state of Ohio, and capture selection into the labor force with a Heckman type methodology.

My findings suggest that the tax reform did not significantly boost individual wages or employment but seems to have impacted self-employment earnings. This result is consistent with a literature (Clotfelter 1983, James Long 1982, Richard Goode 1976) that underscores a negative relationship between marginal tax rates and taxable income when tax filings involve some degree of compliance as is the case for self-employed individuals. This group of economic agents is more

sensitive to tax rates because income earned, and deductions are partly determined by voluntary cooperation.

The rest of the paper is organized as follows. Section 2 describes the policy context with a detailed exposition of the tax reform. Section 3 briefly reviews the relevant literature, with a focus on the impact of comprehensive tax reforms. 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 several alternative specifications. Section 8 presents the limitations and explores the avenues for future research on the incidence of comprehensive tax reforms.

3.2. Policy context: Tax reforms in Ohio over 2005-2009

3.2.1 Phase-in of the Commercial Activity Tax (CAT)

The Corporate Franchise Tax (CFT) which is imposed on the activities of corporations operating in Ohio before the 2005 reform represents a flat rate of 8.6 percent applicable to the profits generated within the state. Beginning in 2006, the Corporate Franchise Tax (CFT) was progressively phased out up until 2010, when it was completely replaced by a Commercial Activity Tax (CAT) of 0.26 percent.

Apart from the differential in rates, the new tax features an important distinction with regards to the taxable base. It is levied on all businesses irrespective of their legal form of organization and applies to gross receipts as opposed to the CFT which targets profits. During the phase-out period, corporations respectively paid 80 percent in 2006, 60 percent in 2007, 40 percent in 2008 and 20 percent in 2009, of the pre-existing corporate franchise tax due. Additionally, the new CAT rate has been gradually phased-in from an initial rate of 5.6 percent in 2006 to its final

full rate of 0.26 percent by 2010. The corporate tax cut is designed to reduce the cost of capital for entrepreneurs and unleash capital investment and productivity.

The literature on corporate tax incidence has long defended that the impacts of business tax reform on wages in the long-run depend on (1) the degree of capital mobility across sectors, (2) the elasticity of substitution between capital and labor in production, (3) the elasticity of substitution between products in consumption, (4) the factor intensities in production and (5) the elasticity of demand with regards to the corporate output (Harberger 1962, Martin & Davidson 1985, Raveendra 1975, and Gravelle 2006). Assumptions regarding these parameters determine the distribution of the corporate tax burden between workers, capital owners, and consumers.

In the short-run, the tax effects are primarily driven by market imperfections (Felix et al. 2006, Arulampalam 2010 and Liu & Altshuler 2013). This builds on a rather heterodox tradition which emphasizes the role played by labor and product market institutions in the analysis of tax incidence. It is important to note that these studies do not measure the classical general equilibrium effects of the corporate tax on wages through investment. Instead they seek to evaluate the effect of the tax inherent to differences in bargaining power between workers and firm owners, or the presence of market power that enable the shifting of the tax to consumers and workers.

Also, a few papers have suggested that the incidence of corporate tax changes might be asymmetric (Ljungqvist 2014, Kakpo 2017) and tax cuts do not necessarily boost employment and wages in the short-run. However, given that the magnitude of the CFT reduction in Ohio is substantially higher than previous rate cuts, the anticipated incidence is uncertain at best.

3.2.2 Phase-out of the property tax and investment credits on equipment and machinery

Another significant change that was introduced in the Ohio tax code through the 2005 tax reform relates to the removal of the state property tax on equipment and machinery. Under the new tax code, manufacturing machinery and equipment are exempt from property taxation beginning in 2006. The property tax on existing machinery and equipment, furniture and fixtures, and inventory is phased out starting in the tax year 2008 and ending with no tax due in 2009. This policy is designed to induce more investments specifically in manufacturing. However, the state also eliminated tax credits on machinery and equipment investments in 2009.

The few empirical works that have explored the incidence of investment subsidies on growth and employment did not observe any substantial effects (Gravelle & Hungerford 2010). Other works studied the incidence of tax-induced changes in the user cost of capital on investments. The user cost of capital reflects the present discounted value of the marginal cost of a unit of investment. It accounts for the future stream of net returns associated with an asset including interest payments, property taxes, and credits on investment expenditures. Policy changes with regards to investment tax credits and the property tax on productive assets affect the user cost of capital. Chirinko & Wilson 2008 noted that corporate tax-induced changes in the user cost of capital affects state investment dynamics. The combined effect of these two offsetting changes on wages and employment will depend on the response elasticities of capital formation with respect to both instruments.

3.2.3 Reduction of the personal income tax

On top of the changes highlighted above, the Ohio legislature introduced a reduction by 21 percent of the personal income tax rate over the period 2006-2010. The initial rate of 7.5 percent is scheduled to be reduced in five annual increments (of 0.26 percent each), resulting in a tax rate

of 6.2 percent by 2010. The personal income tax reduction targets to induce higher household savings, incite labor participation, and promote economic growth.

There is a wealth of research (Eissa 2005, Gordon et al. 2007, Henrekson et al. 2007) on the incidence of personal income taxation on labor supply decisions. These papers observe labor market outcomes for individuals around policy reforms affecting the marginal income tax rate. The empirical consensus defends that marginal tax rates on personal income affect the supply of labor specially for individuals with a low attachment to the labor market such as married women. The behavioral response to the personal income tax could be investigated across several dimensions such as hours worked, wages, participation in the labor force or employment. I choose to explore the incidence of the Ohio tax reform on all these aspects except hours worked due to the poor quality of the data on this variable.

3.3. Relevant literature

The literature on the economic effects of tax reforms could be divided into two main categories. Applied general equilibrium models adopt a theoretical framework where the incidence of alternative tax schemes is studied, and the most efficient structure is discussed (Altig et al. 2001, Auerbach & Kotlikoff 1987, Fullerton et al. 1993, Auerbach 2002, and Nickell 2003). The critical challenge here relates to the specification of several elasticity parameters for calibration.

In contrast, empirical works exploit reduced-form models to investigate the impacts of tax changes on employment, wages, and investment. Using various identification approaches to tackle the endogeneity between tax changes and economic trends, these papers analyze variations in wages or employment around tax reforms (Gruber 1997, G. Cruces et al. 2009 and Jeffrey Kubik 2006). Many of these works consider specific reforms targeted at the taxation of labor or capital earnings, with a focus on national or federal reforms.

The methodological challenge relates to the endogeneity of policy reforms. Tax changes could be motivated by pre-existing economic conditions that also affect current dynamics of wages and employment. To circumvent that shortcoming, scholars use identification strategies ranging from the "Narrative approach[9]" (Romer & Romer 1989, 2004, and Ramey & Shapiro 1998) to the "Structural Vector Regression method" (Blanchard & Perotti 2002). The main goal is to isolate the components of policy changes that vary exogenously from economic outcomes by exploiting political speech materials or the institutional design of tax collections. But recovering archival qualitative data is a daunting task[10] especially at the regional level and adopting the Structural Vector Approach requires the use of high-frequency time series on public finance aggregates that are not available for most states. For these considerations, this paper will not adopt such identification strategies.

Other studies observe panels of individuals or firms and treat national tax reforms as exogenous policy changes from the perspective of these units (Gruber 1997, Cruces et al. 2009 and Kubik 2006). These papers compare variations in labor earnings and employment status of individual workers around tax reforms. The difference in difference approach that is usually adopted, requires the choice of an appropriate control group which would be observed over the same period to capture counterfactual developments. Gruber (1997) compare Chilean manufacturing firms around the dramatic payroll tax change of 1981 while Cruces et al. (2009) studied the relationship among payroll taxes, wages and employment inherent to policy-driven geographical variations within Argentina over the period 1995-2001.

[9] The Narrative Approach exploits speech materials by policymakers to identify exogenous policy reforms.
[10] This would require collecting speech materials from state policymakers prior to the reform.

The critical challenge for this methodology is inherent to the choice of a control group. The existence of potential heterogeneous trends in wages and employment across individuals or firms warrants the adoption of a methodology that captures pre-existing dynamics across the observed units. The empirical literature generally includes individual or state time-invariant effects, which only captures differences in average across regions and individuals but fails to account for previous trends. I address these methodological shortcomings by including state-specific time trends to capture regional time variant heterogeneity.

Plus, most papers study national policy reforms and very few works examine comprehensive tax reforms. Even when they do, these papers focus on specific taxes within the overall reform. The most notable case relates to Jeffrey Kubik (2006) who studied the U.S Federal Tax Reform Act (TRA) of 1986 to examine the short-run effects of the change of labor taxation on the wage structure of several skills. The author utilizes the differential impact of the TRA on median marginal tax rates across several skills to analyze the incidence of the reform on labor supply. This approach does not capture the interdependency between several aspects of the tax code. The measured impacts might just reflect the general equilibrium incidence of the overall tax reform. Other changes in the TRA might have affected the behavior of the control individuals which would bias the estimated tax effect.

3.4. Data

This paper studies the impact of the 2005 Ohio tax reform on labor market outcomes in Ohio by observing individuals between the ages 25 and 64 over the period 2000-2015. I pooled several annual cross-sections obtained from IPUMS-CPS, an integrated dataset of the randomly-sampled March Current Population Survey (CPS). The CPS data provide information on employment status, wages, industry, sociodemographic attributes, occupation affiliation and

additional characteristics of the employer such as the size of the firm or incorporation status. The overall sample contains 1,667,068 individuals.

Dependent variables: My primary outcome variables are wages, self-employment earnings and employment status. The survey measures wages as the total nominal pre-tax wage and salary income that is, money received as an employee over a calendar year. I use consumer price index adjustment factors to convert this information into real employee compensation. The survey also indicates for every working-age individual whether the person was in the labor force and, if so, whether they were currently unemployed. I use this information to construct a dummy variable for a person's employment status.

Individual characteristics: Given that the dataset does not provide the number of years of schooling. I impute this information from the nine categories of educational attainment used by the CPS. Following the tradition established by Mincer (1974), I compute work experience (exper) as the difference between age and the years of schooling minus six ($exper_i = age_i - s_i - 6$). This approach features some limitations but represents a good approximation of experience if one considers groups of individuals like middle-aged males with a strong attachment to the labor market.

Sociodemographic characteristics include marital status, age, number of children younger than 5 years old, number of children between the ages of 6 and 20, gender and race. Other economic variables used in this analysis are the employment status, the occupation category of employment, annual wages, nonwork income as well as the metropolitan status of the individual's residence. Table 3.1.3 in the Appendix describes some summary statistics of the sample used in this analysis.

3.5 Methodology

3.5.1 Samples of interest

The objective of tax reform is to boost economic activity in the state of Ohio. The policy change impacts labor supply decisions by individual workers and the demand for labor by firms. I focus on individuals between the ages 25 and 64. These are prime age workers with a strong attachment to the labor market. I excluded students and those working in the armed forces and consider several sub-samples based on the characteristics of the employer.

As a robustness check, I also observe changes in employment and wages paid by large (between 50 and 250 workers) firms around the tax reform given that the reform is expected to affect businesses differently depending on their legal form of organization. I also observe self-employed individuals who might elect to report their earnings as pass-through entities or businesses depending on the net advantages of each alternative. By altering the treatment of different types of income (business vs. labor income) the tax reform increases the incentives to shift earnings across alternative sources of income to minimize tax liabilities. I explore this possibility by analyzing reported income by self-employed individuals before and after the reform in Ohio relative to other states.

3.5.2 Identification strategy

The primary goal of this analysis is to study the impact of these policy reforms on individual earnings and employment in Ohio. As a result, it is important to control for the different confounding factors affecting labor market outcomes at the state and the national level. This is even more important considering that the policy reform covers a period which features other

remarkable shocks not only in the state of Ohio (other policy changes) but also at the national level (Great Recession).

I address these considerations by using alternative specifications that impose different dynamics in employment and wages across states. This paper considers two main control groups. First, I compare individuals in Ohio to similar peers in the midwestern states, the majority of which are neighbors of Ohio. These contiguous states are expected to provide better controls for dynamic changes in employment, wages, and labor force participation.

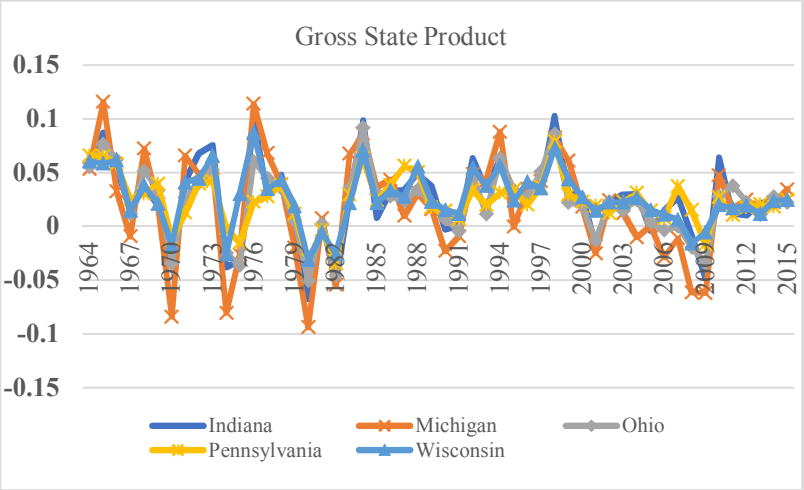

Figure 3.1: Growth of output in Midwestern states

Figure 3.1 plots the growth rate of output in Ohio relative to its neighboring states. The dynamics of the Gross State Product (GSP) are relatively similar across these midwestern states[11]. Considering that most of these states are part of the "U.S Manufacturing Belt", this graph suggests that they feature similar trends in economic activities. This also supports the belief that adverse sectoral shocks that affect a given state might translate into lower output growth in neighboring states inducing a spatial association in output trends. I take that into consideration by including

[11] The partial correlation coefficients between these series belong to the interval [0.78 -0.89]

time-varying regional trends in economic outcomes which capture unobserved developments in regional economic conditions that might be correlated to labor market outcomes.

The second control group considers all other individuals in the U.S. with socioeconomic characteristics similar to the ones observed in Ohio. This group of individuals represents an alternative set of counterfactual individuals. Since geographic neighbors might be contaminated by spatial spillovers, observing all other U.S. states can be useful. Difference-in-differences specifications assume that the treated and control region have the same trends in the absence of the policy (parallel trends assumption) and will generally fail to produce consistent treatment effect estimates if this assumption is not true. Table 3.1 describes characteristics of individuals between the ages 25 and 64 living in Ohio relative to the same type of individuals in each of the control geographies prior to the tax reform. This comparison reveals important similarities regarding socioeconomic characteristics, except for the percentage of people with a salaried employment which appears higher in Ohio and the Midwestern states.

Table 3.1: Characteristics of sub-samples over the period 2000-2005

	Ohio		Other Midwest(*)		All other U.S.	
	Mean	S.D.	Mean	S.D.	Mean	S.D.
Age	43.05	10.34	42.67	10.28	42.98	10.34
Male (%)	0.47	0.50	0.48	0.50	0.49	0.50
White (%)	0.84	0.36	0.82	0.38	0.82	0.38
Married (%)	0.68	0.47	0.68	0.47	0.67	0.47
Years of schooling	13.44	2.46	13.38	2.73	13.33	2.92
In the labor force (%)	0.81	0.39	0.80	0.40	0.82	0.39
Employed (%)	0.95	0.21	0.95	0.21	0.96	0.19
Self-employed (%)	0.09	0.29	0.10	0.29	0.13	0.33
Living in a metropolitan area (%)	0.87	0.33	0.84	0.36	0.75	0.43
With a salary employment (%)	0.72	0.45	0.73	0.44	0.68	0.46
Number of children	1.17	1.24	1.23	1.27	1.14	1.22
Number of children less than 5 y.o.	0.22	0.54	0.24	0.56	0.21	0.52
Wage earnings ($ 1000)	39.16	39.82	41.54	44.76	39.69	43.61
Number of Obs.	21,732		58,281		688,622	

(*) Other Midwestern states include Michigan, Pennsylvania, Indiana, Wisconsin.

It is important to be cautious regarding the parallel trend assumption given that the state of Ohio experienced dismal economic growth relative to its neighbors in the years leading up to the reform[12] and may have been slightly more impacted by the 2008 Great recession due to the relative importance of manufacturing in its economic structure[13]. I consider this possibility through a specification with a time-variant heterogeneity at the state level. More precisely, I include state-specific linear time trends that capture dynamic developments on state labor markets over time. I later include cubic and quadratic time terms at the state level to better approximate unobserved shocks that affect states.

[12] Over the period 2005-2006, the average growth of output in Ohio is 0.02% vs 1.62% for the Great Lakes.
[13] Over 1998-2007 manufacturing represents 22% of private GDP in Ohio vs. 20% in other Great Lakes

3.6. Empirical strategy

3.6.1 Selection bias

Approximately one-fifth of the observations in my sample have missing wages because the individual is not in the labor force and is not working as a result, presenting a typical sample selection problem. The sample of working individuals might not be random, and the unobserved factors that determine wages are likely to be correlated with the unobservables that influence a person's decision to supply labor. It is well known that a simple OLS regression on the sample of individuals employed would yield biased and inconsistent estimates for the wage regressions.

To correct for the sample selection bias, I adopt a Heckman-type methodology and add a first-stage selection equation. Participation in the labor force and employment for individual *i* over period *t* (emp_{it}) depends on the individual nonwork income ($nwinc_{it}$), education ($educ_{it}$), marital status ($married_{it}$), number of children younger than 5 ($child5_{it}$), age of youngest child ($yngch_{it}$), and an error term (u_{it}).

$$emp_{it} = \begin{cases} 1 \text{ if } \delta_1 nwinc_{it} + \delta_2 educ_{it} + \delta_3 child5_{it} + \delta_4 yngch_{it} + \delta_3 married_{it} + \delta_4 female_{it} + u_{it} > 0 \\ 0 \text{ otherwise} \end{cases} \quad (3.1)$$

This first-stage is identified by the variables "nonwork income" and "number of children younger than 5 years old" which are expected to affect incentives related to labor force attachment without altering potential wage earnings across individuals. In other words, this variable is excluded from the second-stage specification but helps induce some variation in the probability of being employed according to equation (3.1). I also explore the incidence of the policy reform on an individual's participation in the labor force. The policy reform represents an exogenous shock from the perspective of an individual and the period dummies help to identify the tax effects.

$$\text{emp}_{it} = \begin{cases} 1 \text{ if } \delta Z_{it} + \theta_0 \text{dohio}_{it} + \theta_1 \text{d0610}_{it} + \theta_2 \text{d2011}_{it} + \theta_3 \text{dohio.d0610}_{it} + \theta_4 \text{dohio.d2011}_{it} + \text{u}_{it} > 0 \\ 0 \text{ otherwise} \end{cases} \quad (3.2)$$

In addition to the set of control variables Z_{it} which refers to the variables described in specification (3.1), specification (3.2) includes a set of period and state dummies to measure the incidence of the tax reform on labor force participation in Ohio. Given that the reform affects the marginal tax on labor earnings, it is expected to alter participation in the labor force for workers at the margin, by increasing the opportunity cost of unemployment. Plus, I am aware of the consideration that the comprehensive reform would affect both the demand and supply of labor. The analysis carried out in this paper should be interpreted as a reduced form exercise that seeks to measure the impacts of the full policy package on labor market outcomes. The design and timing of the reform does not enable me to distinguish the supply response from the demand elasticities.

The rest of the variables are period dummies broken into three groups (before 2006, between 2006-2011 and after 2010) and an indicator for the state of Ohio which equals one if the individual resides in Ohio. The period dummies are defined as such to reflect the timing of the policy. I consider a pre-treatment period (before 2006 which is the excluded period), a treatment period – *d0610* – (2006-2010) and a post-treatment period – *d2011* – (after 2011). This specification captures average differences in wages between Ohio and other states before the policy, over the policy period and after the adoption of the policy. The pre-treatment dummy ensures that any pre-existing heterogeneity in average wage differentials across states is controlled for.

Of interests are the slopes of the interaction terms which measure the wage difference between individuals with similar characteristics living in Ohio and those in control states over and after the policy reform period relative to the pre-policy period. Other specifications include

respectively state-specific linear trends, quadratic and cubic state trends, regional fixed and time variant effects.

3.6.2 Second-stage: wage and self-employment earning regressions

The second stage of the regression studies wages and self-employment earnings for individuals living in Ohio who were targeted by the reform compared to similar individuals in other states. I control for the traditional determinants of labor market outcomes at the micro level such as education ($educ_{it}$), experience (exp_{it}), marital status ($married_{it}$), a dummy variable indicating whether the individual lives in a metropolitan area ($metro_{it}$), the occupation of the individual ($occup_{it}$), and sociodemographic characteristics like gender ($female_{it}$) and race ($white_{it}$).

This set of control variables is chosen to compare labor market outcomes between individuals with similar socioeconomic characteristics before and after the Ohio reform. The regression is akin to a matching estimator, which compares earnings across different cells of the covariates across states and over time. The standard errors are clustered at the state level to capture the fact that individuals living in the same state are subject to identical policy upheavals. Depending on the specification, I also include state-specific linear, quadratic and cubic trends, or region-specific fixed and time variant effects. The control variables are included in the matrix X_{it} according to the following equation.

$$\log(y_{it}) = \beta X_{it} + \alpha_0 \text{dohio}_{it} + \alpha_1 \text{d0610}_{it} + \alpha_2 \text{d2011}_{it} + \alpha_3 \text{dohio.d0610}_{it} + \alpha_4 \text{dohio.d2011}_{it} + v_{it} \quad (3.3)$$

I choose to control for occupation rather than industry for a few reasons. First controlling for both is problematic because they are highly correlated (Angrist 2002). Second, even though they are both endogenous to earnings, the response elasticity of industry of work to economic

shocks is generally higher than that of occupation[14]. Unlike the sector of work, occupation is more likely to remain unchanged in the face of policy shocks that alter wage incentives especially in the short-run.

3.7 Main results and Discussion

3.7.1 Effects of the tax reform on wages

Table 3.2 reports the results of the second-stage Heckman regression for all specifications.. The first four specifications (column 1-4) compare individuals living in Ohio to similar peers in the U.S while the last specification (column 5) considers a control of midwestern states. The first column describes the results of the baseline model with no state or regional fixed and dynamic effects. The coefficients of the interaction terms suggest that the state tax reform of 2005 did not translate into substantial wage gains for prime-age workers in Ohio. The negative and statistically significant estimate of the post-reform interaction parameter reflects the widening wage differential for workers living in Ohio relative to their peers in other states. On average, over the period 2011-2015, the wage differential between an employed prime age individual living in Ohio and a comparable individual living elsewhere in the U.S. was 7.0 percent less than it was over the pre-reform period of 2000-2005. The estimates of the coefficients on the individual socioeconomic characteristics included to control for systematic differences in wage variations, which are not reported in table 3.2 are highly significant in all specifications.

This finding is robust to the inclusion of state fixed effects which control for systematic time-invariant characteristics at the state level that correlate with earning patterns. The results illustrated in column 2 suggest a moderate change (8.0 percent vs. 7.0 percent) of the estimated

[14] The response elasticity of occupation to economic shocks is usually smaller (See Kosteas & Park 2013)

coefficient of interest even when one assumes the existence of unobservable factors affecting wages in a given state that do not vary much over time.

In contrast, when including state-specific time trends in wage dynamics (column 3), I observe a positive but insignificant impact of the policy change on wages. This finding reinforces the hypothesis that U.S. states feature different evolutions in wages due to a heterogeneity in employment and growth opportunities. It also lends support to the belief that the state of Ohio was on a downward wage growth trajectory prior to the reform. The estimate implies that relative to trend, individuals living in Ohio did not experiment a drop in wages after the reform was adopted even though the coefficient is imprecise. This confirms that the estimated negative effects of the first two specifications might be misleading because wages were falling in the state of Ohio prior to the reform.

Column 4 presents a specification that improves upon the previous model by adding quadratic and cubic time trends to better approximate state dynamic shocks to labor markets. The estimated coefficient remains negative but insignificant. This specification seeks to control for other state policy developments that might correlate with wage variations. The measured effects imply no remarkable incidence of the tax reform. I also document in the appendix a rather parsimonious model which includes state fixed effects along with some state time variant policy changes that could influence wages such as sales tax rates, income tax rates, and investment tax credits. The estimates from this latter specification confirm the previous conclusions and do not suggest any significant effect of the tax reform on wage earnings.

Table 3.2: Main results: Effects on wages

	(1)	(2)	(3)	(4)	(5)
Ohio indicator	-0.004	0.03**	0.06	0.11	-0.002
	(0.02)	(0.01)	(0.04)	(0.07)	(0.02)
2006-2010 indicator	-0.02	-0.01	-0.08*	-0.06	-0.08*
	(0.03)	(0.02)	(0.05)	(0.05)	(0.05)
2011-2015 indicator	-0.007	-0.006	-0.10	0.002	-0.10
	(0.02)	(0.03)	(0.08)	(0.07)	(0.08)
Ohio x 2006-2010 indicator	-0.01	-0.03	0.07	0.07	-0.01
	(0.03)	(0.02)	(0.05)	(0.06)	(0.03)
Ohio x 2011-2015 indicator	-0.07***	-0.08***	0.08	-0.02	-0.07***
	(0.02)	(0.03)	(0.09)	(0.08)	(0.03)
State fixed effects	No	Yes	No	No	No
State linear time trends	No	No	Yes	No	No
State linear + quad + cubic time trends	No	No	No	Yes	No
Regional fixed effects	No	No	No	No	Yes
All sociodemographic controls (*)	Yes	Yes	Yes	Yes	Yes

Notes: the dependent variable is the log average real wage for individuals between the ages 25-64. Standard errors are in parentheses. (*) Sociodemographic controls include gender, marital status, race, experience, education, the metropolitan status of residence, and occupation. These characteristics were all significant at 1%.

Finally, I consider regional heterogeneity in wages as evidenced by the results exposed in column (5) which include regional fixed effects. Figure 3.2 describes the differential in output growth between the state of Ohio and its contiguous neighbors compared to the same differential with all other U.S. states. The smaller gap with respect to the midwestern states indicates that these states might represent better controls for wage patterns in Ohio. With the inclusion of regional fixed effects, I am basically comparing individuals in Ohio to similar peers within the midwestern region. The negative estimated effect of -7.0 percent suggests that compared to its contiguous counterparts, the state of Ohio recorded a decline of average wages paid to prime-age workers over the period 2010-2015 relative to the pre-policy period. The primary insight that emerges from this finding is that using individuals living in the geographic neighbors of Ohio as controls does not alter the main conclusion drawn from the initial specifications. I also estimated a model with region fixed effects along with state linear time trends which yields a conclusion identical to the one drawn from column 3.

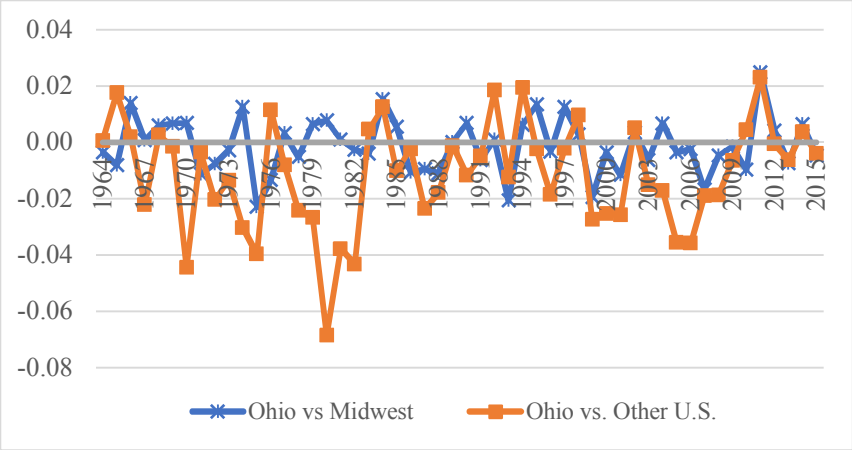

Figure 3.2: Growth of output in Ohio relative to midwestern vs. Other U.S. states

3.7.2 Effects of the tax reform on self-employment earnings

The policy reform in Ohio affected both the taxation of personal and corporate income but to different magnitudes. On the one side, the dramatic cut to the Corporate Francise Tax almost eliminated the taxation of corporate income, while the personal income tax has been reduced by 21% over the period 2006-2010. These changes might increase incentives to report earnings, particularly for self-employed individuals. This hypothesis is consistent with a wealth of empirical findings (James Long 1982, Richard Goode 1976, Saez 2010, le Maire & Schjerning 2013) that have suggested that taxable income is sensitive to marginal income tax rate especially for self-employed individuals for whom taxation involves some degree of voluntary compliance. The argument supports that a reduction in tax rates might induce a higher reporting of self-employment income.

I explore this possibility by comparing self-employment earnings of individuals living in Ohio respectively to similar individuals living in the Midwestern and other U.S. states. I found strong evidence in support of this hypothesis. The coefficient of the treatment period interaction term carries a positive value which is statistically significant at the one percent level in all

specifications. After the reform, self-employed individuals living in Ohio reported higher earnings on average relative to control individuals in neighboring states and elsewhere.

The baseline estimated impact described in column (1) of Table 3.3 implies that the difference in reported self-employment income between Ohio and all other states was 14 percent higher over the period 2006-2010 relative to the pre-treatment period. This finding is robust to the inclusion of state fixed effects and remains significantly positive as evidenced by the results in column (2). Also, the inclusion of state and region-specific linear trends and region fixed effects (columns 3, 4, and 5) did not substantially affect this conclusion. Unfortunately, this paper could not identify the type of income subject to more reporting (capital vs. labor). Doing so would require an additional knowledge regarding the incorporation status of self-employed individuals, which is not provided by the CPS dataset.

Table 3.3: Main results: Effects on self-employment earnings

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Ohio indicator	-0.12***	-0.17***	-0.05	-0.11***	-0.12***
	(0.04)	(0.05)	(0.10)	(0.04)	(0.04)
2006-2010 indicator	-0.03***	-0.03***	-0.04**	-0.03***	-0.03*
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
2011-2015 indicator	-0.07***	-0.07***	-0.08***	-0.06*	-0.06*
	(0.01)	(0.01)	(0.03)	(0.01)	(0.04)
Ohio x 2006-2010 indicator	0.14**	0.12**	0.21**	0.13***	0.14***
	(0.06)	(0.06)	(0.10)	(0.06)	(0.06)
Ohio x 2011-2015 indicator	0.02	-0.001	0.16	0.01	-0.02
	(0.09)	(0.09)	(0.19)	(0.09)	(0.09)
State fixed effects	No	Yes	No	No	No
State linear time trends	No	No	Yes	No	No
Region-specific effects	No	No	No	Yes	No
Regional time trends	No	No	No	No	Yes
All sociodemographic controls (*)	Yes	Yes	Yes	Yes	Yes

Notes: the dependent variable is the log self-employment earnings for individuals between the ages 25-64. Standard errors are in parentheses. (*) Sociodemographic controls include gender, marital status, race, experience, education, the metropolitan status of residence, and occupation.

3.7.3 Effects of the tax reform on employment

The results in Table 3.4 describe the probability of being employed for individuals living in Ohio relative to others (i.e. the first stage of the wage regressions). The dependent variable is a dummy variable that equals 1 if the person is employed and 0 otherwise. This is a Linear Probability Model with robust standard errors. In addition to the socioeconomic determinants of individual wage earnings present in the wage regressions, I also included several controls affecting labor force participation and employment status such as the number of kids less than 5 years old and non-work income.

Column 1 presents the baseline specification with no state or regional effects and indicates that the reform might have increased the probability of employment in the state by 0.2 percent over the period 2011-2015 even though this estimate is very imprecise. This conclusion is robust to the inclusion of state and region fixed effects as evidenced by the results in columns (2) and (3). In contrast, when including region year effects which allow the comparison between Ohio and its regional neighbors over time, the employment effect increases to 0.8 percent becoming statistically significant at 1%. This finding suggests that the reform might have boosted employment in Ohio relative to a control of midwestern states.

Additionally, as a robustness check, I present in the Appendix the results for the female subpopulation. This subgroup is usually described to be responsive to marginal tax changes by supplying more hours of work or participating in the labor force. The results in Table 3.1.4 and Table 3.1.5 do not suggest any significant positive effect of the policy on female's labor market outcomes in Ohio.

Table 3.4: Main results: Effects on the probability of employment

	(1)	(2)	(3)	(4)	(5)
Ohio indicator	-0.01**	-0.02**	-0.01**	-0.001	-0.0005
	(0.004)	(0.007)	(0.004)	(0.005)	(0.002)
2006-2010 indicator	-0.03***	-0.03***	-0.03***	-0.03***	-0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
2011-2015 indicator	-0.02***	-0.02***	-0.01**	-0.02***	-0.03***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.007)
Ohio x 2006-2010 indicator	0.01	0.01	0.01	0.01	0.01***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.005)
Ohio x 2011-2015 indicator	0.002	0.003	0.002	0.003	0.008***
	(0.008)	(0.007)	(0.008)	(0.007)	(0.004)
State fixed effects	No	Yes	No	No	No
State linear time trends	No	No	Yes	No	No
Region fixed effects	No	No	No	Yes	No
Regional year fixed effects	No	No	No	No	Yes
All sociodemographic controls (*)	Yes	Yes	Yes	Yes	Yes

Notes: the dependent variable is a dummy variable for employment status for individuals between the ages 25-64. Standard errors are in parentheses. (*) Sociodemographic controls include gender, marital status, race, experience, education, the metropolitan status of residence, occupation, non-work income and number of children less than 5 years old.

This specification presents the advantage of using a control group that better approximates employment dynamics in Ohio. However, the presence of spatial correlations through migration and business reallocations requires some caution with regards to this interpretation. The potential shift of economic activity from neighboring states to Ohio in response to the tax cut might overstate the employment benefits of the reform. I will have to verify if such spillovers occurred by analyzing migration or population growth around the policy reform.

3.8 Limitations and avenues for future research

This paper observes several snapshots of middle age individuals between the ages 25-64 over the period 2000-2015. Given that the analysis does not feature a longitudinal study of working age persons exposed to the policy, the results described here are confounded by changes to the structure of the workforce in Ohio. This could occur if the policy intervention spurs a higher inflow of individuals to the state. To the extent that the resulting in-migration flow does not alter the

composition of workers across different cells of the socioeconomic controls, these estimates provide a valuable approximation of the effects of the tax reform on labor market outcomes.

The negligible estimated effect of the tax change on employment status might just reflect a population inflow that reshapes the composition of the labor market of Ohio. Unfortunately, there is no continuous series of cross-state migration flows over an extended period. However, the Census Bureau through the State to state migration flow tables provides net migration statistics over five-year periods for each state. Over the period 2005-2010, the state of Ohio actually featured a negative net migration of 170,470 individuals equivalent to 1.5 percent of its 2005 population. This implies that the in-migration of population that would bias the estimated effects probably did not occur.

To provide additional support to this conclusion, I also analyze the dynamics of population growth in the state around the policy reform. Figure 3.3 which plots the growth of population in the state of Ohio relative to its neighbors over the period 1970-2015, confirm this hypothesis. I consider the possibility of a minimal population growth in Ohio being associated with an upsurge in the migration of working age individual as a highly unlikely scenario since it would imply a restructuring of the age composition of the state's residents.

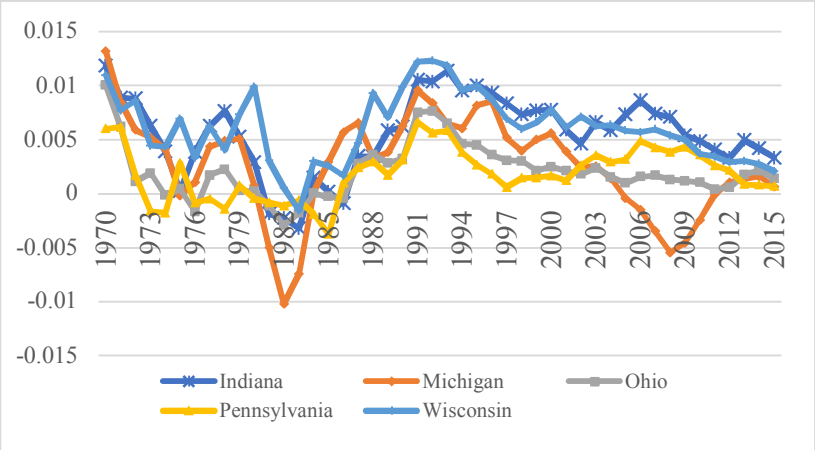

Figure 3.3: Growth of population in Midwestern states

Another limitation of the analysis described in this paper relates to the measured impacts of the policy reform on self-employment earnings. As discussed earlier, self-employed businesses might be incorporated and therefore subject to the corporate tax. It would have been appropriate to explore the extent to which the differential magnitudes of the cuts to alternative sources of income (capital vs. labor) affected the reporting of both types of returns. Additionally, considering that the reform changed several taxes simultaneously, I was not able to relate the estimated effects to a particular tax which limits the opportunity to draw meaningful conclusions for other policy considerations.

Last, the ideal set-up would require using a longitudinal dataset of individuals and observe how incentives to work and earnings are changed by the policy. This would not only allow me to control for individual heterogeneity but also to observe annual variations in labor market outcomes for the treated population even though I would still have to identify the appropriate control group. The relevance of my results rests upon the hypothesis that the reform did not dramatically alter the composition of the Ohio workforce with regards to unobservable individual characteristics. The only publicly accessible longitudinal dataset I could have used is the Panel Study of Income Dynamics (PSID). I learned with a quick examination that this dataset is not representative at the state level and features a non-negligible rate of cross-state migration over time.

3.9 Conclusion

Secular stagnation and ever-decreasing levels of corporate tax collections around the globe contributed to the re-emergence of a widespread interest in tax reform. Both ideologies of the political spectrum in the U.S favor some changes to the current tax system. The unsatisfactory empirical evidence which does not usually provides room for an asymmetrical response to tax changes limits the scope for policy interventions. There is good reason to believe that tax increases

and reductions do not trigger the same behavioral reactions from economic agents and that dramatic reforms are more likely to cause significant upheavals in labor market outcomes. This paper explores the economic incidence of state tax cuts by studying the impact of the 2005 Ohio tax overhaul on wages, self-employment earnings, and employment status.

I observe several cross-sectional random samples interviewed through the microdata survey of the Current Population Survey (IPUMS-CPS) over the period 2000-2015 and compare groups of individuals in Ohio to similar individuals in (i) the midwestern states and (ii) the U.S. in general, before and after the reform. I also address the main caveat of previous empirical works, related to time-varying heterogeneity in potential earnings across jurisdictions.

My preferred specifications suggest that the reform did not significantly boost employment and wages. However, it seems to have increased the average reported self-employment earnings by 12-21% depending on the specification. This finding is consistent with a literature (Long 1982, Goode 1976, Le Maire and Scherjning 2013) that defends that the degree of voluntary compliance involved in the taxation of self-employment returns reflects on the sensitivity of this type of income to tax differentials. As a comparaison, the self-employment taxable income elasticity measured here is close to the estimate found by Le Maire and Scherjning 2013 (14-20%). The employment and wage results complement the conclusion that corporate tax cuts do not significantly boost wages and employment in the long-run as evidenced by other empirical works (Ljungqvist and Smolyansky 2014, Fuest et al. 2017). The results described in this paper imply that in the short-run the windfall of a corporate tax cut is not necessarily shared with employees. However, the extra revenue could be reinvested or used to repurchase shares (Chetty and Saez 2003). Unfortunately, I am not able to explore these dimensions for the Ohio reform.

One major caveat of this analysis is that since I am not able to observe a longitudinal dataset of individuals affected by the policy, the relevance of my results rests upon the hypothesis that the reform did not dramatically alter the composition of the Ohio workforce with regards to unobservable individual characteristics. To circumvent this shortcoming, I could have used an alternative source of data such as the Panel Study of Income Dynamics (PSID). However, a brief investigation reveals that this dataset is not representative at the state level and features a substantial rate of cross-state migration over time. Additionally, this paper did not explore the extent to which the Ohio tax reform affected hours of work supplied by individuals due to the poor quality of this variable in the dataset. That intensive margin of the response might be more meaningful than the extensive margin investigated in this paper.

Importantly, the negligible measured wage and employment effects only relate to short-run impacts and cannot be extrapolated to other contexts. The long-run incidence might be much higher than what is reported in this analysis, and one should expect investment patterns to react to the dramatic corporate tax cut over a longer time frame. It would also be misleading to automatically relate the findings in Ohio to other policy considerations at the federal level especially with regards to the recently adopted tax code. Nonetheless, the self-employment results suggest that any federal reform that alters the tax treatment of different sources of income would likely induce a higher reporting of certain earnings in the short-run. Finally, the timing of the reform did not enable me to disentangle the separate effects of the corporate tax cut from the personal tax and the property tax reductions since the changes were introduced simultaneously.

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APPENDIX A.

Chapter 1 Additional Content

A.1 Chapter 1 Additional Content Tables

Table 1.2: Results with regional business cycle (deviation of regional employment from average)

Dependent variable	*Effective rate*[(a)]			*Statutory rate*[(a)]		
Specification	(1)	(2)	(3)	(1)	(2)	(3)
	Change	Cut	Hike	Change	Cut	Hike
Tax competition						
[(a)]*Diff. with avg. rate of contiguous states* $_{t-1}$	-0.007***	0.793**	-0.073	-0.008***	0.782**	-0.002
	(0.002)	(0.400)	(0.219)	(0.003)	(0.351)	(0.172)
Domestic economy						
[(a)]*Growth output* $_{t-1}$	-0.008***	0.132	-0.703***	0.005	0.128	-0.442***
	(0.002)	(0.196)	(0.183)	(0.003)	(0.200)	(0.173)
Political Control						
Republican Control $_{t-1}$	-0.004***	0.010	-0.030***	0.005***	0.049**	-0.013*
	(0.001)	(0.023)	(0.008)	(0.001)	(0.025)	(0.009)
Democratic Control $_{t-1}$	0.000	-0.006	0.013	0.000	0.015	0.006
	(0.001)	(0.019)	(0.014)	(0.000)	(0.014)	(0.014)
Budgetary pressures						
Budget Deficit$_{t-1}$	-0.000	-0.013	-0.031***	-0.000	-0.016	-0.001
	(0.000)	(0.013)	(0.013)	(0.001)	(0.014)	(0.012)
Debt to output ratio $_{t-1}$	-0.000	0.037	-0.006	-0.000*	0.020	-0.001
	(0.000)	(0.026)	(0.015)	(0.000)	(0.037)	(0.012)
Post-1986 dummy	-0.007***	0.033**	-0.043***	-0.008***	0.031**	-0.045***
	(0.001)	(0.017)	(0.013)	(0.001)	(0.016)	(0.013)
Constant	0.001***	0.015	0.122	0.001***	-0.003	0.086***
	(0.000)	(0.024)	(0.019)	(0.000)	(0.026)	(0.017)
Observations	1824	1824	1824	1824	1824	1824
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional Business Cycle	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.03	0.02	0.03	0.03	0.02	0.02

Each model is estimated by OLS with a panel of 48 U.S. states over the period 1969-2014.
All standard errors are robust and clustered at the state level. Regional Business Cycle is proxied by deviation of regional employment from average. [(a)] Effective and statutory tax changes are measured in units (5% is equivalent to 0.05). ** significant at 10%; ** significant at 5%; *** significant at 1%*

Table 1.3: Results with year fixed effects

Dependent variable	*Effective rate*[(a)]			*Statutory rate*[(a)]		
Specification	(1)	(2)	(3)	(1)	(2)	(3)
	Change	Cut	Hike	Change	Cut	Hike
Tax competition						
[(a)]*Diff. with avg. rate of contiguous states*$_{t-1}$	-0.007***	0.814**	-0.077	-0.008***	0.788***	0.005
	(0.003)	(0.401)	(0.216)	(0.003)	(0.352)	(0.167)
Domestic economy						
[(a)]*Growth output*$_{t-1}$	-0.006***	0.249	-0.390**	-0.007**	0.297	-0.281*
	(0.002)	(0.211)	(0.191)	(0.003)	(0.234)	(0.187)
Political Control						
Republican Control$_{t-1}$	-0.003**	0.015	-0.015*	0.004**	0.044*	-0.004
	(0.002)	(0.024)	(0.010)	(0.002)	(0.025)	(0.008)
Democratic Control$_{t-1}$	0.000	-0.011	0.014	0.000	0.015	0.001
	(0.001)	(0.018)	(0.013)	(0.000)	(0.014)	(0.013)
Budgetary pressures						
Budget Deficit$_{t-1}$	0.000	0.038***	-0.005	-0.000	0.002	0.002
	(0.000)	(0.015)	(0.018)	(0.001)	(0.018)	(0.015)
Debt to output ratio$_{t-1}$	-0.001	0.029	-0.010	-0.000*	0.034*	-0.007
	(0.000)	(0.026)	(0.016)	(0.000)	(0.023)	(0.013)
Post-1986 dummy	–	–	–	–	–	–
Constant	0.009*	-0.013	0.143***	0.002***	0.000	0.192***
	(0.005)	(0.043)	(0.047)	(0.000)	(0.034)	(0.057)
Observations	1824	1824	1824	1824	1824	1824
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Regional Business Cycle	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.07	0.06	0.10	0.06	0.07	0.07

Each model is estimated by OLS with a panel of 48 U.S. states over the period 1969-2014.
All standard errors are robust and clustered at the state level. Regional Business Cycle is proxied by deviation of regional employment from average in a given year. [(a)] Effective and statutory tax changes are measured in units (5% is equivalent to 0.05). ** significant at 10%; ** significant at 5%; *** significant at 1%*

Table 1.4: Results with regional heterogeneity

Dependent variable	*Effective rate*[(a)]			*Statutory rate*[(a)]		
Specification	(1)	(2)	(3)	(1)	(2)	(3)
	Change	Cut	Hike	Change	Cut	Hike
Region (excluded *New England)*						
Mideast	-0.0004	-0.021	-0.039*	-0.0004	-0.029	-0.057***
	(0.0003)	(0.035)	(0.024)	(0.0003)	(0.028)	(0.024)
Great Lakes	-0.0006	0.024	-0.024	-0.0006	0.017	-0.024
	(0.0005)	(0.037)	(0.026)	(0.0005)	(0.037)	(0.024)
Plains	-0.0002	-0.023	-0.003	-0.0004	-0.033	-0.046**
	(0.0003)	(0.036)	(0.029)	(0.0003)	(0.036)	(0.024)
Southeast	-0.0004	-0.002	-0.036*	-0.0005**	-0.031	-0.064***
	(0.0002)	(0.038)	(0.024)	(0.0002)	(0.040)	(0.020)
Southwest	-0.0004	-0.049	-0.026	-0.0004	-0.035	-0.054***
	(0.0004)	(0.036)	(0.024)	(0.0004)	(0.037)	(0.025)
Rocky Mountain	-0.0002	-0.037	-0.036	-0.0001	-0.049	-0.048**
	(0.0003)	(0.039)	(0.027)	(0.0003)	(0.042)	(0.024)
Far West	-0.0005	-0.034	-0.058**	-0.0005	-0.031	-0.061***
	(0.0003)	(0.030)	(0.027)	(0.0004)	(0.030)	(0.023)
Constant	-0.0016***	0.029	0.156***	0.0018***	0.024	0.165***
	(0.0004)	(0.045)	(0.035)	(0.0004)	(0.045)	(0.031)
Observations	1824	1824	1824	1824	1824	1824
State fixed effects	No	No	No	No	No	No
Regional Business Cycle	Yes	Yes	Yes	Yes	Yes	Yes
Region fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
All other controls included	Yes	Yes	Yes	Yes	Yes	Yes
R-squared	0.02	0.03	0.03	0.02	0.03	0.03

Each model is estimated by OLS with a panel of 48 U.S. states over the period 1969-2014.
All standard errors are robust and clustered at the state level. Regional Business Cycle is proxied by deviation of regional employment from average in a given year. [(a)] Effective and statutory tax changes are measured in units (5% is equivalent to 0.05). ** significant at 10%; ** significant at 5%; *** significant at 1%*

Table 1.5: State statutory and effective corporate tax rates between 1970 and 2014

Region	State	1970	2014	1970	2014
		Effective rates (%)		*Statutory rates (%)*	
New England					
	Connecticut	8.0	9.0	8.0	9.0
	Maine	4.0	8.9	4.0	8.9
	Massachusetts	7.5	8.0	8.5	8.0
	New Hampshire	6.0	9.3	7.0	9.3
	Rhode Island	8.0	9.0	7.0	9.0
	Vermont	6.0	8.5	6.0	8.5
Mideast					
	Delaware	6.0	8.7	6.0	8.7
	Maryland	7.0	8.3	7.0	8.3
	New Jersey	4.3	9.0	4.3	9.0
	New York	7.0	7.1	7.0	7.1
	Pennsylvania	12.0	10.0	7.5	10.0
Great Lakes					
	Illinois	4.0	7.0	4.0	7.0
	Indiana	2.0	7.5	2.0	7.5
	Michigan	5.6	6.0	5.6	6.0
	Ohio	No Tax	0.3	No Tax	0.3
	Wisconsin	7.0	7.9	7.0	7.9
Plains					
	Iowa	6.2	10.1	8.0	12.0
	Kansas	6.8	7.1	4.5	7.1
	Minnesota	11.3	9.8	11.3	9.8
	Missouri	2.6	5.2	5.0	6.3
	Nebraska	2.6	7.8	2.6	7.8
	North Dakota	3.1	3.0	6.0	4.5
	South Dakota	No Tax	No Tax	No Tax	No Tax
Southeast					
	Alabama	2.6	4.3	5.0	6.5
	Arkansas	4.2	6.5	6.0	6.5
	Florida	No Tax	5.5	No Tax	5.5
	Georgia	6.0	6.0	6.0	6.0
	Kentucky	7.0	6.8	7.0	6.8
	Louisiana	2.1	5.3	4.0	8.0
	Mississippi	4.0	5.0	4.0	5.0
	North Carolina	6.0	6.0	6.0	6.0
	South Carolina	6.0	5.0	6.0	5.0
	Tennessee	5.0	6.5	5.0	6.5
	Virginia	5.0	6.0	5.0	6.0
	West Virginia	6.0	6.5	6.0	6.5
Southwest					
	Arizona	6.0	6.5	8.0	6.5
	New Mexico	5.0	7.3	5.0	7.3
	Oklahoma	4.0	6.0	4.0	6.0
	Texas	3.3	No Tax	No Tax	No Tax
Rocky Mountain					
	Colorado	5.0	4.6	5.0	4.6
	Idaho	6.0	7.4	6.0	7.4
	Montana	6.3	6.8	6.3	6.8
	Utah	6.0	5.0	6.0	5.0
	Wyoming	No Tax	No Tax	No Tax	No Tax
Far West					

	California	7.0	8.8	7.0	8.8
	Nevada	No Tax	No Tax	No Tax	No Tax
	Oregon	6.0	7.6	6.0	7.6
	Washington	No Tax	No Tax	No Tax	No Tax

Table 1.6: Summary statistics of main variables

Variable	Description	Mean	Min	Max	S. D.	
davg	Difference with tax average of neighbors	-0.001	-0.078	0.072	overall	0.028
					between	0.026
					within	0.012
cut	Corporate Cut dummy	0.056	0.000	1.000	overall	0.230
					between	0.059
					within	0.222
hike	Corporate Hike dummy	0.072	0.000	1.000	overall	0.258
					between	0.046
					within	0.254
repcon	Republican control dummy	0.182	0.000	1.000	overall	0.386
					between	0.192
					within	0.335
demcon	Democratic control dummy	0.306	0.000	1.000	overall	0.461
					between	0.233
					within	0.399
ggsp	Growth of output	0.029	-0.168	0.305	overall	0.037
					between	0.008
					within	0.036
debt_gsp	Debt to output ration	0.643	0.030	2.276	overall	0.387
					between	0.338
					within	0.195
budef	Dummy for budget deficit	0.177	0.000	1.000	overall	0.382
					between	0.075
					within	0.374

A.2 Chapter 1 Additional Content: Note on the 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 $\upsilon_{s,t} = \{1.0, 0.5, 0.0\}$, the effective corporate tax rate in state *s* in period *t* is defined by:

$$\tau_{s,t}^{E,S} = \tau_t^{L,S}(1 - \tau_{s,t}^{E,\mathrm{F}}\upsilon_{s,t}) \tag{B1}$$

Where $\tau_t^{L,S}$ denotes the statutory corporate tax rate in state *s* over period *t* and $\tau_{s,t}^{E,\mathrm{F}}$ 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, I 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:

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

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:

$$\tau_{s,t}^{E,\mathrm{S}} = \frac{\tau_{s,t}^{L,\mathrm{S}}(1-\upsilon_{s,t}\tau_t^{\mathrm{L,F}})}{(1-\upsilon_{s,t}\tau_{s,t}^{\mathrm{L,S}}\tau_t^{L,F})} \tag{B3}$$

$$\tau_{s,t}^{E,\mathrm{F}} = \frac{\tau_t^{L,\mathrm{F}}(1-\tau_{s,t}^{\mathrm{L,S}})}{(1-\upsilon_{s,t}\tau_{s,t}^{\mathrm{L,S}}\tau_t^{L,F})} \tag{B4}$$

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

APPENDIX B.

Chapter 2 Additional Content

B.1 : Chapter 2 Additional Content Tables

Table 2.1.1: Employment and wage effects

No imperfect competition				***Imperfect competition***	
Growth of employment (1)		*Growth of wage (2)*		*Growth of wage (3)*	
Tax changes	-0.05*** (0.02)	*Tax changes*	-0.17* (0.42)	*Tax hikes*	-0.14** (0.07)
Tax cuts	-0.0005 (0.02)	*Tax cuts*	0.02 (0.06)	*Tax hikes* Estab.*	0.0018* (0.001)
Tax hikes	-0.17*** (0.06)	*Tax hikes*	-0.14* (0.08)		
Controls					
Establish.	-0.00005 (0.00005)	*Establish.*	-0.00001 (0.00002)	*Establish.*	-0.00001 (0.00001)
Population	0.77*** (0.05)	*Population*	0.40*** (0.11)	*Population*	0.41*** (0.09)
emp interior counties	0.05*** (0.01)	*emp interior counties*	0.05 (0.04)	*emp interior counties*	0.06 (0.05)
Number of Obs. = 1931 *R-squared= 0.15*		*Number of Obs. = 1585* *R-squared= 0.03*		*Number of Obs. = 1585* *R-squared= 0.04*	

*Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%*

Table 2.1.2: Employment and wage effects (No combined reporting)

Growth of employment (1)		*Growth of wage (2)*	
Tax changes	-0.07 (0.05)	*Tax changes*	-0.43*** (0.05)
Tax cuts	0.06 (0.10)	*Tax cuts*	0.53 (0.42)
Tax hikes	-0.14 (0.10)	*Tax hikes*	-0.28*** (0.07)
Controls			
Establish.	-0.000006 (0.00001)	*Establish.*	0.00001 (0.00004)
Population	0.90*** (0.08)	*Population*	0.58*** (0.11)
emp interior counties	0.04* (0.02)	*emp interior counties*	0.09* (0.05)
Number of Obs. = 955 *R-squared= 0.12*		*Number of Obs. = 955* *R-squared= 0.04*	

*Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%*

Table 2.1.3: Employment and wage effects (Differential combined reporting)

Growth of employment (1)		*Growth of wage (2)*	
Tax changes	-0.06** (0.03)	*Tax changes*	-0.005 (0.05)
Tax cuts	-0.01 (0.02)	*Tax cuts*	0.008 (0.08)
Tax hikes	-0.23*** (0.06)	*Tax hikes*	-0.01 (0.01)
Controls			
Establish.	0.000006 (0.00005)	*Establish.*	0.00003 (0.00002)
Population	0.90*** (0.08)	*Population*	0.12 (0.24)
emp interior counties	0.10*** (0.03)	*emp interior counties*	0.04 (0.09)
Number of Obs. = 669 *R-squared= 0.25*		*Number of Obs. = 669* *R-squared= 0.01*	

*Standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%*

Table 2.1.4: Summary statistics of main variables

Variable	*Description*		*Mean*	*Std. Dev.*
Employment	Employment	overall	44917.91	164338.3
		between		159964.7
		within		37477.2
Effcit	Effective state corporate tax rates	overall	0.0599248	0.025454
		between		0.022244
		within		0.012379
Establishments	Number of establishments	overall	41.89928	71.43846
		between		59.21136
		within		36.86805
CIT	Statutory state corporate tax rates	overall	0.0592841	0.030204
		between		0.027939
		within		0.011487
Population	County population	overall	83310.55	274210.3
		between		269515.1
		within		50137.87
income	County personal Income	overall	1990945	8893925
		between		7228974
		within		3,773,177
Avg. wage	Average weekly wage	overall	402.36	206.19
		between		112.85
		within		5178441

B.2 Chapter 2 Additional Content: Theoretical model

B.2.1. Neoclassical general equilibrium models of corporate taxation

Following the benchmark contribution of Harberger (1962), most neoclassical general equilibrium models of corporate taxation divide the economy into multiple sectors and treat the corporate tax as a partial tax on the returns of capital in the corporate sector. This approach implies that the inefficiencies of the tax arise primarily from the reallocation of factors across sectors. Of vital importance is the role played by a number of parameters including factor substitution elasticities, factor intensities, and substitution elasticities in demand between products in the incidence of the tax.

In a nutshell, two primary channels of effects are identified: the substitution effect and the output effect. As the tax induces an increase in the gross returns to capital (i.e.) the cost of capital, businesses in the corporate sector substitute away from capital by demanding more labor featuring a substitution effect. To the contrary, the output effect relates to the decrease in the corporate demand as the tax pushes up output prices in the corporate sector. These basic forces are slightly altered in the presence of imperfect competition given the importance of strategic interaction in such market organizations.

B.2.2. General equilibrium model of corporate taxation with imperfect competition

Davidson and Martin (1985) describe the theoretical framework that underlies the empirical work carried out in this paper. In this section we expose the main conclusions of this general equilibrium model while highlighting the role played by imperfect competition in the analysis of the incidence of the tax on factor returns. We also conduct some comparative statics to analyze how the corporate market structure distorts the wage sensitivity of the corporate tax.

- Theory

Consider that the economy is divided into two sectors. The non-corporate sector denoted Y is composed of firms operating in a perfectly competitive market. The corporate sector is modeled as an oligopolistic market featuring N identical firms, each producing a single output X under a constant marginal cost c.

Firms in the corporate sector play an infinitely repeated Cournot game. Davidson and Martin assume that tacit collusion is the norm in the corporate sector by making use of one fundamental insight of the supergame literature[15]: As a market situation is repeated infinitely, the industry may settle at a collusive equilibrium even if the firms are not explicitly colluding. Specifically, the authors consider that the tacit agreement is enforced through the "grim trigger" strategy; that is, all firms produce their share of the cartel quantity unless some firm cheats.

If any cheating occurs at time t, all firms revert permanently to the static Nash equilibrium output. Each firm compares the current higher profits earned while deviating from the agreement to the present value of future losses subsequent to the dissolution of the Cartel. The net gains from cheating Z are given by:

$$Z = (\pi^{ch} - \pi^{c}) - \frac{1}{r}(\pi^{c} - \pi^{n}) \qquad \text{(B1)}$$

where π^{ch} denotes the profit from cheating, π^{c} the profit per firm under constrained production, π^{n} the profit per firm in the static Nash equilibrium, and r the price of capital, (i.e.) the interest rate. Note here that the model uses the interest rate which is an opportunity cost as the discount rate from the perspective of a producer.

[15] See Aumann 1959; Friedman 1971, 1977; Rubinstein 1979, 1980; Green 1980; Radner 1980; Abreu 1983; Porter 1983; Brock and Scheinkman

The cartel chooses output per firm Q_c from the set of sustainable outputs $\Omega=\{Q_c : Z \leq 0,\ Q_c \geq 0\}$ to maximize joint profits. The profit of a typical firm of the cartel is defined as:

$$\pi^c = Q_c[P(X_c, \omega) - c] \tag{B2}$$

where P(.) is the inverse demand function for the corporate good X, c is the constant unit cost of production and ω refers to a vector of shift parameters of the demand function. The static Nash equilibrium profits is derived from the following optimization problem.

$$\pi^n = \max_{Q_n} Q_n \ \{P[(N-1)Q_{-n} + Q_n, \omega] - c\} \tag{B3}$$

Davidson and Martin assume that the demand schedule is continuously differentiable which enables the existence of a solution to the Cournot Nash equilibrium set-up. Lastly, the profits from cheating is defined as:

$$\pi^{ch} = \max_{Q_{ch}} Q_{ch} \ \{P[(N-1)Q_c + Q_{ch}, \omega] - c\} \tag{B4}$$

Considering that the representative consumer has preferences represented by the following utility function:

$$U(X, Y) = \alpha \ln(X) + (1 - \alpha) \ln(Y) \tag{B5}$$

Therefore, the inverse demand schedule for the corporate good X is given by:

$$q_x = \frac{\alpha M}{X} \tag{B6}$$

where M is the total income, and α is the budget share of the corporate good X.

Substituting the inverse demand schedule (6) into profits equations (2), (3) and (4), Davidson and Martin solve for π^c, π^n and π^{ch}. Further plugging these profit functions into the net gains from cheating expression (1) and setting $Z(Q_c)=0$ yields the cartel output per firm Q_c.

$$Q_c = \frac{\alpha M(N-1)(rN-1)^2}{cN^2(rN+1)^2} \tag{B7}$$

Assuming that $r > 1/N$, the inverse demand function for the corporate good X becomes:

$$q_x = \frac{N(rN+1)^2}{(N-1)(rN-1)^2} c \tag{B8}$$

As could be noticed the interest rates which also denotes the user cost of capital enters the inverse demand curve through two separate arguments. First it affects the price indirectly through the c argument given that the unit cost of production of the corporate good depends on the cost of capital. It also enters directly in the above equation (8) highlighting the impact of the price of capital on the pricing decision of the corporate cartel. More specifically the effect of the interest rate on the corporate price is:

$$\frac{dq_x}{dr} = \frac{\partial q_x}{\partial c}\left(\frac{\partial c}{\partial r}\right) + \frac{\partial q_x}{\partial r} \tag{B9}$$

The first term in this expression is positive since the unit cost of production increases with the user cost of capital (interest rate). The second term in (9) is negative because the interest rate (the net return to capital) represents the discount rate used by the cartel to set up output per firm. As the interest rate increases, the collusive agreement becomes sustainable only if the cartel pushes up production per firm, resulting in a lower price for the corporate good.

- Introduction of a corporate tax into the model

The inclusion of a corporate tax affects the net returns to capital in the economy through the mobility of capital out of the corporate sector. In addition to the partition of the economy into a corporate sector X and a non-corporate sector Y, the model assumes that producers employ Labor (L) and Capital (K) as factors of production, both of which are fully mobile between sectors. Firms are price takers on factor markets and the fixed stocks of labor and capital are fully employed.

Following the notations adopted above, q_j refers to the gross of tax price of good *j*, c_j the unit cost function for good *j*, *w* and *r* the net returns to labor and capital, T_j one plus the ad valorem output tax on good *j*, T_{ij} one plus the partial tax on input *i* used in the production of output *j,* and M the aggregate income. Assuming that preferences in demand for the representative consumer are Cobb-Douglas and the corporate sector is oligopolistic, the price function for the corporate good X is:

$$q_x = q_x(M, c_x T_x, r) \tag{B10}$$

given that the non-corporate sector is competitive, then the price of good Y equals the marginal cost of production:

$$q_y = c_y(wT_{Ly}, rT_{Ky})T_y \tag{B11}$$

Denoting aggregate demands for both goods as X and Y and assuming that all income is spent in the equilibrium then we have:

$$M = q_x X + q_y Y \tag{B12}$$

Given that the supplies of capital K_0 and labor L_0 are fixed the full employment condition for factors of production becomes:

$$\begin{aligned} K_0 &= c_{Kx} X + c_{Ky} Y \\ L_0 &= c_{Lx} X + c_{Ly} Y \end{aligned} \tag{B13}$$

Where c_{ij} represents the input *i* requirement for the production of a unit of output *j*. Finally, equilibrium in the goods markets require aggregate demand to equal aggregate supply for each market:

$$X = X(q_x, q_y, M)$$
$$Y = Y(q_x, q_y, M) \tag{B14}$$

Using the wage rate as numeraire, Davidson and Martin drop the full employment condition for good X in (14). Taking the corporate output tax into account the corporate oligopoly price becomes:

$$P_x = \frac{N(rN+1)^2}{(N-1)(rN-1)^2} c_x T_x \tag{B15}$$

Finally differentiating the two price conditions (11) and (15), we get:

$$\hat{q}_x - \hat{q}_y = -(\theta^* + \Psi) + (\hat{T}_x - \hat{T}_y) + \theta_{Kx}\hat{T}_{Kx} - \theta_{Ky}\hat{T}_{Ky} \tag{B16}$$

where as usual the circumflex indicates proportional changes ($\hat{X} = dX/X$), $\theta^* = \theta_{Lx} - \theta_{Kx}$, $\theta_{Lj} \equiv wc_{Lj}T_j/c_j$ represents the share of costs going to labor in industry j, $\theta_{Kj} \equiv rc_{Kj}T_j/c_j$ represents the share of costs going to capital in industry j, and θ^* measures the value of factor intensity in the corporate industry. The second term in equation (16), $\Psi = 4rN/[(rN)^2 - 1]$ captures the effect of changes in the discount rate on the ability of the cartel to enforce its output restriction.

Differentiating the full employment conditions (13) and making use of the Cobb Douglas nature of the utility preferences, we get:

$$\hat{X} - \hat{Y} = -(\hat{q}_x - \hat{q}_y) \tag{B17}$$
$$\lambda^*(\hat{X} - \hat{Y}) = -(a_x\sigma_x + a_y\sigma_y)\hat{r} - a_x\sigma_x\hat{T}_x - a_y\sigma_y\hat{T}_y \tag{B18}$$

Where $\sigma_j > 0$ is the elasticity of substitution between K and L in the production of the jth good, $\lambda^* \equiv \lambda_{Lx} - \lambda_{Kx}$, $\lambda_{ij} = c_{ij}/i_0$ the share of factor i employed in industry j, and $a_j \equiv \theta_{Kj}\lambda_{Lj} + \theta_{Lj}\lambda_{Kj} > 0$.

Equations (16) - (18) constitute a three-equation general equilibrium model with three unknowns: $\hat{X}-\hat{Y}$, $\hat{q}_x-\hat{q}_y$ and $\hat{r}$. By only considering a partial tax on capital in the corporate sector T_{Kx} ($\hat{T}_x=\hat{T}_y=\hat{T}_{Ky}=0$) and assuming as is usually the case in general equilibrium models of corporate taxation that there was no capital tax in the corporate sector initially, the impact of the tax is described by the following system of equations:

$$\hat{r}D^* = (\theta_{Kx}\lambda^* - a_x\sigma_x)\hat{T}_{Kx} \quad \text{(B19)}$$

$$(\hat{X}-\hat{Y})D^* = -(\theta_{Ky}a_x\sigma_x + \theta_{Kx}a_y\sigma_y + a_x\sigma_x\Psi)\hat{T}_{Kx} \quad \text{(B20)}$$

$$(\hat{q}_x-\hat{q}_y)D^* = (\theta_{Ky}a_x\sigma_x + \theta_{Kx}a_y\sigma_y + a_x\sigma_x\Psi)\hat{T}_{Kx} \quad \text{(B21)}$$

Where $D^* \equiv a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)$ which must be positive for stability according to Davidson and Martin. Equations (19), (20) et (21) highlight the effects of the corporate tax respectively on relative factor prices, relative outputs and relative output prices. As described earlier, the tax elasticity could be divided into an output et a substitution effect.

The first term on the right-hand side of equation (19) relates to the output effect and varies with factor intensity in the corporate sector. If $\lambda^* > 0$, the corporate sector is labor intensive and the resulting decrease in output in the corporate industry depresses the relative demand for labor featuring an increase in the relative return of capital. Alternatively, if $\lambda^* < 0$ the output effect pushes down the relative return of capital. The second term on the right-hand side of equation (19) describes the substitution effect which is unambiguously negative as the capital escape from the corporate sector triggered by the tax lowers the net returns to capital.

- Comparative Statistics

Equation (19) indicates that the elasticity of the relative returns to factors with respect to the partial tax in the corporate sector X depends on the number of firms making up the collusive agreement.

The magnitude of the effect of imperfect competition on the elasticities depends on the relative factor intensities in production. From equation (19), it follows that the sensitivity of factor relative returns with respect to the cartel pricing term $\Psi = 4rN/[(rN)^2 - 1]$ is given by:

$$\frac{d\left[\frac{\hat{r}}{\hat{T}_{Kx}}\right]}{d\Psi} = -\frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)}{D^{*2}}\lambda^* \tag{B22}$$

As expected the elasticity depends on the cartel collusive agreement which in turn is affected by the number of firms operating in the corporate sector. Keeping in mind that wages are used as numeraire, and rewriting equation (19) by exploiting basic mathematical relationships between percent changes and log operators, we derive the following:

$$\begin{aligned}
&\left[\frac{\hat{r}}{w}\right] = \frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)\hat{T}_{Kx}}{a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)} \Leftrightarrow \\
&d\ln\left[\frac{r}{w}\right] = \frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)d\ln T_{Kx}}{a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)} \Leftrightarrow \\
&d\ln r - d\ln w = \frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)d\ln T_{Kx}}{a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)} \Leftrightarrow \\
&d\ln w = -\frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)d\ln T_{Kx}}{a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)} + d\ln r
\end{aligned} \tag{B23}$$

By denoting initial equilibrium wages, capital returns and tax rates as $\overline{w}$, $\overline{r}$ and $\overline{T}_{Kx}$ in addition to assuming that there is no capital tax before the introduction of the corporate tax ($\overline{T}_{Kx} = 1$) then equation (23) becomes:

$$\ln w = -\frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)\ln T_{Kx}}{a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)} + \ln r + K \tag{B24}$$

where $K = \ln\overline{w} - \ln\overline{r}$ measures the initial difference in equilibrium factor prices before the introduction of the tax. We can also notice from equation (24) that the difference between the

factor price elasticities $\eta_{w\tau} - \eta_{r\tau}$ with respect to the corporate tax depends on the relative factor intensities between the two sectors as well as the corporate market structure:

$$\eta_{w\tau} - \eta_{r\tau} = -\frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)}{a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)} \tag{B25}$$

Based on equation (25), we derive some useful insights. If $\lambda^* > 0$, meaning the corporate sector is more labor intensive, then the right-hand side in equation (25) is negative implying that the wage sensitivity to the corporate tax is much smaller than the capital returns sensitivity ($\eta_{w\tau} < \eta_{r\tau}$). Additionally, we know that in traditional neoclassical closed-economy two-sector corporate taxation models, the tax reduces net returns to capital due to capital mobility across sectors. In open economy models, it is usually assumed that the domestic economy does not influence international capital returns rates. By focusing essentially on U.S contiguous counties, we will expect the open economy predictions to hold, that is we do not think that a state corporate tax reform is important enough to influence the net of tax capital returns in the U.S economy ($\eta_{r\tau} \simeq 0$). As a result, we get:

$$\eta_{w\tau} \simeq -\frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)}{a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)} \tag{B26}$$

We could also analyze the impact the corporate sectoral composition has on the wage elasticity of the capital tax. Differentiating the elasticity expression in equation (26) with respect to the number of businesses operating in the corporate sector yields:

$$\frac{d\eta_{w\tau}}{dN} \simeq \frac{(\theta_{Kx}\lambda^* - a_x\sigma_x)\lambda^*}{[a_x\sigma_x + a_y\sigma_y + \lambda^*(\theta^* + \Psi)]^2}\frac{d\Psi}{dN} \tag{B27}$$

Since we know that:

$$\frac{d\Psi}{dN} = \frac{-4r(1+r^2N^2)}{[(rN)^2-1]^2} < 0 \qquad \text{(B28)}$$

Equation (28) suggests that the elasticity of wages with respect to the tax is decreasing in the number of firms in the corporate sector as long as this sector is labor intensive or alternatively if the corporate sector is capital intensive and the output effect dominates the factor substitution effect. In fact, even though we assumed that the tax leaves the net capital returns unchanged, the after-tax returns to capital which is used by the cartel as a discount rate shrinks. This enables the corporate cartel to sustain the collusive agreement while lowering output per firm. These oligopolistic forces further reinforce the output effect and considering that the corporate sector is labor intensive, the labor factor shares a higher burden of the tax inefficiency. Also, for any given decrease in the discount rate (after-tax capital rate of return), the higher the number of firms operating in the corporate sector, the smaller is output and profits per firm. This in turn lowers the ability of the cartel to enforce the collusive agreement without increasing output per firm. As a consequence, the cartel pushes up output which given that the corporate sector is labor intensive drives up the relative demand of labor thereby reducing the wage responsiveness to the tax.

B.2.3 Chapter 2 Additional Content 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 ∈ {1, 2, ...,G}, as well as to a cluster h ∈ {1, 2, ...,H}, and we have:

$$y_{igh} = \mathrm{x}'_{igh}\beta + u_{igh} \tag{C1}$$

where we assume that for $i \neq j$ $\mathrm{E}[u_{igh}u_{jg'h'} \mid \mathrm{x}_{igh}, \mathrm{x}_{jg'h'}] = 0$ 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 $\Omega = \mathrm{V}[\mathrm{u}|\mathrm{X}]$ 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:

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

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

S^{H} is an N × N indicator matrix with ijth entry equal to one if the ith and jth observation belong to the same cluster h ∈ {1, 2,...,H}, S^{G} denotes a similar N × N indicator matrix with ijth entry equal to one if the ith and jth observation belong to the same cluster g ∈ {1, 2,...,G}, and $\mathrm{S}^{G\cap H}$ refers to an N × N indicator matrix with ijth entry equal to one if the ith and jth observation belong to both the same cluster g ∈ {1, 2,...,G} and the same cluster h ∈ {1, 2, ...,H}.

B.2.4 Chapter 2 Additional Content Extensive review of the literature of corporate tax incidence

- Theoretical background

Harberger's groundbreaking contribution (1962) remains a valuable reference for the neoclassical analysis of the incidence of the corporate income tax. In this benchmark model, the corporate tax acts as a partial factor tax on capital in the corporate sector. Assuming a fixed stock of Labor and Capital, Harberger examined the effects of the tax in a closed economy with two sectors: one corporate and one non-corporate and concluded that the distortions of the corporate tax depend on five (05) parameters: (1) the degree of capital mobility across sectors, (2) the elasticity of substitution between capital and labor in production, (3) the elasticity of substitution between products in consumption, (4) the factor intensities in production and (5) the elasticity of demand with regards to the corporate output. He also calibrated the model for the US economy using reasonable estimates for the above-mentioned parameters and found that capital bears the full burden of the corporate tax in a closed economy.

Raveendra (1975) built on Harberger's original set-up by introducing uncertainty related to the product of the taxed sector into the analysis. Similar to Harberger, he uses a closed economy two-sector general equilibrium model but assumes that the level of output in the corporate sector is subject to random shocks. Supposing that firms maximize expected utility of profits, he concluded that Harberger's findings remain valid under uncertainty only if the relative and the absolute risk aversion of firms are non-increasing in profits. In the face of exogenous supply shocks that affect production decisions, Raveendra's insight provides a more comprehensive description of the corporate income tax effects.

Ballentine (1976) and Atkinson et al. (1980) extend the Harberger model to a monopolistically competitive corporate industry. However, Davidson and Martin (1985) were the first to expose a general equilibrium corporate tax model that features a set up with an oligopolistic corporate sector. Considering that firms operate within an infinitely repeated Cournot game, the authors suggest that a tacit collusion could emerge, whereby each firm produces below the perfectly competitive quantity, resulting in a higher labor share of the deadweight loss generated by the tax.

Besides, over the last decades, capital control instruments have been considerably dismantled across the globe and the subsequent increase in international flows of assets, goods and services is suggestive of the necessity to use an open-economy framework in order to fully account for the distortions arising from corporate taxes. Grubert and Mutti (1985) were the first to adopt such an approach by constructing an open economy general equilibrium model with three sectors (an exporting sector, an importing sector, and a non-traded sector), two trading partners (US and the rest of the world) and three factors (capital, skilled labor, and unskilled labor). Assuming that the traded commodity is imperfectly substitutable internationally they found in the case of perfect capital mobility that only 14% of the tax burden falls on domestic capital. The share borne by labor tends to fall with the degree of international capital mobility.

Similar to Grubert and Mutti, Gravelle and Smetters (2006) used an open economy model with two trading partners (US and the rest of the world), four sectors (traded and non-traded corporate and traded and non-traded non-corporate) and two factors of production (labor and capital). Different alternatives of international capital mobility and international product substitution were considered. Conditional on capital and products being fully substitutable

internationally, the authors estimate the burden of the corporate income that falls on domestic capital to 35%, which is considerably higher than the estimates of Grubert and Mutti.

Randolph (2006) also considers an open economy with two partners, five sectors and assumed that capital is perfectly mobile across countries. Like other open economy general equilibrium models of corporate taxation, the author used a fixed worldwide stock of labor and capital to conclude that capital owners worldwide cannot escape the tax. Since the world capital stock is assumed fixed, domestic owners of capital will shift the burden of the tax to others by moving assets to the rest of the world. Using plausible numerical values for the appropriate elasticities for the US economy, Randolph suggested that labor bears 70% of the burden of the tax with capital bearing the remaining 30%.

Bhatia (1979) built a framework that features an intermediate input. Supposing that firms use capital, labor and an intermediate good produced separately, he concludes that the main findings of the original Harberger contribution are slightly altered once one allows for capital to be used in the production of the intermediate good. He also defends that the greater is the elasticity of substitution between labor and capital in the untaxed industry, the more likely it is that these factors will bear the tax burden in proportion to their initial shares of national income. To date, Bhatia's analysis is the only work that touches on the potential role of the separation of different stages of production, in analyzing the inefficiencies generated by a corporate tax. However, Bhatia used a closed-economy model which is at odds with recent developments in outsourcing activities across the globe.

Jennifer Gravelle (2008) undertakes a review of the literature on open economy general equilibrium models of corporate taxation and highlights that the key drivers of the incidence of the tax on factor returns relate to the specification of a number of elasticity parameters. By comparing

four major studies, she defends that the outstanding differences in the burden share attributed to each factor could be explained by the elasticity assumptions made by the authors. Gravelle also surveys the literature on the appropriate elasticities and concludes using the central empirical estimates of those elasticities that capital bears the majority of the burden of the tax.

In sum, the theoretical research on the incidence of the corporate tax is all but settled. The suggested burden shares are critically dependent upon the assumptions made with regards to a set of elasticity parameters, as well as the sectoral break-up adopted. In the presence of such notable uncertainties, one is forced to rely upon the empirical literature to shed light on some of the ambiguities exposed by the general equilibrium approach.

- Empirical works

Given the limitations of the theoretical general equilibrium research on the distributional impact of the corporate tax, a growing body of recent works on the topic have investigated the empirical relationship between factor returns and capital taxation. These contributions could be divided in two broad categories. The first strand of the empirical research builds on the neoclassical tradition and focuses primarily on the so-called direct effect of the taxation. This relates to long-run changes in investment, employment and wage patterns following corporate tax changes.

The first extensive empirical work trying to study the interdependence between corporate taxes and capital per worker was made by Hassett and Hubbard (1996) who analyzed investment patterns around corporate tax reforms in 14 OECD countries[16]. The authors exploited firm-level data to explore the extent to which changes in the corporate tax legislation alters fixed capital formation by enterprises and found evidence of significant responses of investment to the fiscal

[16] This study features separate regressions for each country

treatment of capital. In so doing, the authors treated corporate tax reforms as natural experiments to better identify the responsiveness of investments to movements in the user cost of capital. As suggested above, treating corporate tax reforms as exogenous policy instruments does not necessarily take into consideration the incentives of public policy, given that tax reforms could be motivated by the business cycle which has been extensively proven to influence corporate investment.

Similarly, Laura Vartia (2008) examined the effects of corporate taxes on investment using a panel of 41 European countries observed over the period 1998-2004. By exploiting industry-level data, the paper suggests that tax-related changes of the user cost of capital have an adverse effect on investment. The long-run elasticity of the investment-to-capital ratio with respect to the tax ranges from -0.35 to -1.0 depending on the empirical specification. The author employs a country-industry dynamic fixed effect model, explaining the natural log of the investment to capital ratio with the user cost of capital, the natural log of the lagged value of the investment to capital ratio and several other determinants. The empirical approach adopted in this paper bears apparently some methodological limitations. Using a panel of industries with industry-country individual effects does not control for the unobserved time variant heterogeneity between industries. Additionally, the author did not cluster the error terms at the industry and country levels, which could potentially underestimate the variance of the standard errors.

Djankov et al. (2006) using new data on effective corporate income taxation in 85 countries found that the effective corporate tax rate has a negative effect on investment. The paper exploited a database newly constructed by PricewaterhouseCoopers which surveys all taxes imposed on "the same" standardized mid-size domestic firm across 85 countries in 2004. The authors concluded that a 10 percent increase in the effective corporate tax rate reduces the aggregate investment to

GDP ratio by 2 percentage points. They also suggested that corporate tax rates are correlated with investment in manufacturing but not in services, as well as with the size of the informal economy. Even though this research exploits an extensive cross-section of countries, the authors were not able to utilize any time series variation due to the limitation of the data structure.

Other empirical studies within the same neoclassical line of approach consider rather the extent to which the tax acts as a deterrent to wages and employment. Hassett and Mathur (2010) using a panel of manufacturing data across 65 countries over 25 years concluded that wages are highly sensitive to changes in the corporate tax rate. The paper adopts a fixed effect model with a five-year average value of the natural log of wages as the dependent variable and the corporate tax rate along with several control variables as independent. The mechanics of the tax incidence as described by the study relates to changes in capital per worker due to the variations in investment arising in the aftermath of a tax reform, yet the authors control for the value added per capita in manufacturing in their regression which casts some interrogations on the nature of the derived elasticities.

Carroll (2009) looks into the experience within the United States, rather than internationally, by exploiting state-level variation in corporate taxes between 1970 and 2007. He finds a negative relationship between corporate tax rates and the real hourly average earnings for production workers. A one percent increase in the average state and local corporate tax rate lowers workers' real wages by 0.014 percent. The author used state and time fixed effects to control for individual and temporal unobserved heterogeneity. Nonetheless, if wage and employment dynamics feature different patterns across states irrespective of the corporate tax rate, the computed elasticity could be biased. State corporate tax changes are motivated by a number of factors, including the business cycle which in turn relates to employment fluctuations. The

potential reverse causality between corporate tax legislation and employment and wages warrants the adoption of an appropriate identification strategy.

Ljungqvist and Smolyansky (2014) addressed that limitation by considering contiguous county groups that straddle state borderlines. Using corporate tax changes across U.S. states between 1970 and 2010, the authors compare employment and income dynamics between counties which share similar economic conditions and are likely to serve as appropriate controls in the face of legislation changes affecting investment on either side of the border. It is found that corporate tax cuts boost employment when implemented during recessions. Even though the approach adopted by the authors addresses the potential reverse causality between taxes on the one side and employment and income on the other side, the mechanics according to which the tax impacts labor are not clearly identified. The dual mobility of labor and capital across contiguous counties, suggests that in the long-run, the extent of the tax incidence related to investment changes may not be as severe in this set-up. The bulk of the burden measured in this study is more likely inherent to imperfect competition or market power, in which case the economic forces at play are governed by a different school of the theoretical literature.

The other approach prevalent among empirical studies focuses on the importance of market failures or imperfections in the distribution of the burden of the corporate tax. This builds on a rather heterodox tradition which emphasizes the role played by labor and product market institutions in the analysis of tax incidence. It is important to note that these studies do not measure the classical general equilibrium effects of the corporate tax on wages through investment. Instead they seek to evaluate the impact of the tax that is attributable to the bargaining between workers and firm owners over economic rents or the existence of highly concentrated productive sectors that enable the shifting of the tax to consumers and workers.

A pioneering contribution in this line of thought was made by Arulampalam et al. (2010) who investigate the possibility that corporations shift the tax to employees through the wage bargaining process. Exploiting an extensive database of more than 500,000 firms in nine European countries observed over the period 1996-2003, the authors measure the effect of corporate income taxes paid by firms on employee compensation. Controlling for productivity through the value-added per employee, they argued that a one-dollar increase in the tax bill tends to reduce the median real wage by 49 cents.

Finally, Felix and Hines (2009) addressed a similar question using individual data for the year 2000 across fifty U.S. states. The paper notes that high tax states feature lower union wage premium. Specifically, it is argued that wages decrease by 54 cents for every one-dollar increase in the corporate tax bill, implying that workers share about half of the burden of the corporate tax. Unfortunately, the paper could not adequately handle unobserved time-invariant heterogeneity at the state level since only one year of data is analyzed.

APPENDIX C:

Chapter 3 Additional Content

C.1: Chapter 3 Additional Content Tables

Table 3.1.1: Main results: Effects on wages paid by large firms

	(1)	**(2)**	**(3)**	**(4)**
Ohio indicator	0.04***	0.05***	0.02***	-0.11***
	(0.01)	(0.01)	(0.003)	(0.04)
2006-2010 indicator	-0.02***	-0.03***	-0.12***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)
2011-2015 indicator	-0.02***	-0.05***	-0.13***	-0.06*
	(0.01)	(0.01)	(0.01)	(0.01)
Ohio x 2006-2010 indicator	0.01	0.01	0.01*	0.13***
	(0.02)	(0.02)	(0.006)	(0.06)
Ohio x 2011-2015 indicator	-0.05***	-0.05***	-0.02**	0.01
	(0.01)	(0.01)	(0.01)	(0.09)
State fixed effects	No	Yes	No	No
Region-year effects	No	No	Yes	No
Regional time trends	No	No	No	Yes
All sociodemographic controls (*)	Yes	Yes	Yes	Yes

Notes: the dependent variable is the log of earnings for individuals between the ages 25-64. Standard errors are in parentheses. (*) Sociodemographic controls include gender, marital status, race, experience, education, the metropolitan status of residence, and occupation.

Table 3.1.2: Main results: Effects with inclusion of state policy changes

	WAGES	EMPLOYMENT	LABOR FORCE PARTICIPATION
Ohio indicator	0.02	-0.02	0.02
	(0.02)	(0.01)	(0.02)
2006-2010 indicator	-0.03*	-0.04***	-0.03*
	(0.02)	(0.01)	(0.02)
2011-2015 indicator	-0.02*	-0.01*	-0.01
	(0.01)	(0.01)	(0.01)
Ohio x 2006-2010 indicator	-0.03	0.02	0.04
	(0.03)	(0.02)	(0.03)
Ohio x 2011-2015 indicator	-0.08***	-0.02***	-0.01
	(0.01)	(0.01)	(0.01)
State fixed effects	Yes	Yes	Yes
All sociodemographic controls (*)	Yes	Yes	Yes

Notes: Standard errors are in parentheses. (*) Sociodemographic controls include gender, marital status, race, experience, education and the metropolitan status of residence. The regression controls for state policies such as changes in income tax rate, investment tax credits and sales tax rate.

Table 3.1.3: Description statistics

	MEAN	S.D.
Age	44	11
Male (%)	49.9	50.0
White (%)	80.9	39.3
Married (%)	65.4	47.6
Years of schooling	14	3
In the labor force (%)	79.8	40.1
Employed (%)	94.7	22.4
Self-employed (%)	12.0	32.5
Living in a metropolitan area (%)	76.1	42.6
With a wage and salary employment (%)	67.8	46.7
Number of children	1.1	1.2
Number of children less than 5 years old	0.2	0.5
Wage earnings ($)	43,412	50,244
Number of Obs.	1,667,068	

Table 3.1.4: Main results: Wage effects on female sub-sample

	(1)	(2)	(3)	(4)	(4)
Ohio indicator	0.05***	0.04	0.05	-0.11***	-0.04***
	(0.02)	(0.07)	(0.05)	(0.04)	(0.01)
2006-2010 indicator	0.06*	-0.05	0.01	-0.03***	-0.006
	(0.03)	(0.04)	(0.01)	(0.01)	(0.04)
2011-2015 indicator	0.02	-0.14	0.04**	-0.06*	-0.01
	(0.02)	(0.10)	(0.02)	(0.01)	(0.02)
Ohio x 2006-2010 indicator	-0.08***	0.006	-0.04	0.13***	0.01
	(0.02)	(0.05)	(0.03)	(0.06)	(0.01)
Ohio x 2011-2015 indicator	-0.07***	0.05	-0.15***	0.01	-0.02
	(0.03)	(010)	(0.05)	(0.09)	(0.03)
State fixed effects	Yes	No	No	No	No
State linear trend effects	No	Yes	Yes	Yes	No
State linear+quadratic+cubic trends	No	No	Yes	No	No
Region year fixed effects	No	No	Yes	No	Yes
All sociodemographic controls (*)	Yes	Yes	Yes	Yes	Yes

Notes: the dependent variable is the log wages for individuals between the ages 25-64. Standard errors are in parentheses. (*) Sociodemographic controls include gender, marital status, race, experience, education, the metropolitan status of residence, and occupation. The regression controls for state policies such as changes in income tax rate, investment tax credits and sales tax rate.

Table 3.1.5: Main results: Employment effects on female sub-sample

	(1)	(2)	(3)	(4)
Ohio indicator	0.003	0.001	0.004	0.004
	(0.005)	(0.002)	(0.004)	(0.004)
2006-2010 indicator	-0.02***	-0.06***	-0.02	-0.04***
	(0.002)	(0.01)	(0.002)	(0.003)
2011-2015 indicator	-0.02***	-0.04***	-0.02***	-0.03***
	(0.001)	(0.01)	(0.001)	(0.02)
Ohio x 2006-2010 indicator	-0.002	0.003	-0.004	-0.004
	(0.007)	(0.003)	(0.007)	(0.003)
Ohio x 2011-2015 indicator	0.002	0.04*	-0.002	0.002
	(0.004)	(0.02)	(0.004)	(0.002)
State fixed effects	Yes	No	No	Yes
Region-year effects	No	Yes	No	No
Regional fixed effects	No	No	Yes	No
Year fixed effects	No	No	No	Yes
All sociodemographic controls (*)	Yes	Yes	Yes	Yes

Notes: the dependent variable is a dummy variable for employment status for individuals between the ages 25-64. Standard errors are in parentheses. (*) Sociodemographic controls include, marital status, race, experience, education, the metropolitan status of residence, non-work income and number of children less than 5 years old.

C.2: Chapter 3 Additional Content Figures

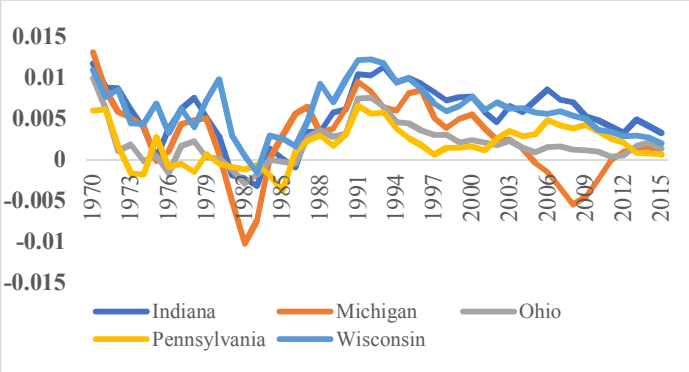

Figure 3.2.1: Growth of personal income in Midwestern states

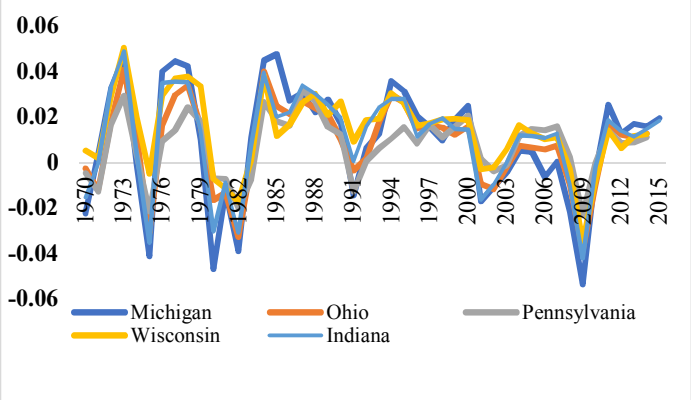

Figure 3.2.2: Growth of employment in Midwestern states

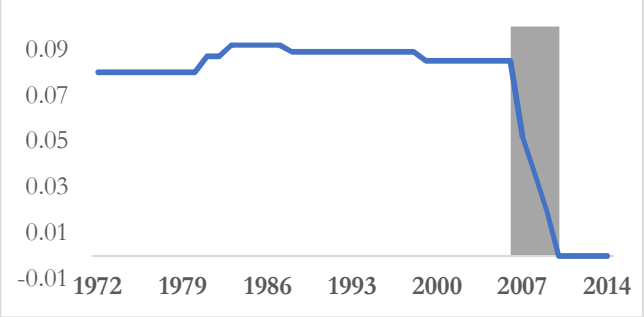

Figure 3.2.3: Corporate Franchise Tax (CFT) in Ohio

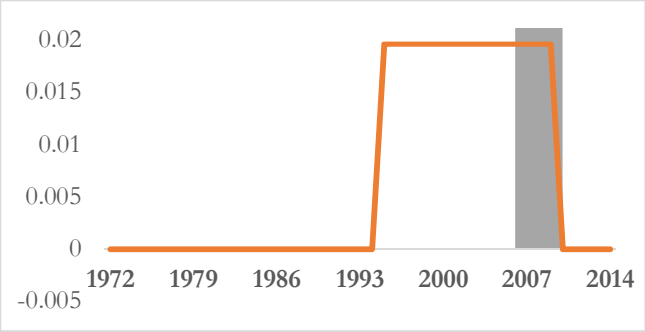

Figure 3.2.4: Investment tax credits in Ohio

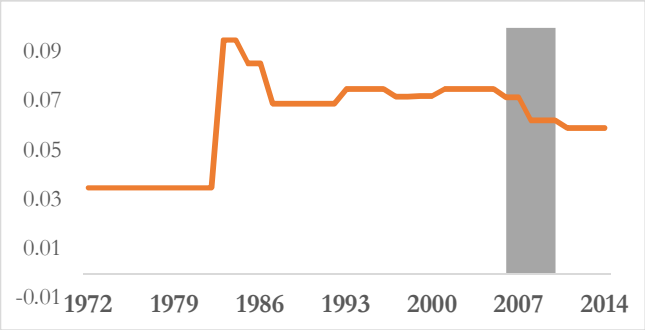

Figure 3.2.5: Personal Income Tax in Ohio