Do stock prices respond to changes in corporate income tax rates?*

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Abstract

For public corporations, changes in corporate tax rates may significantly alter future earnings available to shareholders. When these rates are changed, markets should adjust stock prices to reflect the change in capitalization value of future earnings. State corporate income tax rate changes are heterogeneous across states and time. Using stocks where the state-by-state allocation of income can be determined, I examine price responses around the timing of these tax rate changes using event study and differences-in-differences frameworks. Overall, changes in stock prices are much greater than would be expected if profits changed by the full amount of the tax, but this is driven almost entirely by tax increases. These results suggest effects from interstate competition between companies as well as competition between C corporations and pass-through entities. I also find that the timing of these price changes differ for tax cuts and increases, with lawmakers signaling their intentions sooner for tax cuts.

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1 Introduction

For the last few decades, Federal and state lawmakers in the United States have paid significant attention to improving the efficacy of the corporate income tax. Gravelle (2017) enumerates several public policy examinations at the Federal level, including the 2005 Advisory Panel on Tax Reform, several opinion pieces by policy makers, a 2007 Treasury Department background paper, and numerous bills introduced in Congress. A similar level of examination has occurred at the state level.[1] Though there are numerous issues complicating the debate over the corporate tax, in this paper, I examine the impacts of corporate tax changes on shareholders.

The incidence of the corporate tax is notoriously difficult to predict. Depending on model selection, choice of functional forms within models, and model parameterization, theory gives wildly different results for the incidence of the corporate tax, ranging from more than one hundred percent to less than zero percent of the burden falling on shareholders (Harberger 1962, Auerbach 2006, Harberger 2006). Empirical results are also far from conclusive, with a wide variety of incidence rates. Estimates the incidence on labor range from near zero to up to two thousand percent.[2] A minority of corporate tax incidence studies estimate the incidence on capital (examples include Cragg, Harberger, and Mieszkowski (1967), who estimate that capital bears close to the full burden of the tax, and Desai, Foley, and Hines (2007), who estimate that capital bears between twenty-five and fifty-five percent of the tax).

In this paper, I try to resolve the existing contradictory empirical results by providing evidence from stock markets. Taking the stance that stock markets efficiently integrate publicly available information into stock prices, stock price

[1] Most recently in Iowa, where state corporate tax rates were overhauled (Bloomberg BNA 2018).

[2] (For discussions see Desai, Foley, and Hines (2007) and Dwenger, Rattenhuber, and Steiner (2011)).

changes that accompany corporate tax rate changes should inform us about the value of the burden falling on shareholders.

Estimating stock price changes caused by changes in tax rates at the federal level can be difficult, as the federal tax code often changes all at once in infrequent, major overhauls, so the effects of a particular tax change among many simultaneous changes are difficult to tease out. Cross-country comparisons are challenging because of the political and cultural heterogeneity of the underlying countries. What is needed is a set of reasonably similar jurisdictions, a set of corporate tax rate changes that vary across time and jurisdictions, and a set of stocks of comparable companies where the jurisdiction-by-jurisdiction apportionment of income can be ascertained.

I examine the effects of changes in state-level corporate tax rates on the value of stocks listed on the major United States stock exchanges. For most US equities, determining the exact state-by-state breakdown of corporate income is at best a noisy task, as tax returns of publicly traded corporations are not publicly available information, and proprietary sources of corporate data may not have sufficient data to properly allocate income across states. Fortunately, there is a subset of stocks that permit accurate state-by-state allocation: regional bank stocks. These stocks fulfill a number of desirable characteristics for this exercise: the companies are often completely located within one or a handful of states, corporate taxes represent a large proportion of their net profits, their branch units are comparable within companies, and operations of different companies are similar. I calculate the percentage of each firm's income allocated to each state for tax purposes. Using the set of state-level corporate tax rate changes from 1994-2017, I examine the change in stock valuation of these companies around the public unveilings of corporate tax rate changes, first using financial event studies, then in a differences-in-differences (DiD) regression framework.

In the event studies, I regress stock returns on market returns and other factors over a time span well before the events of a tax change, creating a model of how the stock returns behave. Then I use the deviation of stock prices from the model's prediction around the events of the tax change to establish the stock's abnormal returns. In the DiD regressions, I regress logarithmic changes in stock prices on log tax changes and a variety of controls and fixed effects to determine the percent change in stock price induced by a percent change in corporate tax rates. I augment the basic DiD regressions with other regression techniques that are more robust to outliers.

I find that the timings of these stock valuation changes differ for tax rate decreases and tax rate increases. For tax decreases, most of the associated stock price increases occur well before the corresponding new tax bill is introduced, and mostly disappear as the bill becomes law. On the other hand, most of the price decreases due to tax increases only materialize when the bill is finally introduced. This suggests that lawmakers reap benefits from signalling tax decreases but not increases.

In the case of tax increases, the DiD results imply that far more than one hundred percent of the tax falls on shareholders, approximately a ten percent stock price decrease for a one percentage point tax rate increase. The results for tax decreases imply about a one to two percent increase in stock value per one percent decrease in tax rate, but these results are far noisier. These large results can be explained by a combination of interstate competition between banks, as well as competition between C corporations (those companies directly affected by the new tax rates) and S corporations (those companies that do not pay the corporate tax). In the case of tax decreases, executive management teams may decide to reap the benefits of lower tax expenses as executive compensation rather than passing the benefits along to shareholders. However, some placebo tests provide evidence that treatment coefficient estimates may be somewhat biased.

These results must be taken with some caveats, as even the most basic corporate tax incidence models show that differing production functions yield meaningfully different results (Harberger 1962), and banks may not be representative of publicly traded corporations in other industries. For example, these companies are highly levered relative to the stock market as a whole.[3] However, banking is an industry tied to basically all of the other industries, so these results may be more externally valid than those of many single-industry studies. This work contributes to the financial event study literature, by augmenting event studies with a more causal DiD framework when a large set of geographically and temporally heterogeneous changes permits.

The next section provides a review of related literature. After that, I describe data used in this study while providing institutional details relevant to the data. I then describe the methodologies used and present results for both the event studies and DiD regressions, including placebo tests for both. I discuss reasons for the timing and magnitude of results. Finally, I conclude, commenting on avenues for future research.

2 Related Literature

This research combines literatures in public finance and financial economics. First, it draws from an old but ever controversial literature on the incidence of the corporate tax. As mentioned, conclusions about the incidence of the corporate tax are far from settled. Even in Harberger's (1962) seminal paper, small changes in modeling assumptions drastically change results, from all of

[3] A quick analysis of debt ratios of the Dow Jones Industrial Average components (sans financial components) versus an equally sized sample of regional bank stocks, taken at the midpoint of the sample, shows that the bank stocks have total debt-to-equity ratios that are, on average, approximately six times higher than the Dow components.

the burden falling on capital to most of the burden falling on labor. It should be noted that Harberger's baseline result has the entire burden falling on *all* capital, not just corporate capital. A more modern review of the theoretical literature can be found in Auerbach (2006). Of note, Harberger's (1962) closed economy results reverse in an open economy model, with all of the burden falling on labor. An important common feature of most models is that they describe long run phenomena and ignore short run effects.

On the empirical side, most research focuses on the incidence of the corporate tax on labor. This is at least partly due to the fact that owners of corporate capital tend to be on the upper end of the income distribution, whereas the typical laborer is not. Thus, determining incidence has great importance in terms of the progressivity of the corporate tax. Examples of these labor-focused studies include Dwenger, Rattenhuber, and Steiner (2011), who find an elasticity of wage rates to tax rates of -2.37, Arulampalam, Devereux, and Maffini (2012), who find an elasticity of -0.92, and Hasset and Mathur (2010), who find an elasticity of -0.5 to -0.6. Early studies on the incidence on capital include Krzyzaniak and Musgrave (1963), who find that capital can benefit from a tax, and Cragg, Harberger, and Mieszkowski's (1967) rebuttal, which reverses the 1963 results. A more recent study is Desai, Foley, and Hines (2007), who estimate the relative burden on labor and capital by assuming that they sum to unity, and find that twenty-five to fifty-five percent of the burden falls on capital. Gordon (1985) finds small benefits of the corporate tax on investment. In addition, Gravelle (2017) gives a thorough summary of other studies relating to the corporate income tax (Laffer curve, investment, etc.) as well as describing common econometric issues that arise in studying the corporate income tax.

This paper adds to a newer, burgeoning body of literature that relies on changes to state corporate tax rates in the United States as a source of variation.

An early example is Feldstein and Poterba (1980), who find that an omission of state and local taxes understates the rate of return to capital. More recent examples include Felix and Hines (2009), who show that unionized workers benefit by capturing approximately half of the benefit of lower state corporate tax rates, Giroud and Rauh (2015), who find that employment and number of establishments both have state corporate tax elasticities of approximately -0.4, Heider and Ljungqvist (2015), who find that the use of leverage has a state corporate tax elasticity of about 0.4. In addition, Ljungqvist and Smolyansky (2014) find the asymmetric result that tax increases decrease employment and firm income but tax decreases do little, Ljungqvist Zhang and Zou (2015) find another asymmetry in that tax increases reduce firm risk-taking while tax cuts do little, and Serrato and Zidar (2017) find that the narrowing of states' corporate tax bases over time reduce states' ability to raise revenue through rate increases. Important themes in this literature include the use of tax changes, not levels, as a source of variation (implored for by Auerbach (2006)), leveraging the vast heterogeneity of these changes over space and time, and noting asymmetric results stemming from tax increases versus decreases.

Important for this research, Giroud and Rauh (2015) find that most state corporate tax changes are exogenous with respect to the income of individual firms, following a narrative method of categorizing tax changes as more or less exogenous according to Romer and Romer (2010). This finding is supported by Ljungqvist and Smolyansky (2014), who note that corporate tax revenues typically account for only a small portion of state revenues. The corporate tax may be of second order consideration for closing budget deficits, thus these changes may tend to be more exogenous.

Previous studies differ from this paper in that they focus on non-financial capital. Many states have different corporate tax rates on financial and non-financial institutions. To my knowledge, this is the first paper to focus mainly on the effects of changes in state corporate tax rates on financial institutions. As a result, some of the data look slightly different, and some previous conclusions, such as the inference that state corporate tax rates follow a random walk (Ljungqvist and Smolyansky (2014)) must be revisited. Previous studies also look at firm behavior as rate changes come into effect, whereas this study looks at the market's response to rate changes as the laws are announced and made, well before they come into effect (or sometimes well after, in the case of retroactive law changes).

This research relies heavily on the efficient markets hypothesis (EMH), related to the work of Samuelson (1965), developed by Fama (1965, 1969, 1970), and made famous in the popular press by Malkiel (1973). Fama (1970) developed the notion of the semi-strong form of the EMH, which states that stock prices reflect all publicly available information. Lo (2005) enumerates how much of widely accepted modern financial economics is derived from the EMH. Although non-behavioral critiques of the EMH exist (see, for example, Buffett (1984)), the majority of EMH criticisms come from the behavioral finance camp (a summary of these criticisms can be found in Lo (2005)). Malkiel (2003) rebuts these criticisms by noting that many of the most famous behavioral exceptions, such as the January effect, disappear nearly as soon as they are discovered. Lo (2005) has an interesting approach to this debate, reconciling the EMH and behavioral anomalies by combining the two camps into an imperfect but rational evolutionary process in which rational stock trading strategies are learned over time and respond to changing market conditions. This paper takes the stance that the major US stock markets are imperfectly efficient in the semi-strong sense, but efficient enough to capture the impacts of publicly available information in a reasonable amount of time (say, a few days).

News of changes to a state's corporate tax code may not register with investors immediately if they are not constantly keeping abreast of changes in state laws. Given that, in any one state's legislative session, thousands of bills may be introduced, the passage of one of many proposed tax law changes may slip investors' notice for a short period of time. This relates to the work of Chetty, Looney, and Kroft (1989), who show how purchasers may ignore important tax information if such information is not salient.

Finally, this paper relates to an ongoing debate about the transparency of publicly traded companies' tax information. Currently, the tax returns of publicly traded companies in the United States are not made publicly available. This paper makes use of company data where corporations' business establishment locations are identifiable using publicly available information; in most cases they are not. This point is debated, for example in Lenter, Slemrod, and Shackelford (2003). While the debate is complicated (for example, full transparency may lead to companies publishing lower quality information), this paper suggests that persons who have detailed information about the state-by-state allocations of corporations' incomes may have a trading advantage over other investors.

3 Data and Institutional Details

The goal of this paper is to show how changes in state corporate tax rates effect stock returns; thus, data must consist of a sample of stock returns and a schedule of state corporate tax law changes. For years 1994-2017, I look at changes to states' top marginal corporate tax rates, as these types of law changes are most comparable across states ("states" refers to the 50 states plus the District of Columbia).[4] Table 1, which combines data culled from *The Book of the States* as

[4]These top rate changes include changes to surtaxes, which alter the top rates *effectively* paid by corporations. I only look at top rates, even in the case of states with multiple brackets, for simplicity. First, most states only have one rate. States with multiple brackets have top

well as state websites, provides information on the levels of the top marginal tax rates on C corporations for years 1994-2017 for the 51 "states" in the sample. Of note, states exhibit wide heterogeneity in rates throughout the sample, and over time, there is a trend towards lower tax rates. Also of note, some states have different corporate tax rates for financial institutions. From this point onward, I ignore states with a corporate tax base that is not based on net income (for example, states that tax gross receipts instead).[5]

Corporate taxes are assessed on the taxable net incomes of Subchapter C corporations (Subchapter S corporations and other pass-through entities are not subject to the tax). The taxable net incomes of companies in the stock sample are substantially similar to those companies' accounting profits for book purposes.[6]

Given that, under the EMH, stock prices reflect all publicly available information, stock prices should change at latest shortly after a tax change is made certain, i.e. when the proposed tax change is signed into law. It is important to note that changes in tax rates do not map one-to-one with changes in state laws; one law may enact multiple rate changes over time.[7] Thus, the events used in this study are dates relating to changes in *laws*, which do not always coincide with the dates of changes in *rates*.

Many states have different corporate tax rates for financial institutions, and since I examine the returns to regional bank stocks, my schedule of law changes looks slightly different than those schedules used in the aforementioned litera-

brackets beginning at incomes so low relative to the typical income of a firm in my sample so as to not significantly affect my findings. Possible exceptions are the $250,000 top bracket in Kentucky and the $1,000,000 brackets of New Mexico and South Dakota, which affect less than one percent of the sample of firms.

[5] States excluded for this reason are Michigan, Ohio, and Texas.

[6] Although there are a number of "M-1 adjustments" made to reconcile net income for tax purposes and net income for book purposes, this study merely assumes that such adjustments net to zero.

[7] A complicated example is the corporate tax law passed in Indiana in 2013, which phased in nine successively lower rates over the course of ten years.

ture. I identify these law changes first by identifying changes in the top marginal rates paid by financial institutions by comparing different years' rate schedules found in *The Book of the States* for years 1994-2017.[8] Where surtaxes are employed, top marginal rates and surtax rates are combined into one top effective marginal rate. From these rate changes, I map the large set of top marginal rate changes to a smaller set of corresponding law changes, by searching LexisNexis and the websites of state legislatures. For each applicable law change, I collect five key dates: the date that the earliest talk of an impending corporate rate change was in the news, the date the legislation was introduced, the date the legislation was passed by the state legislature, the date the bill became law, and the date the first rate change in the law went into effect (as well as future dates of other top rates coming into effect, in the case of a law with multiple rate changes over multiple years). These first four dates always follow in chronological order, but the fifth may not, in the case of retroactive law changes.[9] If investors view the legislative process as a process of an impending law becoming more certain, then, under the EMH, stock prices leading up to the signing of a law should reflect investors' evolving notions of the probability of the law's passage. Of these five dates, the most problematic is the first date, hereafter called the "first news date." Unlike the other dates, there is a significant degree of subjectivity in selecting this date, and choosing a date that is too late may result in missing out on a period of time when investors assessed the likelihood of an impending law change as ever increasing (thus, by picking too late of a date I may miss the relevant stock market reaction).[10]

For each law change affecting top marginal rates, additional data collected includes whether or not the change only affected financial institutions, and

[8] For further information about the collection of rate change data, see the Data Appendix.

[9] Nineteen of the forty-nine law changes studied were retroactive.

[10] Just because an impending law change is not touted in the news does not mean that lawmakers and investors are not already discussing it in less public circles.

whether or not the rate changed via a surtax rate change or a regular rate change. Additionally, based on narratives pieced together from LexisNexis news articles, I code each law change using the four categories listed in Romer and Romer (2010) in order to assess the exogeneity of each law change: one for law changes designed to increase output, two for changes designed to change variables related to output, three for dealing with inherited budget deficits, and four for philosophical or ideological reasons such as fairness. I add a fifth category, not found in their paper, for exogenous law changes that were due to an outside body determining that the tax code must change.[11] Reasons one and two describe more endogenous tax changes while the remainder are considered to be exogenous. Each law change is given a two letter, two digit abbreviation for ease of reference, using the state abbreviation and the year the first top rate change in the law came into effect (e.g. MD08).

Since many law changes featured multiple rate changes over time, each law change is distilled down to one overall top rate change, expressed in logarithmic change in present value of future earnings of firms.[12] This log change can be thought of as the overall percent change in corporate tax rates. Table 2 shows the dates of events relating to all state law changes relating to changes in top marginal corporate income tax rates on financial institutions for 1994-2017. Tables 3 and 4 distill this information by displaying the average number of days between key dates in the legislative history for a subset of tax decreases and increases that are used in the event studies in the next section. Table 5 provides summary information for relating to the content of each law change, including the top rate in effect before each change, top rate or rates after the change, and the overall magnitude of the change.

In order to assess the impact of state corporate tax rate changes on stock

[11]For example, this could occur when a portion of a state's tax code was struck down by a supreme court.

[12]For details on this procedure, see the Appendix.

prices, one must be able to determine how much of the underlying companies' incomes are allocated to each state for tax purposes. This task is complicated by states' tax nexus laws, which dictate how income is apportioned to the various states based on varying ratios of sales, payroll, and property. For most publicly traded companies, this is opaque, as publicly traded companies do not have to break down their sales, payroll, and property factors by state in their public filings, and proprietary databases do not contain all three factors. I use a sample of regional and community bank stocks in the US because they overcome this impediment. Unlike most public firms, who raise equity capital in order to expand perhaps nationally or even internationally, companies in the sample of regional bank stocks tend to only have operations in one or a few states, suggesting that part of these firms' business strategy is to stay small and develop a community-oriented reputation.

The business of these smaller banks is simple: they accept deposits which are used to make loans, primarily to homeowners and small businesses. The vast majority of these companies break down the number of bank branches that are in each state in their annual Forms 10-K, which are filed with the Securities and Exchange Commission (SEC) and made publicly available via the SEC's online Electronic Data Gathering, Analysis, and Retrieval system (EDGAR). EDGAR only maintains filings back to 1994. I use the descriptions of bank branch locations to allocate income for each company across multiple states, in proportion to the number of branches.[13] Corporate taxes represent a large portion of these companies' profits, giving investors a good reason to pay attention to the applicable tax rates.[14]

[13]This assumes that each branch within a company uses the same amount of payroll, property, and sales (interest revenue from loans) at each branch. This assumption is unnecessary in the case of companies with operations in only one state. For further information about the allocation of income across states, see the appendix.

[14]For example, the median firm in the sample of firms (firm sample creation is described in the next paragraph) by market capitalization averaged over the past three years 80.39% of net profits (42.90% of net income before taxes) in total income taxes, of which 8.79% of net

The sample of regional bank stocks was created by combining a list of regional bank stocks from InvestSnips.com and by searching the Center for Research in Security Prices (CRSP) database (accessed via the Wharton Research Data Service (WRDS)) for companies with Standard Industrial Classification (SIC) codes relating to regional banking.[15] If banks lobby for lower tax rates, stock returns could be endogenous to tax rate changes. To minimize these concerns, I drop companies belonging to the Financial Services Roundtable lobby and companies with a market capitalization of over ten billion dollars.[16] In order to make sure markets are efficient enough to capture publicly available information in a timely manner, I drop banks that do not trade on major US exchanges and banks with market capitalizations below ten million dollars.[17] I also drop companies that are "too national," i.e. that have operations in more than ten states. Daily stock returns for the final sample of 639 firms are downloaded from the CRSP database.

4 Methodology and Results

In this section, I describe some of the preliminary tests needed to study the relationship between stock prices and expected tax rates, and I detail the design and results of the two main methodologies used. Of these, the first, a series of financial event studies, shows the abnormal stock returns (relative to the market) for a basket of affected stocks around key dates in the legislative histories of corporate tax law changes. The event studies suffer in that they do not account for different treatment sizes (differing sizes of law changes or different percentages of the operations of companies that are affected by a law change), and because the event timelines are different for all law changes. However, they are useful in

profits (4.82% of net income before taxes) was comprised of state corporate income taxes.

[15]InvestSnips.com is a website specializing in creating themed lists of stocks.

[16]The Financial Services Roundtable is the largest banking lobby in the US.

[17]For additional information about dropping stocks from this sample, see the appendix.

that they help visualize the impacts of the unfolding tax law changes on stock returns.

Using the second main methodology, a DiD regression framework, I am able to deal with the shortcomings of the event studies by using different size treatments per stock. In addition to utilizing a traditional DiD framework, I also use median absolute deviation (MAD) and robust regressions, which are less influenced by outliers than least squares regressions. I also offer placebo tests for the event study and DiD frameworks. In all cases, the treatment on a stock is defined as the log change in tax rates, adjusted for timing, multiplied by the proportion of that firm's operations in the state of the law change (for example, if a tax decrease's overall size is determined to be a log change of -0.008, and a firm has fifty percent of operations in the state in question, its treatment is -0.004).

4.1 Preliminary Tests

Although it may be tempting to regress variables related to capital on corporate tax rates (Krzyzaniak and Musgrave (1963)), such regressions are bound to be spurious, due to the random walk properties of these time series variables. Stock prices have long been argued to follow a random walk (hence the title of Malkiel's (1973) famous opus), and past studies have shown that state corporate tax rates follow a random walk (Ljungqvist, Zhang, and Zou (2017)).[18] Because other studies use samples of non-financial firms, which sometimes have different corporate tax rates than financial firms, I run Dickey-Fuller tests on the top marginal corporate tax rates for the forty-five of fifty-one states that tax the income of financial corporations (Dickey and Fuller (1979)). I fail to reject the null hypothesis of a unit root for all states tested except California and

[18] To be fair, if we assume that such rates are bounded between zero and unity, then they cannot truly follow a random walk. However, an accepted procedure is to run Dickey-Fuller tests nonetheless.

Connecticut, which reject at the one and five percent levels, respectively. If a trend is included, the results for CT go away, and CA only rejects at the ten percent level.[19] Taken as a whole, these results suggest that top rates of state income taxes on financial corporations also follow a random walk. By definition of a random walk, these results help to alleviate concerns that investors may expect tax rates to return to some average level after the implementation of a rate change.

4.2 Event Studies

Because stock prices and top state corporate tax rates on financial institutions are both integrated of order one, first differences in these variables - stock returns and tax rate changes - must be used for inference. The financial event study, developed by Fama, et al. (1969) is a common financial inference tool that employs the use of first differences in stock prices to determine unusual stock return behavior related to an event. The procedure for this technique is as follows: I regress stocks' returns on market returns (and other factors) well before the occurrence of some event; this establishes a model of how the stock typically moves with the market; then, in an event window surrounding the event in question, I use the model to predict the expected returns for the stocks; deviations from the model's prediction are dubbed abnormal returns, and the sum of these, dubbed cumulative abnormal returns (CAR) explain the portion of returns' behavior due to the event in question. When events for different stocks are associated with different dates, event dates are all normalized to be time zero, effectively stacking multiple events on top of one another and taking the average CAR at each point in time. The time periods involved are known

[19]The results for Connecticut may be driven by the fact that the CT legislature repeatedly renewed a large surtax which was originally intended to exist for only a couple of years. California also had an unusual tax rate system for banks in the early to mid 1990s, where a committee would set tax rates periodically (thus rates were not known far in advance).

as the "estimation window," the time period over which the model is run, the "gap," a space between the estimation window and the period to be studied, and the "event window," which is the period under study, centered around a particular event.

I create the event studies in this paper using WRDS' built-in EVTSTUDY module, using the Carhart four-factor estimating model (Carhart (1997)),

$$\hat{R_{it}} = \alpha_i + RF_t + \beta_{1,i}(R_{Mkt,t} - RF_t) + \beta_{2,i}SMB_t + \beta_{3,i}HML_t + \beta_{4,i}MOM_t,$$

where $\hat{R_{it}}$ represents the expected return to stock i over some time frame t, based on actual market return $R_{Mkt,t}$ (which usually comes from a broad-based stock index such as the S&P 500), RF_t is the risk-free rate (to adjust for risk), and there are factors for the return premiums of small company stocks over larger stocks SMB_t (based on market capitalization), high book-to-market-value stocks ("value stocks") over low book-to-market-value stocks ("growth stocks") HML_t, and momentum MOM_t, which adjusts for the markets' tendency to stick with winners over losers.[20]

I run event studies separately for tax increases and decreases, for both using each of the five dates for each tax law change's legislative history, for a total of ten studies. For all event studies, I estimate the Carhart model using an estimation window of five hundred days, a gap of three hundred days, and an event window of thirty days on either side of each event. The long estimation

[20]A number of other models are commonly used. Event study models available in WRDS' EVTSTUDY module include the market model (Fama et al. (1969)),

$$\hat{R_{it}} = \alpha_i + \beta_i R_{Mkt,t},$$

the Fama-French three-factor model (Fama and French, (1992)),

$$\hat{R_{it}} = \alpha_i + RF_t + \beta_{1,i}(R_{Mkt,t} - RF_t) + \beta_{2,i}SMB_t + \beta_{3,i}HML_t,$$

the market-adjusted model, which adapts the market model by forcing β to equal unity, and the above described Carhart model. These other models produce substantially similar images.

window was chosen so as to establish a strong relationship between the stocks in my sample and the market, and the long gap was chosen so that the estimation window avoids other events in the legislative history of the same tax law change.[21] For all event studies, I only include firms whose treatment effect (log change from Table 5 multiplied by the proportion of that firm's operations in the affected state) is greater than 0.005 (in other words, if the profits of a firm are expected to change by more than half of one percent, *ceteris paribus*). The number of stocks applicable to each tax change varies even within a single tax change's legislative history, as stocks may have been trading during the estimation window or event window relative to one date but not another.

4.2.1 Predictions

It is important to note that I am capturing the change in *capitalization* of a stocks due to corporate tax changes. If we think of the capitalization value of a stock as representing the present value of future earnings derived from this type of asset, then earnings occurring sooner rather than later account for a larger portion of a stock's value. Thus, the short run effects that are ignored by many corporate tax models may play a large part in the value of a stock. Auerbach (2006), for example, acknowledges that, in the short run, the entire burden of a corporate tax may fall on corporate capital, until it gradually is partially shifted to non-corporate capital, labor or other factors. In the case of regional bank stocks, firms develop community relationships in order to originate more loans. If these community ties make firms less mobile, they may have difficulty quickly responding to tax rate changes (for example, by shifting business to other states). Furthermore, banking is a competitive industry that includes many companies that take business forms other than the C corporation (for

[21] For example, if the gap were shorter, and I run an event study on one of the later dates in a law's legislative history, estimation of the model may be tarnished by stock price changes due to the anticipation of said law's passage.

example, S corporations) that are not subject to corporate taxes. If banks subject to corporate taxes compete on loan rates with banks not subject to the tax who are able to keep rates low, that is reason to suspect that an entire corporate tax change may eat into profits (or may fall on some other factor, such as lowering wages of bank tellers).

To inform these short run predictions, let us suppose that the price of a stock P_{it} can be approximated by the dividend growth model (Gordon and Shapiro, (1956)) as

$$P_{it} = \sum_{k=1}^{\infty}(1-\tau)d_0\left(\frac{1+g}{1+r}\right)^t, \tag{1}$$

where d_0 is the current period earnings before state corporate taxes, g is the growth rate of earnings, r is the required rate of return, τ is the state corporate tax rate and $r > g > 0$. Then it can be shown that the first n terms of the above summation represent a proportion $1-\left(\frac{1+g}{1+r}\right)^n$ of the total value of the stock. I estimate g and r for the regional banking industry to be 6.5% and 13.5% respectively (annually), which means that earnings over the next five years make up over a quarter of the value of the stock, and earnings over the next ten years make up a little less than half the value of the stock in this model.[22] Even if a significant portion of the corporate tax is eventually shifted to labor, short run effects of less mobile firms should account for a hefty portion of the change in stock price associated with a change in corporate tax rates.

As far as timing goes, it is difficult to predict when stock prices will react to potential corporate tax rate changes. Because corporate tax revenues make up a small portion of state revenues (and bank-specific corporate tax revenues even less so), investors may expect state budget deficits to be closed with personal income tax or sales tax increases, as these other types of taxes typically make

[22]For details about the estimation of parameters g and r, see the appendix.

up the lion's share of state revenues. If lawmakers wish to stimulate their state's economy with tax cuts, then investors may predict that taxes other than corporate taxes may fall, for similar reasons. Once a tax rate change bill is introduced into the state legislature, there is some anticipation that it may pass and become law, although it is difficult to predict how much the likelihood that the bill becomes law changes throughout its legislative history. The full impact of the tax law change on stock prices should be felt by the time the bill is signed into law (or shortly thereafter), as that is the time the change is nearly certain. I highly anticipate that there will be no action around the date a law goes into effect. By that point, the law has been publicly available information for some time.[23]

Finally, I expect the results to be quite noisy, as the volatility in stock prices is greater than what can simply be explained by changes in future earnings (though stock prices do fluctuate *around* the discounted value of future earnings (Shiller, (1980)). Also, my sample sizes are small, so the lack of power should contribute to the noisiness. Furthermore, I am comparing bank stocks to the market as a whole. I run the risk of results that are driven by periods of time when the banking sector diverged from its usual relationship with the market. While the comparison to the market should control for economy-wide cyclical movements such as the bear market of late 2007 and the following financial crisis, it may not control for banking-specific cyclical movements, such as the crash in the housing market in 2006 that preceded the economy-wide collapse.

4.2.2 Event Study Results and Discussion

Figures 1 and 2 show the results of the event studies. Tables 3 and 4 are helpful in figuring out how each of the five panels in each figure fit together with one

[23]With the exception of retroactive tax law changes, of which there are many (again, nineteen out of the fifty law changes in Table 2 are retroactive). In those cases, the laws typically go into effect well *before* the legislative process.

another across time. Figure 1 shows the CAR for tax decreases for firms with treatment sizes greater than 0.005 in magnitude. Notably, there is an upward spike exactly coinciding with the first news date, suggesting that markets are incorporating news of impending tax law changes in close to real time. However, there is some upward movement in stock prices beginning about twenty days prior to lawmakers publicly announcing interest in a tax cut. This could be due to noise or misspecification of the first news date for some law changes. After the initial spike, prices seem to be somewhat mean-reverting. The remaining panels in Figure 1 are mostly flat and noisy.

Figure 2 shows the CAR for tax increases. In general, the CAR exhibit a downward trend, as predicted. However, significant negative CAR do not appear until panels (c) and (d), around the dates tax increase bills are actually passed. Although these images are more descriptive than causal, they suggest that markets believe potential tax decreases will occur as they are announced, but markets wait to incorporate news of tax increases until they are nearly certain (and are possibly slower to do so even after they are signed into law). The images in Figure 2 are noisier than those in Figure 1, as the event studies for tax increases only employ about one fifth of the firms (there are far fewer tax increases in the sample).

The timing of the reactions of stock prices, coupled with the EMH, suggests that lawmakers signal potential tax decreases well before they are introduced as bills, and that these signals are heeded. On the other hand, the timing for increases suggests they do not send signals in advance or that the signals are unheeded early on in the legislative process. This is rational for lawmakers if lawmakers receive some additional benefit of signaling potential tax decreases but not increases. For example, if a reelection campaign is underway, incumbent lawmakers have incentive to tout future tax decreases as a way to curry votes.

Interestingly, of the sixteen tax increases in the entire sample (including those excluded from the event studies due to their small magnitude), nine increases were made by raising surtaxes rather than regular rates. As a method for changing top effective rates, surtaxes are more opaque than regular rates, as they require understanding of how they work and require additional calculations. This is consistent with the idea that lawmakers may be sheepish to signal tax increases.

Perhaps shockingly, the magnitudes of the CAR seem quite large relative to the size of the tax rate changes. Again, these figures suffer from the wide range of treatment sizes, which in these figures range in (log) magnitude from 0.0050 to 0.0370. The one-day spike on the announcement of potential tax decreases is approximately three and one half percent, and the total decline in the event window surrounding the passage of tax increases is roughly fifteen percent.

There are a number of possible explanations for the large magnitudes of these effects. First, the banking industry is highly competitive, meaning that cost increases for a subset of firms can have a deleterious effect on market share. Especially in the case of tax increases, the new costs of a tax increase may force banks to increase loan rates to borrowers. These higher rates may be enough to incentivize borrowers to cross state lines for lower rates. Even within states, these higher costs only effect C corporations, and publicly traded regional banks face competition from S corporations, which do not face the tax. Thus, S corporations may be able to hold their loan rates steady while C corporations increase theirs, which may shift market share in the favor of S corporations. S corporations may take further advantage of tax increases by offering loan refinancing to the clients of C corporation banks.[24]

[24]Banks were first allowed to operate as S corporations in 1997, as part of the Small Business Job Protection Act of 1996. Unfortunately, I do not have enough variation in state corporate income tax rates in my sample before this date to draw any meaningful conclusions about the effects of this legislation.

Additionally, twenty-three out of the twenty-seven the tax changes used in these event studies impacted both the top marginal corporate tax rates of both financial and non-financial corporations. If some of these non-financial corporations are customers of their regional banks, lower taxes may lead to more bank deposits by non-financial corporations, which, dollar for dollar, leads to even more in loans made by banks. Lower taxes may allow both banks and their corporate customers to increase investment, which may lead to new, profitable projects for both types of corporations, and as a bonus for the banks, more loans to finance those projects. Although community banks may remain rooted, corporate tax cuts in a state may entice large, national firms to move business units into that state (especially when such movements can take advantage of nexus laws), potentially leading to more banking deposits and loans in that state. Hines (1996) finds that a one percent decrease in a state's corporate tax rate leads to increase in investment from foreign sources on the order of about ten percent. The situation here is somewhat different, in that firms moving across state borders are making nearby moves rather than choosing a location across the world, but it is similar in that small tax changes can exert large effects on firm decisions.

4.2.3 Placebo Event Studies

In order to challenge my event study findings, I run a series of placebo event studies. For the first set of tests, I rerun the original event studies, but substituting twenty random stocks from my sample of bank stocks for each event, conditional on the random stocks having no operations in the states in question. Flat, insignificant results should help to alleviate concerns that my results in the previous section were driven by events where the banking sector as a whole diverged from its usual relationship with the market. Figures 3 and 4 show these results for tax decreases and increases, respectively. In general they are flat and insignificant, however, they do sometimes show trends and occasional spikes. These movements in what should be a null result show that these highly correlated stocks and the banking sector as a whole can diverge from the market sometimes in unpredictable ways.

Next, I rerun the original event studies using the original companies, except that I shift all dates backwards by four years. I choose this date because it is far before the events in question (so that the event window of later dates like the date the law becomes effective do not coincide with earlier actual dates, such as the first news date). Figures 5 and 6 display these results for tax decreases and increases, respectively. These results are less convincing. While most of these are flat and insignificant, the CAR in panels (c) and (d) of Figure 6 veer sharply downward. The basket of stocks used to make this graph come from only six states, and thus have highly correlated returns within states, so the coincidence of unusual events in just a few of these states could significantly drive results. As these same stocks were used in the original event studies, this means that concerns that random events are driving results are not alleviated. While the event studies are helpful as a descriptive tool to help visualize changes in stock prices brought on by changes in tax laws, they are not conclusive. However, the DiD regressions in the following sections are able to improve upon the shortcomings of these event studies.

4.3 Differences in Differences Regressions

The main advantages of the DiD regressions are that they can account for different sizes of treatment effects and that they can allow for different numbers of days between dates in the legislative histories of different tax law changes. Also, the EVTSTUDY module relates stock returns to the market as a whole, and the

DiD regressions can compare bank stocks to other bank stocks specifically.[25] I estimate the equation

$$\log(P_{i,t2}) - \log(P_{i,t1}) = \beta Treatment_{i,j} + \gamma X_{i,j,t2-t1} + \epsilon_{i,t2-t1}, \tag{2}$$

where $P_{i,t}$ is the price of stock i at time t, with times $t1$ and $t2$ being the beginning and ending dates of some window for tax change j, $t2 > t1$, $Treatment_{i,j}$ is the amount of tax law change j that effects firm i as measured in logs, $X_{i,j,t2-t1}$ is a vector of controls, and $\epsilon_{i,t2-t1}$ is an error term.[26] $Treatment_{i,j}$ is calculated by multiplying the overall tax law change in logs times the proportion of stock i's income derived from the state in question of tax law change j. The window used is five days prior to the first news date of a law change until five days after that law is signed.[27] A constant term is eliminated because these windows vary between laws, though the number of trading days is used as a time control. Other controls include the average number of housing starts during the window (as a measure of overall loan origination), the average number of banking companies during the window (which includes S corporations and non-publicly-traded banks), and measures of the federal funds rate, all downloaded from the Federal Reserve Economic Database (FRED). Additionally, I construct a number of dummy variables that allow me to run regressions on subdivisions of the full sample, particularly flags for tax increases, and for tax changes that fell into the more exogenous categories. [28] Finally, I utilize fixed

[25] I am working on custom code to be able to create event studies to compare banks stocks to other bank stocks directly, perhaps by creating my own index of regional and community bank stocks.

[26] In a sense, $t1$ and $t2$ are "tied" to j, so either the j or t notation can be dispensed with, depending on the control.

[27] Regressions results for other windows are forthcoming.

[28] I constructed other dummy variables that were used in regressions that are not shown because they do not meaningfully change results: if the change was for banks only, if the change was made via a surtax, if the change was retroactive, if the change was larger in magnitude than the mean increase (for increases) or decrease (for decreases) in the sample of changes, if the change only affected a small number of firms in the sample, if changes follow a similar change made in the same state in the previous year, whether the change was ranked

effects for the state of the tax change and the year the bill was signed into law.

Regression results are displayed in Table 6. In all cases, the dependent variable is the summation of log returns for each firm throughout the window. The regressor of interest is Treatment, the log size of the tax change. Because the regressions regress log changes on log changes, coefficient estimates can be interpreted as the elasticity of stock prices on tax rates. The top panel of Table 6 includes all tax law changes in the sample, while the middle and bottom panels only include tax decreases and increases, respectively. The first column shows regression results for the log stock returns regressed only on each firm's treatment effect and a control for the number of trading days elapsed in the window. Standard errors are clustered at the state (of the law change) level. The second column adds controls for US housing starts during the window, US bank companies active during the window, and the federal funds rate. The fifth column adds state and year fixed effects, as well as a time trend. The pooled results are generally significant at the five percent level, and show elasticities of approximately negative five percent for each percent of the tax change, but these estimates fall to about three quarters of a percent when fixed effects are added, and lose significance. The middle panel shows that these elasticities are generally insignificant for tax decreases. However, the bottom panel shows that these elasticities are estimated to be around negative twenty-five percent for tax increases, before fixed effects are added, and are significant at the one percent level. Once fixed effects are added, the estimate for tax increases falls to about negative eighteen percent, and is only significant at about the fourteen percent level. These results echo the results of the event studies.

It is probably most proper to cluster by the state of the law change, as

exogenous, whether the change occurred in CA or CT (see the section on preliminary tests), if there was some uncertainty in apportioning a firm's income across states, and if stock/tax law change pairs could possibly interfere with one another (as an example, as CT09 was going on, so was NC09 and OR10).

all C corporations in such a state are treated all at once. However, there is a problem with the small number of clusters. When clustering by state, there are only twenty-five clusters altogether, nineteen for tax decreases, and ten for tax increases. Cameron, Gelbach, and Miller (2008) show that clustering relies on the asymptotic assumption of infinitely many clusters, and suggest a wild bootstrapping procedure when there are too few clusters. The results of this procedure, based on column five of Table 6, are shown in Figure 7 (bootstrap distributions for other columns are not shown, though the ninety-five percent confidence intervals for tax decreases for the regressions in columns one and two of Table 6 do not include zero). The distributions for all changes and decreases only show a familiar shape, whereas the distribution for tax increases is oddly shaped and rather fat, and thus includes zero. Another possibility is to cluster by stock. In this case, there are an ample number of clusters for tax decreases and increases, and when the regressions of column five a repeated with these clusters, the coefficient estimate for the Treatment variable for tax increases is significant at the one percent level (results not shown).

Given the large coefficient estimates from the standard DiD regressions, especially for tax increases, and the tendency for distributions of stock returns to be fat-tailed, it is natural to be concerned that outliers are driving results. I take two approaches to mitigating the effects of outliers: using robust regressions and median absolute deviation (MAD) regressions. Column three of Table 6 shows the results of the outlier robust regressions, which use the method suggested by Li (1985). The method first screens for extreme outliers (those with a Cook's distance greater than one: no observations were excluded based on this criterion), then uses a combination of M-estimators to down-weight observations that exert too much influence over the point estimates. It performs a series of Huber iterations followed by a series of biweight iterations until weights change by less than five percent, and uses these final weights to run a weighted regression. Column six repeats this, using fixed effects. Results of the MAD regressions are in column four. These regressions minimize the sum of absolute deviations from the median, rather than minimizing the sum of least squares from the mean, as in OLS. One unfortunate problem with this approach is that there may be multiple fits that minimize the absolute deviation from the median; these are noted on the table. A summary of uses of quantile regressions of this kind in financial economics can be found in Koenker and Hallock (2001). These are repeated, but using fixed effects, in column seven. The magnitudes of these estimates tend to be smaller, suggesting that outliers did indeed influence results in columns one, two, and five, particularly for tax increases. Elasticities for the pooled changes are generally significant and hover around negative four, or between negative one and two when fixed effects are included. Treatment coefficient estimates for tax decreases teeter on the edge of significance, and have values around negative one and negative two. The elasticity estimates for tax increases are approximately negative fifteen to negative sixteen, or around negative ten when fixed effects are employed, and are all significant at the one percent level. Given that columns six and seven are the most robust to outliers, utilize the most controls, and yield similar treatment point estimates for tax increases, I am more swayed by their results than some of the other columns.

Across the entire window of these tax law changes, changes in stock prices behave asymmetrically for tax decreases versus increases. Depending on the specification, the upward spike seen in event study for the announcement of tax decreases is washed out by the time those laws are signed. The decrease in costs brought on by a decrease in corporate income tax rates, which only benefits C corporations, could be passed along to shareholders, but it is also possible that C corporation banks use this opportunity to increase pay for employees or executive management. The upward spike that mean reverts could be a behavioral reaction of some market participants who initially anticipate that the windfall will benefit investors, only to be told by the broader market that such a windfall will not be passed along to them. This possibility - that a decrease in tax expense is turned into an increase in management compensation - will be the subject of further research.

As mentioned in the discussion of the event studies, the large magnitudes of the Treatment estimates for tax increases likely come from multiple sources of competition that deteriorate profits and market share for publicly traded banks. If C corporation banks are forced to raise loan rates to borrowers, they may face stiff competition from S corporation banks within their state (both for new loans and refinancings) as well as C and S corporation banks in lower tax states. In the extreme hypothetical case of perfect competition, a tax increase that only increases costs for some firms should drive those firms out of the market entirely.

The extreme asymmetry of results for tax decreases versus increases is in line with previous studies that use state corporate tax rate changes as a source of variation. For example, Ljungqvist, Zhang, and Zou (2017) find that firms reduce risk-taking only in response to tax increases, Ljungqvist and Smolyansky (2016) find that employment and employment income generally only respond to tax increases, and Heider and Ljungqvist (2015) find that firms' leverage decisions respond only to tax increases.

4.3.1 Placebo Regressions

I rerun the regressions of subsection 4.3 by keeping the same event windows (first news date minus five days until bill signed date plus five days) but artificially changing the treatment values to randomly generated placebo treatment values. I estimate a series of one percent tax increases by assigning random treatments using the following distribution: approximately ninety percent of stocks are assigned a zero treatment, approximately four percent are assigned the full one percent treatment (as if their operations were fully contained in the state of a change), and the rest receive treatment randomly uniformly distributed between zero and one. I generate five hundred sets of random treatments using the actual time frames for all tax law changes, and, for each, regress true log stock returns on the false treatments using OLS, while controlling for the number of trading days. I repeat this procedure by using tax decreases only and by using tax increases only.

The results of these OLS placebo regressions are summarized in Figure 8. This graph plots the kernel density of the coefficient estimates for the false treatment variable for all three sets of placebo OLS regressions with random treatments. The placebo treatment estimates for tax decreases are centered around zero, however, the placebo estimates for tax increases are slightly to the left of zero. The stock return data have very fat tails, and if enough very negative returns are paired with small positive treatment values, this could explain the non-zero result. To try to correct for this, I rerun the placebo tests with random treatments using the robust regressions and MAD regressions used in the previous section, again each using 500 sets of random treatments for all tax changes, tax decreases, and tax increases. The results of these placebo regressions are shown in Figures 9 and 10. The estimates are nicely centered around zero when these alternate regression types are used. As they are particularly robust to the influence of outliers, this suggests that outlier stock returns paired with small treatments may indeed be the cause of the non-zero result for tax increases in Figure 8.

In addition to running placebo regressions using the correct time frames of law changes with fictitious treatment sizes, I also run regressions using the actual treatment sizes but with fictitious time frames. The law changes I observe evolve over an average of many hundreds of days with a great deal of variation in window length. To safely shift them to a non-treated time period, I must be sure to shift them far enough on either side of the actual window so as to not overlap with the actual window and pick up some of the actual treatment. Therefore, I shift each law change's window back in time one thousand days, back in time two thousand days, and forward in time one thousand days, creating only three placebo time frames for each law change. Given that most stocks have a fairly short life in the sample (being publicly traded on major stock exchanges for about a decade, on average), this is about the extent of the time shifting that I am able to achieve. Unlike the previous set of placebo tests, where treatments are delivered randomly and we would expect to achieve an average zero result, there is no reason to think that nothing will show up when the treatments are shifted in time. These stocks are highly concentrated in just a few states each, returns of same-state stocks tend to be correlated, and other non-tax related events could occur at different time periods in these states that create significant results.

These regression results are presented in Table 7. The same regressions are run using OLS, MAD regressions, and robust regressions. These results lack the clear patterns of Table 6. The majority of treatment coefficient estimates are not significant, but those that are tend to be very significant, suggesting that other events occurred in enough treated states during these shifted time periods. The three regression types often create wildly different results in point estimates and significance levels, including signs that flip from one regression to another. Unlike the original DiD regressions, where all point estimates have the expected negative sign, signs for decreases and increases are often opposite each other. In addition, pooling tax decreases and increases together often creates a significant result where there was no significant result within the subgroups of just decreases or just increases, suggesting that the combined power of the two sets of observations was more important for creating a result than anything truly going on within those subgroups. However, the large point estimates of these placebo regressions, coupled with an abnormal proportion of significant estimates, suggests that the actual treatment coefficient estimates of Table 6 may be biased.

The shifted time frame of greatest potential concern is the actual windows minus one thousand days, as these periods occurred soonest before actual treatment. Here, we see three very different point estimates for the decreases subgroup, all significant at at least the ten percent level, and all positive in sign. This could suggest that state corporate income tax decreases follow periods where stocks of companies in that state decline in value. For tax increases, we see large, positive, sometimes significant coefficient estimates when treatments are shifted back two thousand days. It is hard to think of a reason why stock prices would respond six years before being treated by a tax increase, unless those states who pass tax increases see companies in that state rising in value many years before they decide to tax them. Finally, there are fairly consistent, though not often significant negative coefficients for decreases and increases, shifted forward in time one thousand days. This suggests that there may be lagged effects of tax changes, inversely related to the direction of the tax change, years after they are signed into law. This possibility will be the subject of further study in future papers.

5 Conclusion

In this paper, I use the wisdom of the markets to estimate the change in capitalization value of publicly traded regional banks due to changes in state corporate tax rates. I do this by leveraging the heterogeneous set of changes in state corporate tax rates and by employing a combination of financial event studies and DiD regressions. My results suggest that these changes in capitalization values are far greater than a 100% burden of the corporate tax on shareholders would suggest. Estimates of these elasticities (change in log stock price over change in log tax rate) range from -5.64 to -0.74, but are very asymmetric for tax decreases and tax increases. For decreases, estimates range from -2.62 to -0.22 and are generally insignificant. Estimates for tax increases are generally highly significant and range from -25.39 to -9.53, with preferred specifications, utilizing a wide variety of controls and accounting for outliers, yielding values around negative ten. This excess change in value is plausibly due to the effects of competition from banks in lower tax states and competition between C corporations and S corporations within states. The timing of these changes suggest that lawmakers stand to benefit by signalling tax decreases in advance, whereas they send far fewer signals before the passage of a tax increase.

The strength of these results should be taken with some caveats, as some placebo tests suggest that coefficient estimates may be biased. The external validity of these results may also be questioned, as my sample of stocks was limited to regional bank stocks, and models of corporate tax incidence often give drastically different results when firms' production functions are altered. I would like to expand this research by looking into stocks from other industries that also share the feature of having discernible locations. These results also represent a snapshot in time from the perspective of investors. In the long run, the burden of the corporate tax may shift to other factors, but what matters most to the stock price now is the present value of changes to earnings that occur sooner rather than later.

Given the large treatment estimates for tax increases compared to the much smaller estimates for tax decreases, future planned research will try to help explain this asymmetry. One plausible theory posits that tax decreases may not be passed on to shareholders if management uses that windfall to increase managment compensation. Thus, I intend to test whether management compensation increases for publicly traded companies in a state in the years after tax decreases are signed. Another possible reason for the asymmetry is that markets either responded wrongly in their timing (either responding too early or too late) or in their pricing, by overshooting or undershooting the valuation of these companies following tax changes. I intend to test this by looking for pricing trends in the pre- and post- periods relative to the windows of these tax changes.

The ongoing quest to determine the incidence of the corporate tax is relevant for lawmakers who wish to make a more equitable tax system. Many previous studies have focused on the impacts to labor, as the effects on labor inform the progressivity of the tax system. The results of studies that measure the incidence on capital can be combined with studies that measure the relative incidences on capital and labor to back out the incidence on labor. Thus, research on the incidence of corporate taxes on shareholders can be useful in determining the distributional effects of the corporate tax.

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7 Tables and Figures

Table 1: Descriptive Statistics, Top Marginal State Corporate Tax Rates, Levels

Tax variable	**1994 (beginning of sample)**	**2017 (end of sample)**
States taxing net income of non-financial corporations	44	44
States taxing net income of financial corporations	45	45
States w/ 0 top rates, non-financial corps.	4	4
States w/ 0 top rates, financial corps.	3	3
States w/ tax based on variables other than net income	3	3
States w/ multiple corp. tax brackets	13	14
Highest bracket (STATE)	$1,000,000 (NM)	$1,000,000 (NM, OR)
No. states w/ different rates for financial and non-financial corporations	15	14
Highest top marginal rate, non-financial corporations (STATE)	12% (IA)	12% (IA)
Highest top marginal rate, financial corporations (STATE)	12.54% (MA)	10.84% (CA)
Lowest non-zero top marginal rate, non-financial corporations (STATE)	4% (KS)	3% (NC)
Lowest non-zero top marginal rate, financial corporations (STATE)	1% (ME, SD)	0.25% (SD)

Table 2: Dates of law changes relating to top corporate tax rates on banks

Abbrev.	First news date	Bill introduced date	Bill passed date	Bill became law date	Law effective date
AL02	3/1/99	11/15/99	11/23/99	3/21/00	1/1/01
AZ94	1/1/94	3/7/94	3/30/94	4/4/94	12/31/93
AZ99	11/4/97	5/8/98	5/8/98	5/20/98	1/1/98
AZ00	11/4/97	2/4/98	5/6/98	5/19/98	12/31/99
AZ01	1/11/99	4/7/99	4/7/99	4/15/99	1/1/01
AZ14	11/19/10	2/14/11	2/16/11	2/17/11	1/1/14
CA97	1/4/94	4/10/96	7/8/96	7/15/96	1/1/97
CO99	12/16/98	1/13/99	5/3/99	6/4/99	1/1/99
CO00	7/13/99	1/5/00	5/1/00	5/3/00	1/1/00
CT95	10/28/94	5/27/95	5/31/95	6/1/95	1/1/95
CT03	12/1/02	2/3/03	2/18/03	3/6/03	1/1/03
CT04	4/16/03	7/30/03	7/31/03	8/16/03	1/1/04
CT06	2/9/05	3/16/05	6/7/05	6/30/05	1/1/06
CT09	2/9/09	8/31/09	8/31/09	9/8/09	1/1/09
CT12	2/14/11	5/2/11	5/3/11	5/4/11	1/1/12
CT14	5/13/13	6/1/13	6/3/13	6/18/13	1/1/14
CT16	3/2/15	6/2/15	6/3/15	6/30/15	1/1/16
DC95	2/27/91	1/14/94	8/1/94	8/2/94	1/1/95
DC15	2/15/14	2/15/14	6/24/14	6/25/14	1/1/15
ID01	1/21/00	3/26/01	3/29/01	4/11/01	1/1/01
ID13	12/1/11	2/17/12	3/29/12	4/5/12	1/1/12
IL11	1/4/10	1/6/10	1/12/11	1/13/11	1/1/11
IN14	10/14/12	2/18/13	4/1/13	4/26/13	1/1/14
KS98	10/3/97	1/10/98	3/1/98	3/18/98	1/1/98
KY06	2/12/04	2/2/05	3/8/05	3/18/05	1/1/05
MA95	2/2/95	5/10/95	7/17/95	7/27/95	1/1/95
MA10	12/18/07	6/13/08	7/3/08	7/3/08	1/1/10
MD08	7/26/07	10/29/07	11/13/07	11/19/07	1/1/08
NC97	12/9/94	7/8/96	8/2/96	8/2/96	1/1/97
NC09	10/20/08	2/17/09	8/5/09	8/7/09	7/1/09
NC14	4/28/12	4/17/13	7/17/13	7/23/13	1/1/14
ND17	6/17/16	8/1/16	8/3/16	8/5/16	1/1/17
NH00	1/7/99	3/4/99	4/22/99	4/29/99	7/1/99
NH16	10/29/14	2/18/15	6/24/15	9/16/15	1/1/16
NJ06	1/17/06	6/26/06	7/8/06	7/8/06	1/1/06
NM14	1/3/13	2/14/13	3/16/13	4/4/13	1/1/14
NY00	12/17/97	1/20/98	4/14/98	4/28/98	7/1/99
NY07	4/27/05	1/11/06	3/31/06	3/31/06	1/1/07
NY16	12/10/13	1/6/14	3/31/14	3/31/14	1/1/16
OR10	11/20/08	3/12/09	6/11/09	7/20/09	1/1/10
PA94	1/15/93	3/2/93	6/14/94	6/16/94	1/1/94
PA95	11/15/94	1/23/95	6/15/95	6/30/95	1/1/95
SD01	1/13/99	1/25/00	2/18/00	3/3/00	1/1/01
TN03	3/30/99	1/31/02	7/3/02	7/4/02	7/15/02
VT97	2/5/97	3/13/97	6/12/97	6/26/97	1/1/97
VT07	1/6/04	4/21/04	5/19/04	6/7/04	1/1/07
WV07	10/30/06	10/30/06	11/13/06	11/14/06	1/1/07
WV09	2/26/07	2/15/08	3/8/08	4/1/08	1/1/09

Table 3: Mean Number of Days between Event Dates Used in Event Studies, Tax Decreases (Standard Deviations in Parentheses)

	Legislation Introduced	Legislation Passed	Legislation Signed	Legislation Effective
First News Date	191.65 (157.92)	228.00 (158.66)	242.06 (159.83)	567.71 (310.78)
Legislation Introduced		60.79 (105.24)	74.00 (107.99)	345.74 (343.82)
Legislation Passed			12.60 (18.71)	280.25 (352.71)
Legislation Signed				251.29 (352.79)

Table 4: Mean Number of Days between Event Dates Used in Event Studies, Tax Increases (Standard Deviations in Parentheses)

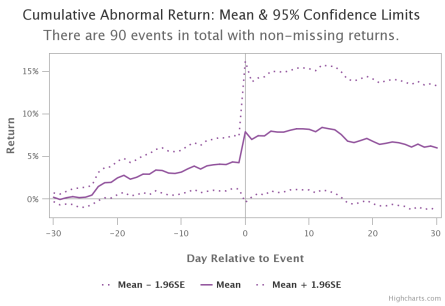
	Legislation Introduced	Legislation Passed	Legislation Signed	Legislation Effective
First News Date	247.67 (397.48)	362.17 (419.80)	386.83 (414.19)	422.67 (450.57)
Legislation Introduced		114.50 (136.65)	139.17 (123.80)	175.00 (182.72)
Legislation Passed			24.67 (46.46)	60.50 (187.55)
Legislation Signed				35.83 (149.10)

Table 5: Content of law changes of top marginal corporate tax rates on banks

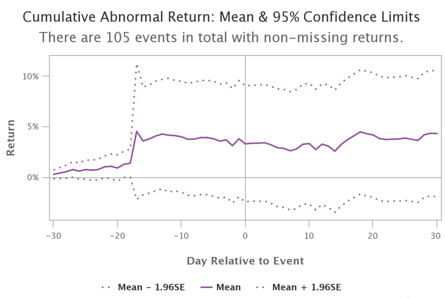
Abbrev.	Top rate before	Top rate(s) after	Overall log size
AL02	6	6.5	0.0051
AZ94	9.3	9	-0.0034
AZ99	9	8	-0.0113
AZ00	8	7.968	-0.0003
AZ01	7.968	6.968	-0.0096
AZ14	6.968	6.5, 6, 5.5, 4.9	-0.0164
CA97	11.3	10.84	-0.0050
CO99	5	4.75	-0.0027
CO00	4.75	4.63	-0.0013
CT95	11.5	11.25, 10.75, 10.5, 9.5, 8.5, 7.5	-0.0370
CT03	7.5	9, 8.25, 7.5	0.0016
CT04	9	9.375, 7.5	0.0008
CT06	7.5	9, 7.5	0.0010
CT09	7.5	8.25, 7.5	0.0016
CT12	8.25	9, 7.5	0.0020
CT14	9	9, 7.5	0.0020
CT16	9	9, 8.25, 7.5	0.0025
DC95	10.25	9.975	-0.0030
DC15	9.975	9.4, 9, 8.5, 8.25	-0.0165
ID01	8	7.6	-0.0044
ID13	7.6	7.4	-0.0022
IL11	7.3	9.5, 7.75, 7.3	0.0075
IN14	8.5	8, 7.5, 7, 6.5, 6.25, 6, 5.5, 5, 4.9	-0.0287
KS98	6.375	4.375	-0.0215
KY06	8.25	7, 6	-0.0233
MA95	12.54	12.13, 11.72, 11.32, 10.91, 10.5	-0.0209
MA10	10.5	10, 9.5, 9	-0.0141
MD08	7	8.25	0.0135
NC97	7.75	7.5, 7.25, 7, 6.9	-0.0082
NC09	6.9	7.107, 6.9	0.0003
NC14	6.9	6, 5, 4, 3	-0.0359
ND17	7	4.31	-0.0276
NH00	7	8	0.0103
NH16	8.5	8.2, 7.9	-0.0060
NJ06	9	9.36, 9	0.0010
NM14	7.6	7.3, 6.9, 6.6, 6.2, 5.9	-0.0152
NY00	9	8.5, 8, 7.5	-0.0130
NY07	7.5	7.1	-0.0041
NY16	7.1	6.5	-0.0057
OR10	6.6	7.9, 7.6, 6.6	0.0031
PA94	12.25	11.99, 10.99, 10.75, 9.99	-0.0236
PA95	10.99	9.99	-0.0013
SD01	1	0.25	-0.0072
TN03	6	6.5	0.0052
VT97	8.25	9.75	0.0171
VT07	9.75	8.5	-0.0116
WV07	9	8.75	-0.0027
WV09	8.75	8.5, 7.75, 7, 6.5	-0.0184

Figure 1: Event Studies, Tax Decreases, Firms with Log Treatments Greater Than 0.005 in Magnitude

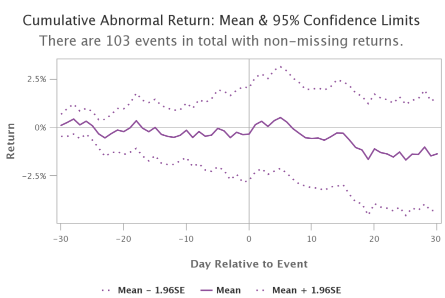
(a) Decreases, First News Date



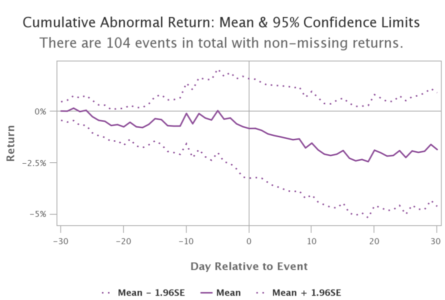
(b) Decreases, Legislation Introduced



(c) Decreases, Legislation Passed



(d) Decreases, Legislation Signed



(e) Decreases, Legislation Effective

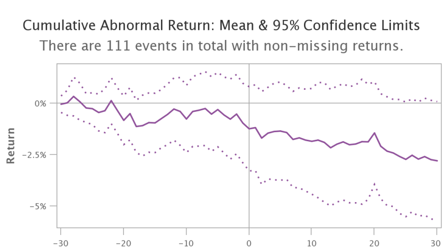
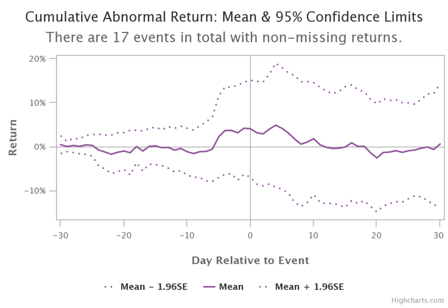
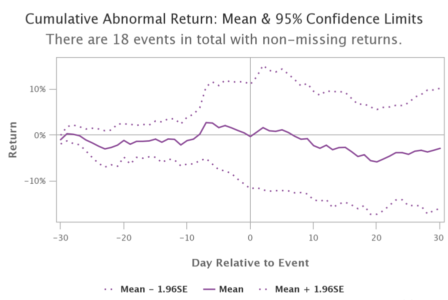


Figure 2: Event Studies, Tax Increases, Firms with Log Treatments Greater Than 0.005 in Magnitude

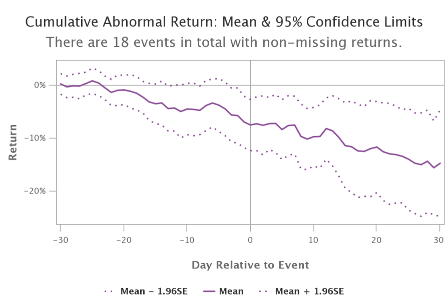
(a) Increases, First News Date



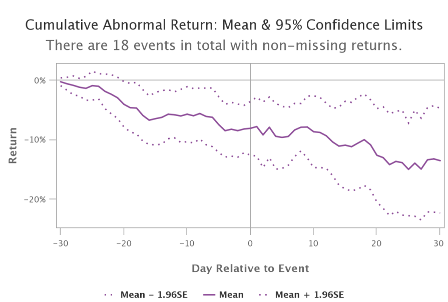
(b) Increases, Legislation Introduced



(c) Increases, Legislation Passed



(d) Increases, Legislation Signed



(e) Increases, Legislation Effective

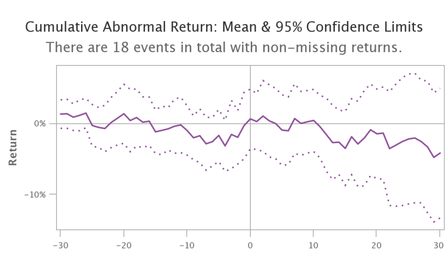
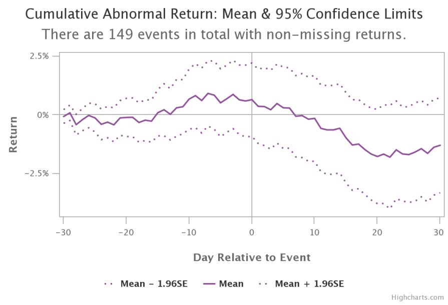
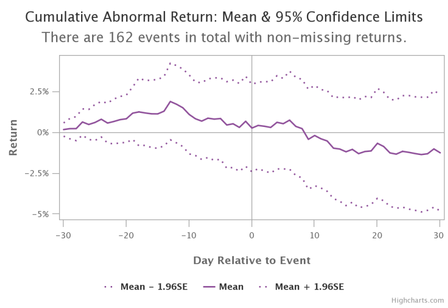


Figure 3: Event Study Placebo Tests, Tax Decreases, Using Random Firms with No Operations in State of Tax Law Change

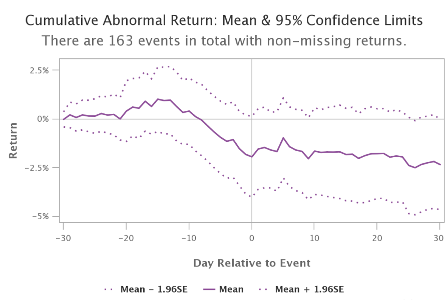
(a) Placebo for Decreases, First News Date (Random Firms)



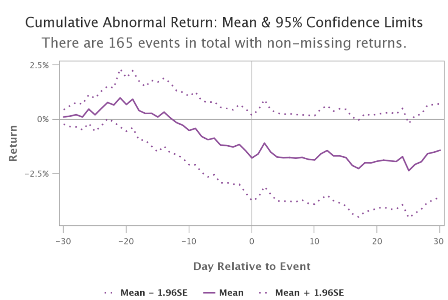
(b) Placebo for Decreases, Legislation Introduced (Random Firms)



(c) Placebo for Decreases, Legislation Passed (Random Firms)



(d) Placebo for Decreases, Legislation Signed (Random Firms)



(e) Placebo for Decreases, Legislation Effective (Random Firms)

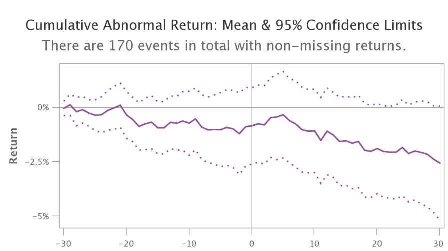
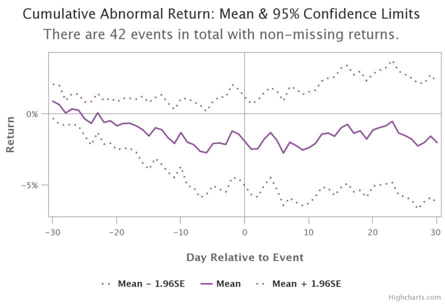
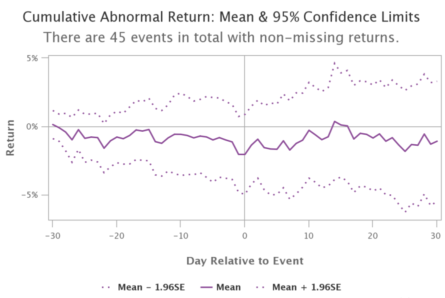


Figure 4: Event Study Placebo Tests, Tax Increases, Using Random Firms with No Operations in State of Tax Law Change

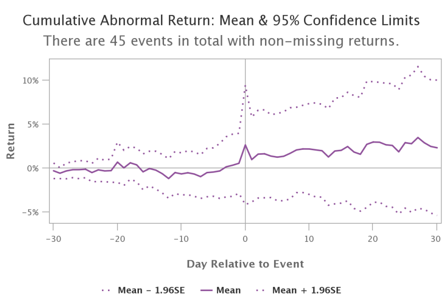
(a) Placebo for Increases, First News Date (Random Firms)



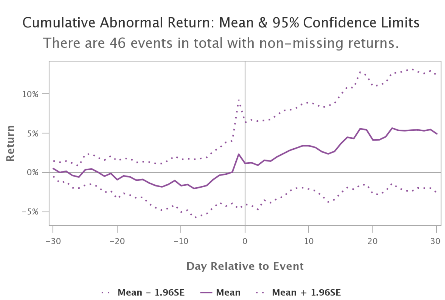
(b) Placebo for Increases, Legislation Introduced (Random Firms)



(c) Placebo for Increases, Legislation Passed (Random Firms)



(d) Placebo for Increases, Legislation Signed (Random Firms)



(e) Placebo for Increases, Legislation Effective (Random Firms)

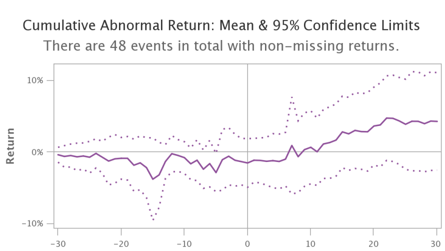
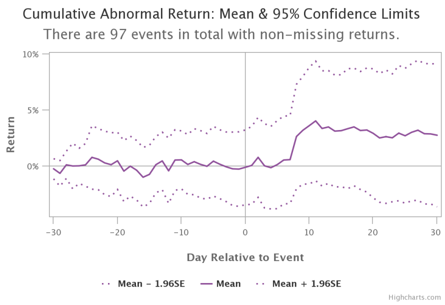
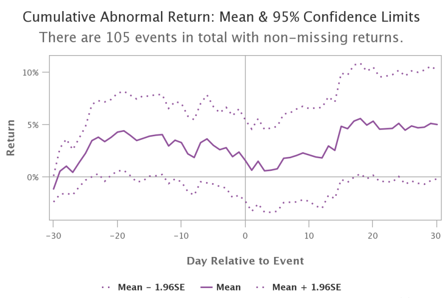


Figure 5: Event Study Placebo Tests, Tax Decreases, Shifting Event Dates Backwards in Time by Four Years

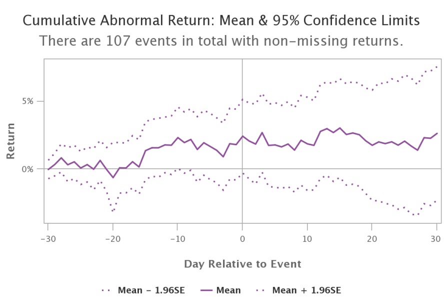
(a) Placebo for Decreases, First News Date (Shifted in Time)



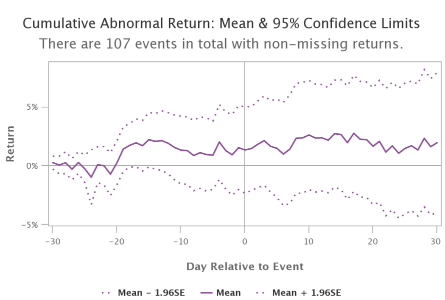
(b) Placebo for Decreases, Legislation Introduced (Shifted in Time)



(c) Placebo for Decreases, Legislation Passed (Shifted in Time)



(d) Placebo for Decreases, Legislation Signed (Shifted in Time)



(e) Placebo for Decreases, Legislation Effective (Shifted in Time)

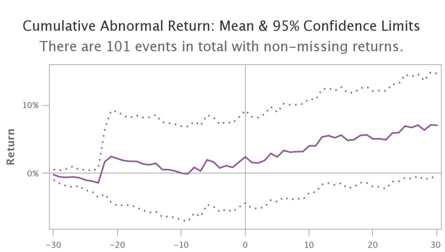
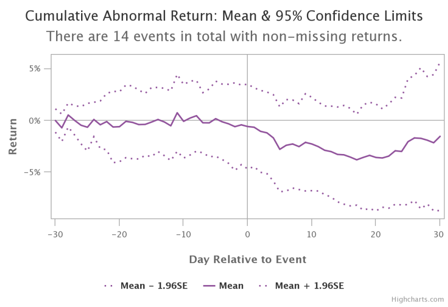
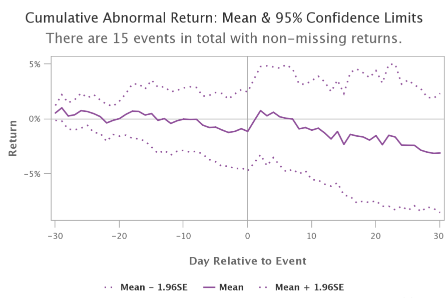


Figure 6: Event Study Placebo Tests, Tax Increases, Shifting Event Dates Backwards in Time by Four Years

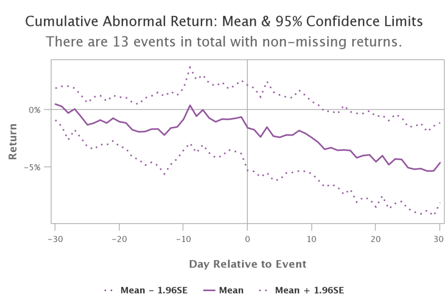
(a) Placebo for Increases, First News Date (Shifted in Time)



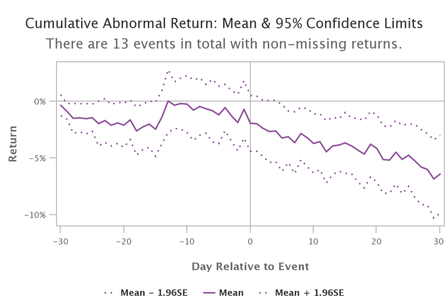
(b) Placebo for Increases, Legislation Introduced (Shifted in Time)



(c) Placebo for Increases, Legislation Passed (Shifted in Time)



(d) Placebo for Increases, Legislation Signed (Shifted in Time)



(e) Placebo for Increases, Legislation Effective (Shifted in Time)

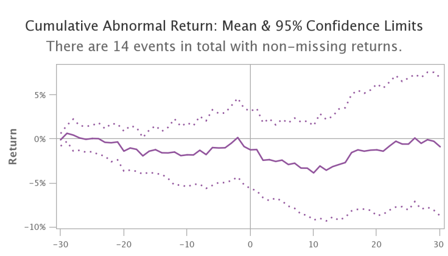


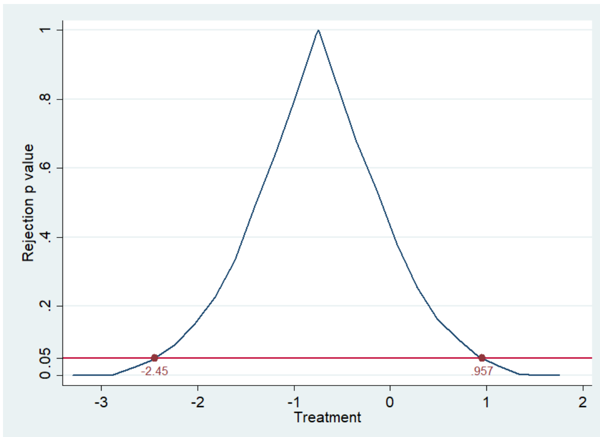
Table 6: Differences-in-Differences Regression Results

	(1) Least Squares	(2) Least Squares	(3) Robust Regression	(4) MAD Regression	(5) Least Squares	(6) Robust Regression	(7) MAD Regression
All Changes:							
Treatment	-5.6364^{**}	-5.4427^{**}	-4.268687^{***}	-4.0536^{***}	-0.7450	-1.6572^{**}	-1.0913 †
	(2.1367)	(2.3186)	(0.8481)	(1.1367)	(0.9296)	(0.7026)	(1.3815)
R^2	0.0336	0.0476		0.0261	0.3343		0.1995
Obs.	11,719	11,719	11,719	11,719	11,719	11,719	11,719
Decreases Only:							
Treatment	-2.6157^{*}	-2.0191	-1.5929^{*}	-1.1913	-0.2229	-1.6876^{**}	-0.8398†‡
	(1.4911)	(1.4200)	(0.8185)	(0.8039)	(1.5400)	(0.6878)	
R^2	0.0871	0.1524		0.0627	0.4169		0.2324
Obs.	6,963	6,963	6,963	6,963	6,963	6,963	6,963
Increases Only:							
Treatment	-25.3944^{**}	-24.6057^{***}	-16.8688^{***}	-15.7145^{***}	-17.5220	-9.5331^{***}	-10.3595^{***}†
	(9.2741)	(6.2074)	(3.8010)	(2.7271)	(10.6910)	(3.1188)	(3.7465)
R^2	0.0063	0.0821		0.0301	0.2654		0.1625
Obs.	4,756	4,756	4,756	4,756	4,756	4,756	4,756
Time Control	Y	Y	Y	Y	Y	Y	Y
Industry Controls	N	Y	Y	Y	Y	Y	Y
State Fixed Effects	N	N	N	N	Y	Y	Y
Year Fixed Effects	N	N	N	N	Y	Y	Y
Year Trend	N	N	N	N	Y	Y	Y
Standard Errors	Clustered by State	Clustered by State	See Footnote	Heteroskedastic Robust	Clustered by State	See Footnote	Heteroskedastic Robust
Exogeneity Codes Included	All	All	All	All	All	All	All

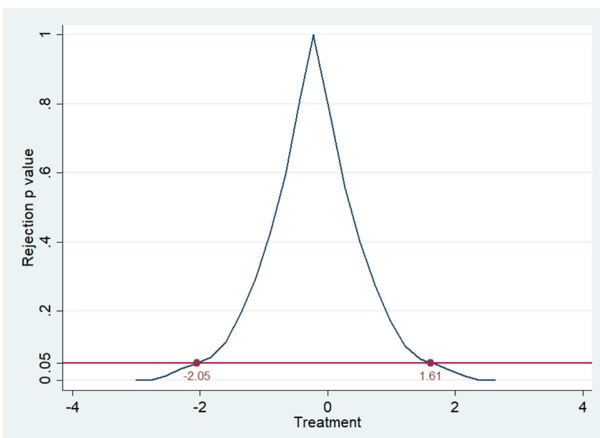
This table presents results of DiD regressions of log returns of bank stocks on treatment sizes, where treatment is the log size of a tax change in a state multiplied by the proportion of a firm's operations in the state undergoing the tax change (thus, coefficient estimates on the Treatment variable can be thought of as elasticities of stock prices on tax law changes). All regressions are taken over the time period of five days prior to the first news date to five days after the bill was signed into law. The top panel shows regression results when all tax law changes are pooled together; the bottom two panels show results only for tax decreases and tax increases, respectively. R^2s are pseudo-R^2s for MAD regressions. MAD stands for median absolute deviation. Robust regressions are based on an initial screening for outliers with Cook's distance of greater than one, followed by Li's method of following Huber iterations with biweight iterations in order to determine observation weights, before finally running a weighted regression. Standard errors for robust regressions are calculated by using the correction suggested by Street, Carroll, and Ruppert (1988). The time control is the number of trading days in each window, and industry controls include measures of number of banks in the US, number of housing starts, and the federal funds rate. Coefficient estimates marked with one, two, and three stars are significant and the one, five, and ten percent levels, respectively. Results marked with a dagger are unstable (alternate solutions may exist). Results marked with a double dagger have a highly singular variance-covariance matrix.

Figure 7: Distribution of Treatment Point Estimates Using Wild Bootstrap Procedure, OLS Model with State and Year Fixed Effects, Clustered by State

(a) All Tax Changes



(b) Tax Decreases



(c) Tax Increases

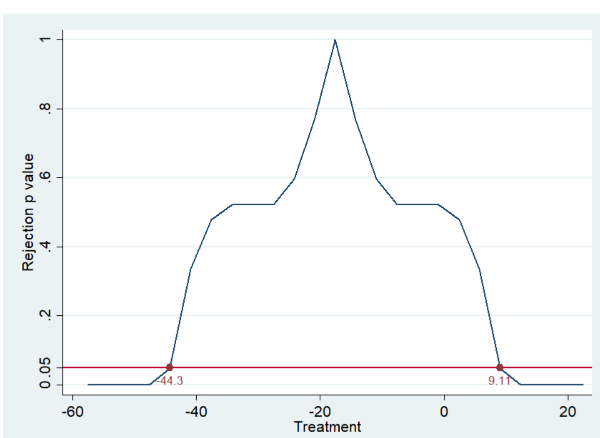


Figure 8: Treatment Coefficient Estimates, Placebo Tests Changing Treatments to Random Values, Using OLS

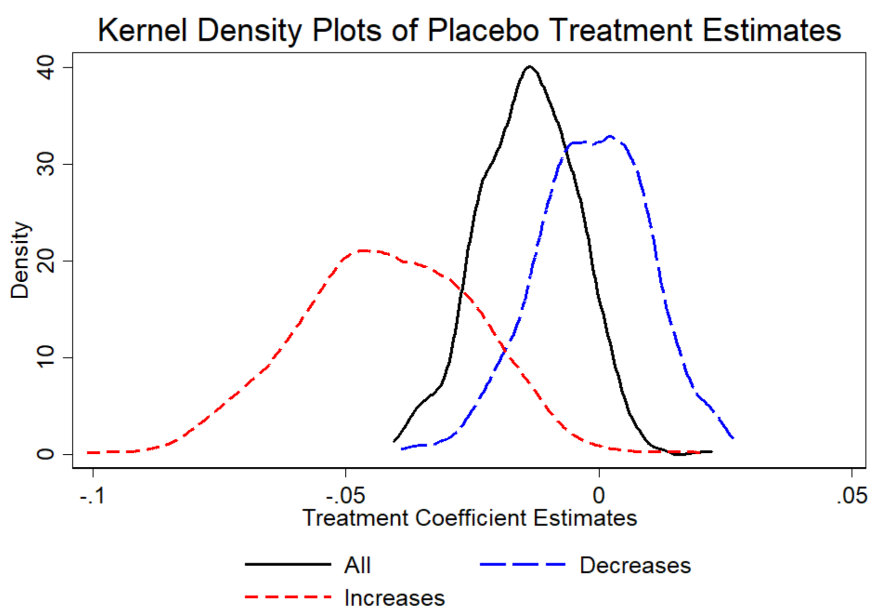


Figure 9: Treatment Coefficient Estimates, Placebo Tests Changing Treatments to Random Values, Using Robust Regressions

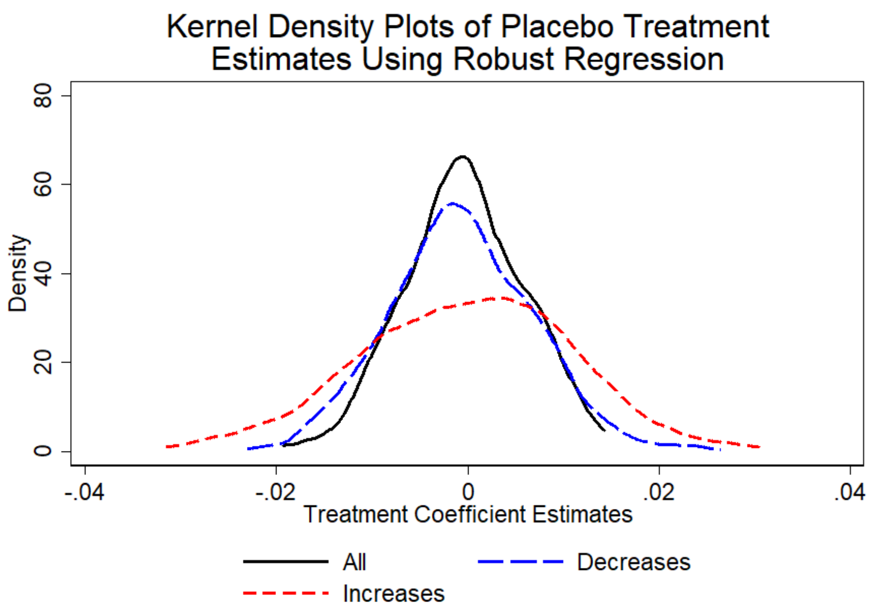


Figure 10: Treatment Coefficient Estimates, Placebo Tests Changing Treatments to Random Values, Using Median Absolute Deviation Regressions

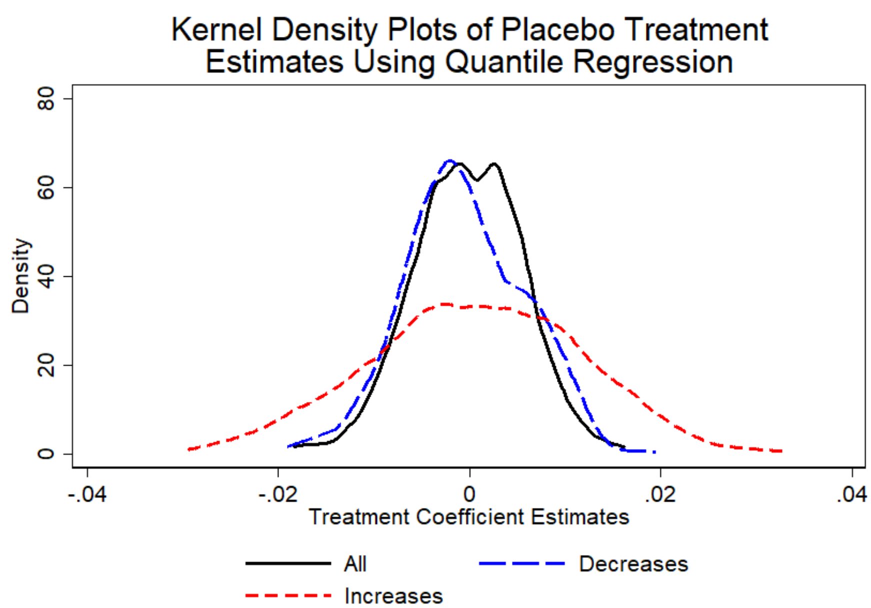


Table 7: Treatment Coefficient Estimates for Placebo Tests, Treatment Shifted in Time

	(1) Least Squares	(2) MAD Regression	(3) Robust Regression
All Changes:			
All Time Frames	2.3606	1.7506^{*}	2.1961^{***}
	(3.0707)	(1.0033)	(0.5745)
Minus 1,000 Days	6.2801	2.3333	−0.4380
	(5.5816)	(1.5238)	(0.9413)
Minus 2,000 Days	4.3104^{*}	5.9187^{***}	6.1930^{***}
	(2.3744)	(1.5572)	(1.0782)
Plus 1,000 Days	−3.6964	-2.6483^{***}	-1.8825^{**}
	(2.3151)	(0.9590)	(0.9272)
Decreases Only:			
All Time Frames	2.1957	1.9976	2.6678^{***}
	(3.0479)	(1.2279)	(0.6288)
Minus 1,000 Days	8.533^{*}	3.999^{***}	1.9033^{*}
	(4.8679)	(1.5070)	(0.9757)
Minus 2,000 Days	0.7573	1.6215	2.4939^{**}
	(2.6831)	(1.5004)	(1.0826)
Plus 1,000 Days	−3.1426	−1.026	−0.4823
	(2.3408)	(1.2115)	(0.9564)
Increases Only:			
All Time Frames	−0.8289	−1.4389	0.8828
	(2.0050)	(2.1768)	(2.3142)
Minus 1,000 Days	−0.6687	7.3003	4.6173
	(3.3293)	(5.8562)	(3.6055)
Minus 2,000 Days	7.6421^{*}	8.3826	10.4409^{***}
	(3.7898)	(7.8289)	(3.9150)
Plus 1,000 Days	−3.4608	-2.4885^{***}	−3.0259
	(6.7926)	(0.4602)	(3.8915)

This table presents results of placebo DiD regressions of log returns of bank stocks on log treatment sizes. These placebo regressions use the actual treatment sizes, while changing the time frame of actual law changes to fictitious time frames. These time frames take the original time frames for each law change and shift them back in time by one thousand days, two thousand days, and shift them forward in time by one thousand days. "All Time Frames" pool all three time frame shifts together. The top panel shows regression results when all tax law changes are pooled together; the bottom two panels show results only for tax decreases and tax increases, respectively. R^2s are pseudo-R^2s for MAD regressions. MAD stands for median absolute deviation. Robust regressions are based on an initial screening for outliers with Cook's distance of greater than one, followed by Li's method of following Huber iterations with biweight iterations in order to determine observation weights, before finally running a weighted regression. All regressions control for the number of trading days in each window as well as the change in the federal funds rate. Standard errors are shown in parentheses. Coefficient estimates marked with one, two, and three stars are significant and the one, five, and ten percent levels, respectively.

A Appendix: Data Collection

In this section, I go into more specific detail about the data collection process. In addition to using *The Book of the States* for identifying tax rate changes, I also use similar spreadsheets from the Tax Foundation as a double check. Both of these sources list many rates that are different than the actual statutory marginal rates firms would eventually pay, due to the simple fact that a large number of tax law changes are made retroactively while these two sources are forward-looking. Many states have additional types of taxes on corporations, such as franchise taxes, but they are deemed to be too small to worry about. The business tax systems of Michigan, Ohio, and Texas are generally ignored because their alternative tax forms are not directly comparable to taxes on net income.

The SIC codes used in identifying potential bank stock for the sample are codes 6021, 6022, 6029, 6035, and 6036. Some additional criteria are used for dropping firms from my sample. Firms are only kept if they are listed on the New York Stock Exchange (NYSE), the National Association of Securities Dealers Automated Quotation exchange (NASDAQ), the NYSE American exchange (f.k.a. the American Stock Exchange or AMEX), and the NYSE Arca exchange (f.k.a. the Archipelago Exchange or ArcaEx). Firms are also dropped if they are accidentally coded with the wrong SIC code, if their business resembles some type of financial service company other that a community bank (such as a credit card processing company), if they are duplicates, if no stock price data is available or if they have less than one year of data available, and if they have operations in foreign countries. In general, stocks are identified with their CRSP permanent number or PERMNO, which is the only key unique to a specific financial instrument. When Compustat data are needed, a crosswalk is made between their PERMNOs and their Committee on Uniform Security

Identification Procedures (CUSIP) numbers.

Banks' 10-K filings on EDGAR are generally a good source for the number of bank branches in each state. In some cases, the firm could not be found on EDGAR, so filings or annual reports are accessed on Morningstar.com or the company's website. The company profile on Yahoo! Finance is used as a last resort. Firms are dropped if no source of location data is found. When firms list the states of their operations but not the number of branches, values for the proportions of income for those states are set to missing while values for states with no operations are set to zero. If a firm lists a string of known locations "and others," or uses some other vague description, the firm is dropped.

Although income is apportioned to each state using a simple assumption of same payroll, property, and sales at each branch, I do heed state nexus throwback rules. Throwback rules work as follows: if a firm is located in a state with a throwback rule and earns income from a state with no corporate income tax, income from the non-taxing state is apportioned to the taxing state, or "thrown back." Schedules of these state-by-state rules are found via the Tax Foundation.

B Appendix: Estimating g and r

Multiple times in this paper, I make use of g, the growth rate of the earnings of bank stocks, and r, the required rate of return on these stocks. Values for these parameters are not readily available, so I estimate them.

To estimate g, I assume that publicly traded regional banks' earnings grow at a constant rate. I identify nineteen states whose top corporate tax rates for financial institutions did not change from 1994-2017 (including zero rate states). I then identify 85 firms in my sample of bank stocks whose income only comes from these nineteen states. This is to ensure that changes in state corporate tax rates do not directly effect these firms' earnings. Using the CompuStat database, accessed through WRDS, I download the daily listings of earnings per share (EPS) for these stocks. These EPS are only updated quarterly, at different dates for different firms. In order to adjust for these timing differences, I collapse the mean of these EPS within each quarter for each stock (essentially smoothing out the EPS). Because the financial crisis beginning in 2007 was an outlier event, I only keep quarterly EPS values up until Q1 2007. I drop the few stocks that have negative values for these smoothed EPS. I then take the logarithm of these smoothed EPS and regress them on an index for each quarter in a time series regression, with fixed effects for each stock. This provides an estimated coefficient on the quarterly index variable of 0.0162, with a p-value of 0.000. This quarterly rate corresponds to an annual rate of 6.7%, which I round down to 6.5% due to the dropped stocks with negative EPS.

To estimate r, I assume prices of bank stocks can be approximated by the aforementioned dividend growth model. For the same set of 85 stocks, I download daily stock prices and EPS from CompuStat. I create a variable which is the current EPS divided by the current price for all stock/day combinations. By equation 2, this variable should be equal to the expression $r - g$. I take the mean of this variable and substitute my estimate of g, yielding a quarterly estimate of r of 0.0321, which corresponds to an annual rate of 0.1349. I also estimate r by rearranging equation 1 so that

$$d_0 = \left(\frac{r-g}{1+g}\right) P.$$

I regress d_0 on P, forcing the intercept to be zero. This provides an estimate of $(\frac{r-g}{1+g}$ of 0.0159 with an R-squared of 0.91. Plugging in my estimate of g, I solve for r, obtaining a quarterly estimate of 0.0325, which corresponds to an annual estimate of 0.1364. I take the two annual estimates of r and round them to 13.5%.

C Appendix: Comparing Tax Law Changes

Comparing different tax laws would be easy if one single new rate went into effect immediately after tax rate change laws were passed. Comparisons are complicated by the fact that most such laws go into effect months after they are passed, and tax rate change laws are often bundles of multiple rates that get phased in over multiple years. To compare these different laws, I use a baseline model that relies on a *ceteris paribus* assumption: that net incomes before state corporate taxes of my sample stocks will keep on their current growth path. This means that the only presumed difference comes from differences between the old top rate(s) and new top rate(s).

I fix the date that a tax law change "happened" as the date the bill was signed into law, rounded to the nearest quarter end. I move the date(s) a law will go into effect to the following January 1st, as the vast majority of stocks in my sample have a December 31st. I create a schedule for each tax law change of the number of quarters left (as of the signing date) under the current top rate, the number of quarters under the first new top rate, the number of quarters under the next new top rate, and so on, until at last there is a "final" rate that continues indefinitely. I make a similar schedule for the top tax rate(s) that were in effect before the new tax law change was made.

I assume a dividend growth model. Normalizing d_0 to unity, I create a column of the next 1,000 quarters of earnings, growing at rate g, assuming no tax. This sum approximates the value of the infinite sum to within a tolerance of 10^{-6}. I create another column that discounts these earnings at rate r. The sum of this second column gives the present value of a prototypical firm's earnings under the assumption of no tax. For each tax law change the sample for which stocks exist with fifty percent or more of operations in the state in question, I make two columns: one for the tax rate τ for each quarter as was scheduled before the law changed, and one with the new rates after the change. I create companion columns for each of these two columns that adjust the original column of earnings by a factor of $1 - \tau$. The sums of these tax-adjusted columns give the present value of a prototypical firm's earnings under the old tax law and the new tax law. The difference of the differences between these sums and the no-tax column's sum gives the percent change, adjusted for the time value of money and future earnings growth. This number is then converted to bps for ease of use. In the case of retroactive tax law changes, a small additional "sweetener" was calculated and added to the after-law-change-column's sum, to take into account the sudden realization of profits from unanticipated increased earnings from quarters past. In future research, it would be interesting to see if such sweeteners are more commonly associated with smaller tax cuts, as this kind of sweetener could be a way to placate people for tax cuts of underwhelming magnitude.