

**As Certain as Debt and Taxes:  
Estimating the Tax Sensitivity of Leverage from Exogenous State Tax  
Changes<sup>\* †</sup>**

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**Abstract**

We use a natural experiment in the form of staggered changes in corporate income tax rates across U.S. states to show that tax considerations are a first-order determinant of firms' capital structure choices. Over the period 1990-2011, firms increase long-term leverage by 114 basis points on average (equivalent to \$62.1 million in extra debt) when their home state raises tax rates. Contrary to standard trade-off theory, the tax sensitivity of leverage is asymmetric: Firms do not reduce leverage in response to tax cuts. Using treatment reversals, we find this to be true even within-firm: Tax increases that are later reversed nonetheless lead to permanent increases in a firm's leverage – an unexpected and novel form of hysteresis. Our findings are robust to various confounds such as unobserved variation in local business conditions or investment opportunities, union power, or states' political leanings. Treatment effects are heterogeneous: Tax sensitivity is greater among profitable and investment-grade firms which respectively have a greater marginal tax benefit and lower marginal cost of issuing debt.

*Key words:* Capital structure, debt policy, leverage, leverage dynamics, taxes, trade-off theory, natural experiments, regression discontinuity.

*JEL classification:* G32.

This paper provides evidence that taxes are a first-order determinant of firms' capital structure choices. It is well known that debt confers a tax benefit on firms as interest payments can be deducted from taxable income. While this tax advantage of debt has been a cornerstone of modern corporate finance since at least Modigliani and Miller (1963), showing that it is empirically relevant has proved challenging, as firms generally differ both in their marginal benefits and marginal costs of debt. We address this identification challenge by exploiting plausibly exogenous variation in corporate income tax rates across U.S. states and time. These tax changes exogenously vary firms' marginal benefit of debt without, as we will show, affecting their marginal cost of debt. We can thus trace out the marginal-cost-of-debt curve for U.S. firms. Its shape turns out to be surprising.

Our first contribution is to empirically confirm the importance of the tax benefit of debt. We exploit the natural experiment offered by staggered changes in state corporate income tax rates using a difference-in-difference approach. Unlike federal tax changes, which occur infrequently and affect all firms simultaneously, many states change their corporate tax rates and they do so at different times.<sup>1</sup> We find that firms increase the amount of debt in their capital structure following an increase in the rate at which their home state taxes corporate income, relative to a set of control firms operating in the same industry at the same time but located in states without tax changes.

The magnitude of the tax sensitivity is economically meaningful. The point estimates show that over the period from 1990 to 2011, firms respond to a tax rise by increasing their long-term leverage by an average of 114 basis points from the pre-treatment average of 18.2%, equivalent to an extra \$62.1 million of debt for the average firm. Our estimates imply a tax elasticity of 0.58. Interestingly, leverage is at least three times more responsive to tax increases than to changes in standard determinants of leverage used in empirical capital structure studies, such as profitability, tangibility, size, and market-to-book. Thus, taxes appear to be a first-order determinant of leverage.

Interestingly, the estimated tax sensitivity is asymmetric. While firms borrow more in response

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<sup>1</sup> Asker, Farre-Mensa, and Ljungqvist (2011) use this natural experiment to model exogenous shocks to firms' after-tax returns on investment but find no effect on investment among stock market-listed firms.

to tax rises, tax cuts do not lead to a corresponding cut in leverage. This is true even within-firm: Tax rises that are later reversed nonetheless increase leverage permanently. This dynamic result suggests that leverage not only responds asymmetrically to tax changes but also is path-dependent and so exhibits hysteresis. Neither asymmetry nor hysteresis has previously been documented.

To provide context, Figure 1a presents the capital structure argument typically found in finance textbooks.<sup>2</sup> The value of a levered firm is equal to the value of the unlevered firm plus the tax benefit of debt minus the (net) cost of debt.<sup>3</sup> The optimal level of debt equates the marginal tax benefit and the marginal cost. To square this textbook “trade-off theory” with the observed asymmetry would require the following modification. Rather than being linear, as trade-off theory assumes, the cost curve has a kink at the firm’s pre-treatment level of debt: Tax-induced reductions in debt appear to be infinitely costly at the margin. This finding is our second contribution. We speculate in the conclusions what might cause leverage to be downward sticky in this way.

To understand our identification strategy, consider the ideal experiment shown in Figure 1b: It consists of randomly assigning different tax rates to firms and then comparing their debt policies to see if higher tax rates lead to higher leverage. Random assignment would ensure that observed differences in leverage could not be caused by unobserved firm-level heterogeneity. This, in turn, would allow us to estimate the marginal-cost curve from shifts in the marginal (tax) benefit curve.

Observational data are, of course, not random. Empirical studies typically relate observed differences in debt policies among firms to differences in their actual tax rates. This approach is fraught with difficulties; it risks falsely attributing observed differences in leverage to differences in taxes when other unobserved differences across firms also likely affect leverage. For example, prior work exploits the fact that higher profits put firms into a higher tax bracket. As a result, high-profit firms may borrow more to take advantage of tax shields, as shown in Figure 1c. But it is equally

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<sup>2</sup> See, for example, Brealey, Myers, and Allen (2011), chapter 18.

<sup>3</sup> Debt is costly due to bankruptcy (Kraus and Litzenberger (1973)) and debt-overhang inefficiencies (Myers (1977)). To isolate the tax benefit of debt, non-tax benefits of debt (e.g., curbing free-cash flow problems (Jensen (1986))) are usually counted as negative costs for expositional purposes (see, e.g., van Binsbergen, Graham, and Yang (2010)).

possible that they borrow more because their default risk is lower than that of low-profit firms.

A simple comparison cannot tell if high-profit firms borrow more because debt offers valuable tax shields or because their marginal cost of debt is lower. Unobserved differences across firms would impart a positive bias in the estimated tax benefit. Figure 1d illustrates the extreme case in which the null hypothesis that taxes have no effect on leverage is true. As drawn, we would falsely reject the null, as all of the observed change in leverage is due to differences in the marginal cost of debt. More generally, in the presence of unobserved differences in marginal costs, the effect of taxes on debt is not identified. This is the challenge our natural experiment is designed to overcome.

We illustrate the essence of our identification strategy with a simple example. In 1991, North Carolina raised its top corporate income tax rate from 7% to 7.75%. Following this tax rise, firms headquartered in NC increased long-term leverage from 19.8% to 21.8% on average. The tax rise is plausibly exogenous from the viewpoint of an individual firm in NC.<sup>4</sup> But this is not sufficient to establish causality since other coincident developments could be responsible for the observed increase in leverage. For example, NC may be home to firms from an industry that suffered some other, non-tax-related leverage-increasing shock in 1991. Or investment opportunities in NC may have changed in 1991 in a way that made an increase in debt desirable, regardless of the tax rise.

To control for such contemporaneous industry- and state-specific developments, we compare leverage changes among North Carolina firms to the contemporaneous changes in leverage among firms that operate in the same industry but are located in states without tax changes in 1991, say in South Carolina. To the extent that SC firms face similar investment opportunities as NC firms, holding industry constant, the contemporaneous change in their leverage provides an estimate of how NC firms' leverage would have evolved absent the tax increase. The difference-in-differences, i.e., the difference (across firms in different states operating in the same industry) of the within-firm change in leverage, gives the desired estimate of the tax sensitivity of corporate debt policy.

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<sup>4</sup> For a start, firms presumably do not lobby for tax increases. (Unions might conceivably do so, but as we will show, this does not appear to be the case.) We will address other potential confounds at length throughout the paper.

The identifying assumption central to a causal interpretation of our diff-in-diff estimates is that treated and control firms are only randomly different. This requires that residual variation in state tax changes, conditional on a set of control variables, be uncorrelated with unobserved determinants of leverage. Our results cannot be confounded by unobserved time-varying industry shocks (as we include industry-year fixed effects), by unobserved time-invariant firm heterogeneity (as we first-difference the data), or by firm-level variation in performance or characteristics (as we condition on standard firm-level determinants of leverage). They are also robust to including state fixed effects.

The only remaining type of omitted variable that could confound our results is one that varies within states across time and so is collinear with the dimension of the tax-change treatment. For example, if a state suffers a recession and revenues fall, it may increase corporate taxes to balance its budget. In response to the same recession, firms in that state may have to borrow more to support their operations. In that case, our estimate of the tax effect would be upward biased: We would wrongly conclude that taxes affect leverage, when in truth local conditions determine both.

We address this important concern in four ways. First, we show that states do not raise corporate taxes in response to changes in local conditions (such as state growth or unemployment rates). Second, we show that observed variation in local conditions cannot explain observed leverage changes. Third, we actually find *stronger* treatment effects when we restrict control firms to those located in states bordering a treated state. To the extent that firms in neighboring states share similar economic conditions, this result suggests that instead of leading us to overestimate the sensitivity of debt to tax rises, unobserved variation in local conditions biases our estimates downward. Fourth, a sharp regression-discontinuity test using firms located in adjacent counties on either side of a state border confirms this: The estimated tax sensitivity more than doubles, to about 250 basis points. We show that this increase reflects a tendency for tax rises to coincide with unobserved changes in local conditions that would otherwise cause firms to reduce their leverage absent the tax change. By implication, there must be strong geographic clustering in corporate debt policies, even absent tax

shocks. Such clustering has not previously been documented. This is our third contribution.

Our estimates of the tax sensitivity of debt are likely conservative. The reason is that firms are taxed in every state they have a substantial connection (“nexus”) with, in the form of facilities, staff, or sales. Detailed data on a firm’s nexus are not available, which is why we focus on tax changes in a firm’s HQ state. The resulting measurement error will attenuate the estimated tax sensitivity, to the extent that sample firms have operations outside their HQ state. We confirm this using two falsification tests. The first documents that multinationals show no tendency to respond to state tax changes, while domestic firms respond strongly. The second exploits industry-level variation in the extent to which firms ship their products outside their home state. Here, we find stronger tax effects for firms in industries with low inter-state sales. Both tests are consistent with the expected attenuation bias – but only for tax increases. We continue to find an asymmetric tax sensitivity, suggesting that the absence of a leverage response to tax cuts is not due to measurement error.

Theory suggests that the value of tax shields varies with the interplay of personal and corporate taxes (Miller (1977)), profitability, and debt capacity. This suggests three validation tests. The first exploits Miller’s insight that high personal tax rates on equity income should dampen the impact of a corporate tax change on leverage. Using two proxies for personal taxes that vary in the cross-section, we find evidence to support this comparative static. The second test shows that unlike profitable firms, loss-making firms do not borrow more in response to tax rises. This is consistent with a link between taxes and leverage since loss-making firms have no profits to shield from taxes. The third test shows that the sensitivity of debt to tax increases is concentrated among investment-grade firms (which have flatter marginal-cost curves) and entirely absent among firms rated junk. Each of these validation tests supports a causal interpretation of the observed tax sensitivity of debt.

We proceed as follows. Section 1 situates our paper in the literature. Section 2 outlines our empirical strategy. Section 3 provides an overview of state corporate income taxation in the U.S. Section 4 describes the data and Section 5 presents our empirical results. Section 6 concludes.

## 1. Related Literature

While the literature on taxes and capital structure is vast (Graham's (2008) survey cites more than 200 published articles), ours is the first study to exploit changes in U.S. states' corporate taxes over time. This quasi-experimental setting has the potential to offer a clean causal interpretation of the estimated effect of taxes on firms' capital structure decisions.

The early empirical literature found inconclusive results, which led Myers (1984) to remark that "I know of no study clearly demonstrating that a firm's tax status has predictable, material effects on its debt policy." Taking up this challenge, MacKie-Mason (1990) and Graham (1996a, 1996b) find a significant positive relation between estimates of a firm's estimated or simulated marginal tax rate and its debt policy. However, Fama and French (1998) caution that cross-sectional studies are vulnerable to endogeneity biases as firms' effective tax rates may correlate with omitted variables.

To clarify our contribution, we briefly discuss prior attempts at exploiting variation in tax rates to identify the tax sensitivity of debt, beginning with cross-country studies. Rajan and Zingales (1995) find that firms in countries with higher corporate tax rates use more debt. Similarly, Booth et al. (2001) find a positive relation between country-level tax rates and country averages of leverage in a sample of 17 countries. Faccio and Xu (2011) use variation in tax rates across and within 29 OECD countries to show that leverage increases with taxes only in countries with little tax evasion.

A common concern in cross-country studies is that treated and control firms are located in different countries and so may differ in ways that affect their debt policies. It is debatable whether it is reasonable to assume that, say, a South Korean carmaker is a valid control for a Swiss pharma company, in the specific sense that both share the same marginal cost function (see Figure 1c).

Single-country studies can potentially sidestep this problem. A popular exogenous shock is the Tax Reform Act of 1986 (see Gordon and MacKie-Mason (1991), Givoly et al. (1992), and van Binsbergen, Graham, and Yang (2010)).<sup>5</sup> But since this change in federal taxes affected all firms at

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<sup>5</sup> Lin and Flannery (2012) study the 2003 Bush cuts in personal taxes, finding an effect on firm leverage.



(roughly) the same time, there is no obvious control group with which to disentangle the impact of the Act from other concurrent changes that could affect debt policies (such as changes in interest rates, inflation, the business cycle, or financial regulation).<sup>6</sup> Using state-level tax changes, which are staggered across states and time, thus provides potentially cleaner identification.

## 2. Empirical Strategy

We examine the effect of changes in states' corporate income tax rates on firms' use of debt using a difference-in-difference regression approach of the form:

$$\Delta D_{ijst} = \beta \Delta T_{st-1}^+ + \gamma \Delta T_{st-1}^- + \delta \Delta X_{it-1} + \theta \Delta Z_{st-1} + \alpha_{jt} + \varepsilon_{ijst} \quad (1)$$

where  $i, j, s,$  and  $t$  index firms, industries, states, and years;  $\Delta$  is the first-difference operator;  $D_{ist}$  is a measure of debt usage;  $\Delta T_{st-1}^+$  and  $\Delta T_{st-1}^-$  are indicators equaling one if state  $s$  increased or cut its corporate tax rate in year  $t-1$ , respectively;  $X_{it-1}$  and  $Z_{st-1}$  are firm- and state-level control variables;  $\alpha_{jt}$  are industry-year fixed effects; and  $\varepsilon_{ijst}$  is the usual error term. The coefficients of interest,  $\beta$  and  $\gamma$ , capture the effects of tax increases and tax cuts on firms' debt usage. First-differencing removes unobserved firm-specific fixed effects in the corresponding levels equation, while including industry-year fixed effects allows us to remove unobserved industry shocks.<sup>7</sup>

Regression (1) generalizes the illustrative example in the introduction in three ways. First, it exploits variation in taxes across many states and years, rather than just North Carolina's 1991 tax increase. For any change in corporate income tax in state  $s$  at time  $t$ , the potential control states are all those states that did not change their corporate income tax rates at that time (though we will also consider finer control sets). Second, regression (1) allows for covariates that vary at the firm- or state-level and over time. For example, we can control for time-varying factors at the state level that may be correlated with changes in both state taxes and firm leverage, while firm-level covariates

<sup>6</sup> Van Binsbergen, Graham, and Yang (2010) cleverly exploit slight timing differences in exposure to the 1986 tax reform due to variation in firms' fiscal-year ends.

<sup>7</sup> This is preferable to including average industry leverage as a regressor, as is often done in the capital structure literature. Gormley and Matsa (2012) show analytically that accounting for unobserved group-level heterogeneity by including the group average of the dependent variable as a control can lead to bias. To ensure consistency of the parameters of interest, models should instead include group fixed effects (here: industry-year fixed effects).

control for other firm-level determinants of debt policies. Including industry-year fixed effects allows us to compare treated and control firms within the same industry at the same point in time. Third, the regression distinguishes between tax increases and tax cuts. Hence, we can discriminate between a classic, symmetric tax benefit of debt and an asymmetric one.

The key identifying assumption is that conditional on covariates  $X_{it-1}$  and  $Z_{st-1}$  and on industry-year fixed effects, the tax change in state  $s$  is as good as randomly assigned. Put differently, the estimates of  $\beta$  and  $\gamma$  in regression (1) give the causal treatment effects of tax increases and tax cuts on debt as long as any omitted determinants of capital structure (which are left in the error term  $\varepsilon_{ijst}$ ) are uncorrelated with state-level tax changes. Given our set-up, this identifying assumption can only be violated by confounds that vary at the state-year level.

### 3. State Corporate Income Taxes

#### 3.1 Overview

Most states tax corporate activities within their borders,<sup>8</sup> and most do so using a tax on profits.<sup>9</sup> Firms are subject to state taxes if they have “nexus” with a state, usually meaning they derive income from sales in the state, have employees in the state, or own or lease property in the state.<sup>10</sup> In 2012, top marginal tax rates vary from a low of 4.63% in Colorado to a high of 12% in Iowa. They have also varied considerably over time, and it is this variation that we exploit to identify the tax sensitivity of corporate debt policies. We first discuss tax increases.

#### 3.2 Tax Increases

Using data obtained from the Tax Foundation, a think tank, and the “Current Corporate Income Tax Developments” feature published periodically in the *Journal of State Taxation*, Appendix A

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<sup>8</sup> The exceptions, as of 2012, are NV, SD, and WY. See <http://www.scribd.com/doc/84126982/state-corp-income-rates-2000-2012-20120216>.

<sup>9</sup> The exceptions, as of 2012, are OH, TX, and WA, which use a gross receipts tax assessed on revenue rather than on income. See <http://www.scribd.com/doc/84126982/state-corp-income-rates-2000-2012-20120216>.

<sup>10</sup> States distinguish between multi-state firms (those with nexus with more than one state) and single-state firms. Under the Uniform Division of Income for Tax Purposes Act, multi-state firms pay taxes in each state they have nexus with, at rates and on terms determined by each state. Typically, states apportion the net income of a multi-state firm using three weights: The ratio of the firm’s sales in the state to its total sales, the ratio of the firm’s payroll in the state to its total payroll, and the ratio of the firm’s property in the state to its total property.

lists 38 tax increases in 25 states affecting 1,824 firms in 1990-2011. For example, in 1999, New Hampshire increased its top rate from 7% to 8%. The average shock increases top rates by 90 basis points, or 13% relative to the previous year's top rate (though as more firms are located in states with larger tax rises, the average treated firm experiences an increase of 1.24 percentage points).

To put these numbers into perspective, consider the implications for firms' tax bills. In the year before a tax increase, the average (profitable) sample firm headquartered in that state earns pre-tax income of \$237.5 million. Relative to this baseline, state corporate income tax increases would cost the average (profitable) firm an additional \$2.78 million in taxes a year,<sup>11</sup> absent a response, or a total of \$3.8 billion across all treated (profitable) firms.

Eighteen of the 38 tax rises occurred in the 1990s and 20 in the 2000s. (Our results are nearly identical in either decade.) Figure 2 maps affected states over consecutive five-year periods to show the geographic and time-series distribution of the tax shocks. Geographically, there is little clustering: There are only seven neighboring states that raise taxes at the same time (KY-WV-MO-NE-OK in 1990 and TN-KY in 2002). There is somewhat more clustering over time: The busiest quinquennia are 1990-1994 and 2000-2004, which hints at a possible link between recessions and tax increases (though there have been surprisingly few tax increases in the wake of the 2007-8 financial crisis and subsequent recession).

Clearly, states do not change tax rates in a vacuum. This will not affect identification unless the reasons they do so simultaneously affect corporate debt policies. For example, Figure 2 suggests that tax rises may coincide with economic downturns. If firms also borrow more in downturns, this could lead to a spurious (rather than causal) correlation between taxes and leverage. Alternatively, corporate tax increases may reflect strong union power in the state. This could lead to a spurious correlation if firms use leverage strategically when bargaining with their unions, as Matsa (2010) argues. Finally, corporate tax changes could coincide with changes in personal taxes that could

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<sup>11</sup> Or up to 35% less if the firm has a sufficiently large federal tax bill from which to deduct its increased state taxes.

either amplify or attenuate the effects of corporate taxes on leverage (assuming that the holders of a firm's debt and equity are mainly located in the firm's headquarter state). For example, Miller (1977) shows that higher personal taxes on interest income reduce the value of debt tax shields.

To investigate these concerns, Table 1 relates the probability that a state changes its top corporate income tax rate in year  $t$  to lagged real growth in gross state product (GSP), the state's lagged unemployment rate, the lagged fraction of the state's private sector employees who belong to a union, and changes in a state's personal taxes on wages and on long-term capital gains. (For all variable definitions and details of their construction, see Appendix C.) We also control for local political conditions using the share of votes cast for the Democratic candidate in the most recent prior Presidential election. We estimate linear probability models with year indicators and state fixed effects and cluster the standard errors at the state level.

Column 1 focuses on tax increases. Neither real GSP growth nor state unemployment has any effect on the probability that a state raises its corporate income tax. To illustrate, a one-standard-deviation worsening in real GSP growth or state unemployment is associated with only a 0.1 percentage point change in the likelihood of a tax rise, which is economically small relative to the unconditional likelihood of 3.4%. The same applies to union membership: The estimated coefficient is 0.001 for a one-standard deviation effect of 0.6 percentage points ( $p=0.811$ ).

A state's political leanings, on the other hand, have a large effect: A one-standard deviation increase in the share of the vote won by the Democratic candidate in the previous Presidential election is associated with a 6.2 percentage-point greater likelihood that the state subsequently raises corporate taxes ( $p=0.011$ ). This suggests, perhaps not surprisingly, that left-leaning states tax their corporations more aggressively. If, for whatever reason, firms respond to increasing Democratic support in their home state by taking on more debt, it is possible that the observed positive correlation between tax rises and leverage increases is not causal. Since changes in political leanings are observable, we can investigate this possible confound directly. As we will show, our

results are robust to controlling for observed differences in political leanings across states.

Finally, we find no evidence that changes in corporate state taxation coincide with changes in personal state taxes on income or capital gains.<sup>12</sup> Overall, tax changes appear largely idiosyncratic.

### *3.3 Tax Cuts*

Over the 1990-2011 period, we count 67 state corporate income tax cuts in 29 states affecting 7,021 firms (see Appendix B for details). For example, in 2001, Arizona cut its top rate from 7.968% to 6.968%. On average, tax rates are cut by 60 basis points. In the year before a tax cut, the average (profitable) firm earns pre-tax income of \$235.1 million. Relative to this baseline, state corporate income tax cuts would save the average (profitable) firm \$1.2 million in taxes a year, all else equal, or a total of \$4.97 billion across all treated (profitable) firms.

Thirty-four of the 67 tax cuts occurred in the 1990s and 33 in the 2000s. Figure 2 shows the geographic and time-series distribution of these tax shocks. Tax cuts are spread out fairly evenly across time and like tax increases do not tend to cluster geographically.<sup>13</sup>

Column 2 of Table 1 relates the probability that a state cuts corporate tax rates to local economic and political conditions and changes in personal state taxes. The only variable with a significant effect (and even then only at the 10% level) is lagged GSP growth: States are more likely to cut taxes, the higher their growth rates in the previous year. Economically, the effect is relatively small: A one-standard deviation increase in lagged GSP growth is associated with a 1.4 percentage-point increase in the probability of a tax cut, relative to the unconditional probability of 5.8%.

## **4. Sample and Data**

### *4.1 Sample*

Our sample consists of all U.S. companies traded on the NYSE, Amex, or Nasdaq over the

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<sup>12</sup> The Table 1 regression uses lagged changes in state taxes on income and capital gains. We find the same result if we use contemporaneous changes in state taxes on income and capital gains, but we lose one year since the personal-tax data we use is currently only available through 2010. See <http://users.nber.org/~taxsim/state-rates>.

<sup>13</sup> Of the 67 tax cuts, 51 occur in states whose neighbors do not cut their taxes in the same year. The remaining 16 form seven mini clusters: NJ-NY-PA (1994), PA-NY-CT (1995), NY-CT (1999), AZ-CO (2000), NY-CT, KY-OH (2005), and KY-WV (2007).

period 1989-2011 satisfying the following filters. From the merged CRSP-Compustat Fundamentals Annual database, we exclude financial firms (SIC=6; 42,970 observations), utilities (SIC=49; 4,939 observations), public-sector entities (SIC=9; 1,268 observations), non-U.S. firms (13,895 observations), and firms traded OTC or in the Pink Sheets (1,772 observations). We also drop firm-years with negative or missing total assets (224 observations) or missing return on assets (583 observations), and firms with a single panel year (885 observations) or a CRSP share code >11 (REITS etc.; 2,978 observations). Finally, while cleaning up firms' headquarter states (see below), we filter out 986 observations of firms that were headquartered outside the U.S. The final sample consists of 91,172 firm-years for 10,105 firms (though the need to lag certain variables as well as gaps in the panel structure of some firms will reduce the sample size used in our regressions).

#### *4.2 Firms' Use of Debt*

Table 2 reports summary statistics for sample firms' use of debt and for our control variables. (For all variable definitions and details of their construction, see Appendix C.) There are many ways to measure how much debt a firm uses to fund its operations. Most studies use a leverage measure, though definitions of leverage vary along two dimensions: Book vs. market leverage and the maturity of debt that is included. Some studies use the sum of short-term and long-term debt over total assets, while others focus on long-term leverage.

As we will show, our results are robust to using any of these measures, but there are good reasons to expect long-term leverage to be the most sensitive to tax changes. Short-term debt is used mostly for working capital needs and so is unlikely to be altered in response to tax changes, a conjecture that proves to be true in the data. Thus, we focus on long-term debt. This, in turn, can be measured with or without the portion of long-term debt that is due within a year and so is classified as short-term debt. When tax rates increase, firms can respond by issuing long-term debt, but they cannot increase the "current" portion of their existing long-term debt, which instead varies mechanically with the passage of time as a debt facility nears maturity. This suggests that we should

focus on changes in long-term debt excluding debt due within a year.

Finally, we prefer to model book leverage because firms have greater control over book leverage (which is a function of debt outstanding and the size of the balance sheet) than over market leverage (which in part reflects share prices). Thus, book leverage is a cleaner measure of debt policy, though as we will show, our results are robust to modeling market leverage instead. As Table 2 shows, long-term book leverage averages 17.2% in our sample (18.2% before a tax rise).

Because leverage measures are ratios, variation in leverage could capture variation in the denominator (the book or market value of assets) rather than the numerator (debt). It is therefore useful to model not just leverage but also debt levels. Table 2 shows that the average sample firm has long-term debt of \$383.9 million.

#### *4.3 Control Variables*

We control for the standard financial variables commonly found in empirical models of debt (see, for example, Frank and Goyal (2009)): Profitability (return on assets), firm size (total assets), tangibility (the ratio of fixed to total assets), and investment opportunities (market-to-book). As Table 2 shows, the average sample firm has ROA of 3.4% and \$1,676.5 million in total assets, 26.4% of which is tangible, and trades at a market-to-book ratio of 1.841. In addition, we use the default spread (the difference between the yield on Baa and Aaa rated corporate bonds) to control for conditions in the credit markets. In the average firm-year, this measures 95.5 basis points. Finally, some specifications control for economic conditions in a firm's home state using the growth in gross state product (GSP), the state unemployment rate, and a proxy for the state's sales growth rate. These average 2.9%, 5.8%, and 18.9%, respectively.

Table 2 also shows firm- and state-level conditions one year before a tax rise or tax cut. This reveals a slow-down in GSP growth, lower profits, and higher default spreads ahead of a tax rise.

#### *4.4 Firm Headquarter Locations*

Compustat's location data suffer from a major flaw: Compustat reports the address of a firm's

*current* principal executive office, not its historic headquarter location. Many studies ignore this source of measurement error, treating it as noise. However, in our setting, it is more likely to induce bias. If the null of no association between tax and leverage is false, false negatives (firms that are in fact located in a tax-change state but appear not to be) will reduce the estimated tax sensitivity, as their leverage changes despite the (apparent) absence of a tax change. Similarly, false positives (firms that appear to be located in a tax-change state but in fact are not) will seem to fail to respond to a tax change (though of course there was none). This will bias the tests in favor of a false null.

To remedy this, we extract historic headquarter states for each firm-year in our sample from regulatory filings. Specifically, for each fiscal year, we look up each sample firm's headquarter state as listed in the firm's most recent 10-Q prior to the fiscal year-end using the SEC's EDGAR service (mostly, from May 1996 onwards) and Thomson Research (between 1990 and May 1996).

Errors prove widespread, affecting a non-trivial fraction of the Compustat universe. Overall, Compustat's HQ state information is incorrect in 9,246 firm-years (10.1% of the total) affecting 1,532 firms (15.2% of all non-financial and non-utility U.S. firms in Compustat).<sup>14</sup> Not surprisingly, the problem gets worse the further back in time we go. Figure 3 shows the annual fraction of sample firms whose historic HQ state is misrecorded in Compustat. Using a download dated August 2010 (covering fiscal years 1990-2009), we see that 1% of firms' HQ states are misrecorded in fiscal year 2009, rising monotonically to 16.6% in fiscal year 1991. Thus, where firms are today is often quite different from where they were a decade or two ago.<sup>15</sup>

Importantly, cleaning up firms' HQ locations allows us to remedy 141 false positive and 186 false negative tax increases and 505 false positive and 568 false negative tax cuts.

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<sup>14</sup> A new database, the WRDS SEC Analytics Suite, aims to provide users "historical information on state of incorporation and headquarters", among other items. SEC Analytics appears to pull HQ information not from the filing itself, but from EDGAR's "filing detail page." Unfortunately, this page is frequently out of date for years at a time, apparently because the SEC does not update its database on firm locations in a timely fashion. SEC Analytics also has problems matching filings to the correct gvkey, for example (but not exclusively) when two firms merge. As a result, SEC Analytics misses around one third of the corrections we make to Compustat's HQ location variable.

<sup>15</sup> There is a further twist: In 2010 and 2011, the error rate is actually higher than in 2009, at 5.6% and 4.3%. This reflects the fact that Compustat now frequently fails to record a firm's headquarter state altogether.



## 5. Empirical Results

### 5.1 Graphical Evidence

Figure 4 tests graphically whether firm leverage responds to tax changes. It plots the average annual within-firm change in long-term leverage in years  $t = -2$  to  $t = +2$  for the group of firms experiencing a corporate income tax change in their home state at  $t = 0$  ('treated' firms) and, for comparison, the group of firms not subject to a tax change in their home state ('control' firms). We remove time-varying changes in industry conditions by including industry-year fixed effects.

Figure 4a shows responses to tax rises. In the two years before a tax rise, leverage changes are tiny and statistically insignificant for both treated and control firms, suggesting there are no pre-trends to worry about. In the year of the tax rise, neither group adjusts leverage much. In year  $t+1$ , on the other hand, we see sizeable and significant increases in leverage among treated firms, averaging 105 basis points ( $p < 0.001$ ), while the leverage of control firms falls by an insignificant 13 basis points on average. The diff-in-diff estimate of 118 basis points is highly significant ( $p < 0.001$ ). It is consistent with the interpretation that firms respond to higher taxes in their home state by borrowing more, with a one-year lag. The effect is sizeable: Relative to the unconditional pre-increase mean of 18.2% (see Table 2), firms increase their long-term leverage by 6.5% following a tax rise ( $= 0.0118 / 0.182$ ). This is equivalent to \$64.4 million more debt per firm on average.<sup>16</sup> There is little evidence that firms subsequently reverse these leverage increases in year  $t+2$ .

The response to tax cuts, shown in Figure 4b, is quite different. Neither treated nor control firms change their leverage by much, if at all, in the five years surrounding tax cuts. In year 0, affected firms actually *increase* leverage, by 12 basis points relative to unaffected firms. In the next two years, they decrease leverage a bit, by 16 and 11 basis points for a total reduction over the three years of 14 basis points. None of these diff-in-diff estimates is statistically significant.

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<sup>16</sup> Assuming equity ( $E$ ) remains constant, an  $x$  percent increase in leverage implies that debt  $D$  increases by the amount  $\Delta D \equiv D_1 - D_0 = \frac{(1+x)D_0E}{E - xD_0} - D_0$ .

## 5.2 Conditional Estimates of the Tax Sensitivity of Debt

The changes in leverage illustrated in Figure 4 could potentially be driven by coincident changes in firms' financial characteristics that are unrelated to the tax changes. To control for these, Table 3 reports standard leverage regressions estimated using OLS in first-differences (to remove time-invariant unobserved firm heterogeneity) and including a full set of SIC4-year effects (to remove the effects of unobserved time-varying industry shocks).<sup>17</sup>

The variables of interest in column 1 are the two indicators for state tax increases and state tax cuts. These capture the treatment effects of signed tax changes on corporate debt policies relative to firms in the same industry that are not subject to tax changes in their headquarter states that year, conditional on a set of control variables.<sup>18</sup> Standard errors are clustered at the firm level; later, they will be validated using randomly generated pseudo shocks.

The results show that firms increase long-term leverage by 114 basis points on average in response to a tax rise ( $p < 0.001$ ), relative to other firms in the same industry at the same time. The estimated treatment effect is nearly identical to the unconditional increase of 118 basis points in Figure 4a. This indicates that the covariates we control for in the regression change little around the time states increase their corporate taxes. In fact, these standard determinants of leverage have quite modest economic effects: One standard deviation changes in profitability, size, tangibility, or market-to-book are associated with at most a 30 basis point change in leverage. This is no more than a quarter of the observed sensitivity to tax rises. By contrast, a 114 basis-point increase in leverage is economically meaningful. Relative to the average pre-treatment leverage ratio of 18.2%, it represents an increase of 6.3% ( $= 0.0114 / 0.182$ ) or \$62.1 million in extra debt on average.<sup>19</sup>

To assess the impact of cleaning up firms' historic HQ locations, we estimate (but do not report)

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<sup>17</sup> The industry-year effects also capture nationwide shocks that affect all industries at the same time.

<sup>18</sup> Put differently, we compare the change in industry-adjusted leverage of treated firms to the change in industry-adjusted leverage of control firms that are located in other states, holding covariates constant.

<sup>19</sup> As a reality check, we compare the average annual tax savings on \$62.1 million in additional debt to the \$2.78 million average increase in firms' tax bills absent a leverage response (see Section 3.2). Assuming an effective marginal tax rate of 30.5% (Graham and Mills (2008)) and a coupon of 5%, the resulting annual tax saving is \$2.2m ( $= 0.05 * (1 - 0.305) * \$62.1m$ ). This is in the same ballpark as the tax increase, with the difference representing expected distress costs.

leverage regressions using Compustat’s “current” locations. This yields an estimated sensitivity to tax increases of 87 basis points, 27 basis points below the “true” estimate of 114 basis points shown in column 1. This confirms that measurement error in firms’ HQ locations leads to attenuation bias.

Could the observed sensitivity to tax increases simply be random? The standard errors suggest not, but an alternative way to answer this question is to generate “pseudo shocks” as in Bertrand, Duflo, and Mullainathan (2004). Specifically, we randomly generate 1,000 sets of 38 “pseudo tax increases” and 67 “pseudo tax cuts” (to match the observed number of actual tax shocks). Since the pseudo shocks are random, we know that the null of no tax sensitivity is true. Indeed, the mean of the 1,000 estimates of the effect of the pseudo tax increases or pseudo tax cuts on leverage is zero. More interestingly, we *never* see coefficients as large as those estimated using the actual tax increases. Thus, based on these simulations, there is a zero in 1,000 chance of randomly observing the Table 3 coefficients when the null of no tax sensitivity is in fact true. This suggests that the clustered standard errors in Table 3 are, if anything, slightly conservative.

Next, we consider variation in the magnitude of each state’s tax change instead of using tax change indicators. Column 2 regresses changes in long-term leverage on changes in top marginal tax rates.<sup>20</sup> The results mirror those in our baseline model: Leverage increases as tax rates go up ( $p=0.063$ ). The coefficient estimate of 0.347 allows us to compute the elasticity of debt with respect to taxes,  $\frac{\partial L / \partial \tau_c}{L / \tau_c}$ . The numerator is our coefficient estimate. To compute the denominator, we use the average firm’s leverage of 18.2% from Table 2 for  $L$  and Graham and Mills’ (2008) estimate of 30.5% for the average firm’s effective marginal tax rate. This gives a tax elasticity of 0.58.

### 5.3 Asymmetry: Sensitivity to Tax Cuts

In stark contrast to firms’ responses to tax rises, we find no evidence that firms cut leverage in response to tax cuts. Column 1 shows an average tax-cut treatment effect of minus 3.6 *basis* points.

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<sup>20</sup> Three tax increases (CA 2002, NJ 2002, and MI 2008) and one tax cut (TX 2008) cannot be summarized in terms of changes in marginal tax rates (though their effects on tax shields are unambiguous; see Appendix A and B). Treated firms affected by these four tax changes are dropped from this regression.

This has the expected negative sign but is economically tiny and statistically insignificant ( $p=0.826$ ). Conditioning on the size of the tax cut, in column 2, also yields a small and insignificant effect. These patterns mirror Figure 4b. They suggest that the tax sensitivity of debt is asymmetric: Firms increase leverage when taxes rise but apparently do not reduce leverage when taxes are cut.

Columns 1 and 2 include all treated firms regardless of the type of treatment they experienced. Thus, it is theoretically possible that firms suffering a tax increase are in some unobserved way different from firms experiencing a tax cut and that it is this unobserved difference that accounts for the asymmetry. Column 3 adds firm fixed effects to the first-difference specification (alongside the industry-year effects already included). It thus controls for time-invariant unobserved heterogeneity across firms with regard to changes in leverage. The coefficient for tax increases hardly changes and the effect of tax cuts remains insignificant, so we continue to find an asymmetric tax sensitivity.

Including firm fixed effects goes some way towards ruling out spurious asymmetry, but our data permit an even stronger test. We can restrict the treatment sample to firms experiencing treatment reversals, meaning those that first face a tax increase and then, some time later, a tax cut (possibly in another state if they have moved in the meantime). There are 490 such firms in our sample. Using this treatment group and again including firm fixed effects, column 4 shows evidence of a form of dynamic asymmetry: When hit with a tax rise, firms increase their leverage strongly and significantly but when later experiencing a tax cut, the same firms fail to reduce leverage again.<sup>21</sup>

Figure 5a illustrates the implications of these findings graphically. The results in columns 1-3 of Table 3 trace out a marginal-cost-of-debt curve that is positively sloped above the pre-treatment level of debt and infinitely sloped below the pre-treatment level of debt. Standard trade-off theory, shown earlier in Figure 1a, predicts no kink in the marginal cost of debt. To square trade-off theory with our evidence would require the modification shown in Figure 5b: The total net cost of debt is upward sloping and convex above the “optimal” debt level, as in trade-off theory, but flat below it.

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<sup>21</sup> It is not the case that the subsequent tax cuts are simply too small to respond to. In fact, the average tax cut in the reversal sample measures 64 basis points, a little more than the unconditional average cut of 60 basis points.

Figure 5c illustrates the dynamics of capital structure based on the treatment-reversal results in column 4 of Table 3. A firm that experiences an increase in its marginal tax benefit increases its debt from D to D'. A subsequent decrease in its marginal tax benefit leaves debt unchanged at D'. This implies that the flat segment of the total cost function moves up from C to C' so that the kink in the marginal cost curve moves up and to the right with each tax increase, forever leaving the firm at the kink. Leverage is thus downward sticky and tax increases appear to ratchet it up permanently.

Of course, firms do cut leverage in practice – though apparently not in response to tax cuts. This suggests that reductions in leverage, when they occur, reflect not changes in the marginal tax benefit of debt but changes in the marginal cost of debt (e.g., because firms' debt capacity has changed).

The apparent irreversibility of tax-induced leverage increases is a novel form of hysteresis that has not previously been documented. It implies that tax rises – but not tax cuts – leave permanent marks on firms' capital structures. To test this, we estimate a cross-sectional leverage regression in levels, using only the last panel year  $\tau$  for each firm.<sup>22</sup> The variables of interest count how many tax rises and tax cuts a firm has experienced since the start of our panel in 1990 (or since going public, if later). The results, including standard controls and industry-year fixed effects, are as follows:

$$\begin{aligned}
 leverage_{i\tau} = & \underset{(0.006)}{0.010} number\_tax\_increases_{i\tau-1} + \underset{(0.002)}{0.000} number\_tax\_cuts_{i\tau-1} \\
 & - \underset{(0.009)}{0.018} ROA_{i\tau-1} + \underset{(0.016)}{0.173} tangibility_{i\tau-1} + \underset{(0.002)}{0.016} size_{i\tau-1} - \underset{(0.001)}{0.003} market\_to\_book_{i\tau-1} \\
 & + \underset{(0.017)}{0.011} default\_spread_{i\tau-1} + v_{jt}
 \end{aligned}$$

$R^2 = 54.5\%$ 
 $No. obs = 7,819$

The estimates confirm that the echoes of past tax increases (but not of tax cuts) can still be felt in today's capital structures: All else equal, a firm's leverage is one percentage point higher for every tax increase it has experienced since 1990 or going public ( $p=0.064$ ). Interestingly, this point

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<sup>22</sup> To ensure firms were able to react to tax increases during their time in our panel, we restrict the sample to firms that are neither in financial distress nor have a junk credit rating.

estimate is nearly identical to the treatment effects estimated in Table 3. We discuss possible explanations for why firms may respond asymmetrically to tax changes in the conclusions.

#### 5.4 Pre-trends and Drift

Column 5 of Table 3 considers the timing of the effect of tax rises on leverage. As in the corresponding univariate results shown in Figure 4, we see that firms increase leverage with a one-year lag rather than contemporaneously. There is little sign of a reversal two years after a tax increase (nor in subsequent years; not shown), indicating that the increase in leverage that follows a tax increase is persistent. (We already know that not even a subsequent tax cut will reverse it.)

To test for pre-trends, column 5 also includes two leads. Their coefficients are fairly small (at  $-7.6$  and  $-41$  basis points) and far from statistically significant. This has three important implications. First, pre-trends do not differ significantly between treated and control firms. This is important for identification, since diff-in-diff estimators attribute any differences in trends between treated and control firms that coincide with the tax change to that tax change. So if treated and control firms started off on different trends, estimates could be biased. Second, the absence of significant lead effects means that treated firms do not anticipate future tax changes. One interpretation of this is that even if firms know about tax increases in advance, they do not increase leverage before they can actually reap the benefits of the increased tax shield. Third, the fact that leverage increases only *after* tax rises suggests that this relation is not the result of state lawmakers simply responding to deteriorating economic conditions (an omitted variable) or increases in leverage (reverse causality). Instead, we see firms reacting only once they can take advantage of the increased tax shields.

Finally, we explore whether the failure to respond to tax cuts simply reflects delays, perhaps caused by adjustment costs incurred in reducing leverage. Such delays would imply that the tax-cut coefficient in our earlier regressions understates the full effect of tax cuts on leverage. However, this does not appear to be the case: The coefficients for the first four lags of the tax cut indicator are tiny at  $-9$ ,  $+9$ ,  $+2$ , and  $-12$  basis points for a net four-year decrease of only 10 basis points (not

shown). Thus, we find no evidence that firms react to tax cuts with any kind of reasonable lag.

### 5.5 Robustness

Table 4 reports key robustness tests. First, to assess possible structural breaks, columns 1 and 2 partition the sample by decade. This reveals a modest (and insignificant) increase in the sensitivity of debt to tax rises over time: The estimated diff-in-diff is 109 basis points in 1990-1999 ( $p=0.025$ ) and 116 basis points after 2000 ( $p=0.002$ ). Leverage is insensitive to tax cuts in both subsamples, so the tax sensitivity is asymmetric throughout our sample period. Because we obtain nearly identical results in both periods, our results cannot be driven by any single tax-change event.

We next investigate if the observed tax sensitivity might be due to unobserved time-invariant differences between states. If firms choose where to locate based on unobserved state attributes that correlate with their debt policies, we should not compare, say, Michigan firms suffering a tax shock to control firms in, say, Utah. The solution is to include state fixed effects alongside the industry-year effects used in our baseline specification. Column 3 shows the results. Including state fixed effects barely changes the sensitivity of leverage to tax rises: The diff-in-diff estimate of 112 basis points is only marginally lower than the 114 basis points shown in Table 3 and continues to be highly statistically significant ( $p<0.001$ ). Similarly, tax cuts continue to have no effect on leverage.

Column 4 includes short-term debt in the dependent variable and so models changes in *total* leverage. Consistent with our conjecture that firms respond to tax changes primarily on the long-term debt margin rather than by changing short-term debt, we find attenuation in the estimated tax sensitivity: On average, firms increase total leverage by 69 basis points in response to a tax rise ( $p=0.016$ ). Even this smaller treatment effect is economically meaningful: Relative to the mean pre-treatment ratio of 23.3% (Table 2), it represents an increase of 3%. Column 5 excludes short-term debt but includes the current portion of long-term debt (due within a year). This has, as we predicted earlier, no effect on the estimated treatment effect: On average, firms increase leverage by 115 basis points ( $p<0.001$ ), one basis point more than our baseline estimate in Table 3. As before, we see no

sensitivity to tax cuts. Column 6 models long-term market (rather than book) leverage. The point estimate for tax rises is 71 basis points ( $p=0.043$ ) and we continue to find no reaction to tax cuts.

One potential critique of leverage regressions is that observed variation in leverage may reflect spurious changes in the denominator (the market or book value of assets) rather than changes in the numerator (outstanding debt). This would especially bias market-leverage regressions, because the same factors that cause a firm to adjust leverage (say, changes in union power) could also affect its share price. A simple remedy is to model log debt rather than a leverage ratio. The results, shown in column 7, confirm our findings: While firms are unresponsive to tax cuts ( $p=0.256$ ), they increase their long-term debt significantly after tax rises ( $p=0.001$ ), by a sizeable 10.7% on average.

### *5.6 Causality*

Are the observed sensitivity of leverage to tax increases and the failure to reduce leverage in response to tax cuts plausibly causal? That depends on our ability to rule out the presence of confounding effects, i.e., the possibility that omitted variables simultaneously drive state-level changes in taxes and firm-level changes in leverage. Because our leverage regressions include industry-year effects, we know that our results are not driven by time-varying industry shocks. We have also shown that our results hold within-firm (ruling out that they are driven by time-invariant firm heterogeneity) and that they are robust to the inclusion of state fixed effects.

The only remaining type of omitted variable that could confound our results is one that is collinear with the dimension of the tax-change treatment. Since the treatment varies within states across time, we cannot include state-year fixed effects to remove such a confound directly. In this section, we report tests dealing with three leading potential confounds: Changes in local economic conditions, changes in local labor market conditions, and changes in a state's political leanings.

#### *5.6.1 Potential Confound: Local Business Cycle Effects*

States may change corporate income taxes because of local demand shocks or other changes in their economic conditions. To the extent that these economic conditions also affect firms' debt



policies (say, because firms borrow more when their cash flows fall in recessions), the observed correlation between tax increases and leverage increases may be spurious. While the preliminary results in Table 1 suggest that tax increases are unrelated to state growth and unemployment rates, this potential confound nonetheless deserves serious consideration.

One way to address it is to add controls for state-level economic conditions. Table 5, column 1 includes lagged changes in GSP growth rates and state unemployment rates and a more direct proxy for local demand shocks: The lagged change in a state's sales growth, measured as the value-weighted average sales growth rate of all publicly traded firms headquartered in the state.

The effect of lagged state unemployment rates is positive, consistent with firms borrowing more when economic conditions in their home state deteriorate, but it is only marginally significant ( $p=0.077$ ). It is also economically small: A one-standard deviation increase in unemployment is associated with only a 17 basis point increase in leverage. Neither lagged GSP growth nor variation in the state's sales growth has a significant effect on leverage. Overall, the inclusion of these state-level economic conditions leaves the diff-in-diff estimates of tax increases and tax cuts essentially unchanged at 111 basis points ( $p<0.001$ ) and  $-1.2$  basis points ( $p=0.942$ ), respectively.

Column 1 suggests that omitting these *particular* measures of state-level economic conditions does not confound the estimated tax sensitivity of debt. A variation on the economic-conditions confound is that both firms and states react to omitted *regional* economic conditions and that some important part of this regional variation is orthogonal to our state-level controls for growth, demand, and unemployment. To isolate the potential effect of regional conditions, we consider a type of falsification test. Column 2 includes indicator variables for tax rises and tax cuts in a *bordering* state. The logic of this test is as follows. Suppose tax changes are driven by omitted changes in regional conditions (orthogonal to our state-level controls) and firms respond to these changes rather than to tax changes. Then we should see firms apparently "reacting" to a tax change in a bordering state (assuming they are exposed to similar economic conditions as the state next door).

Column 2 shows that, as before, firms borrow more in response to home-state tax rises and fail to cut leverage in response to home-state tax cuts. Consistent with a causal treatment effect, firms located in a state that does not change its own tax rate but borders one that does do *not* mirror this behavior. But there is more. When a neighboring state increases taxes, control firms actually *reduce* their leverage, by a significant 31 basis points on average ( $p=0.029$ ). If firms in neighboring states share similar economic conditions, this behavior is hard to reconcile with the conjectured confound. Instead, it suggests the presence of a different confound, one that would bias our estimates *downward*: Absent a tax increase, the “normal” reaction to the unobserved change in economic conditions appears to be to *reduce* leverage.

To explore this further, we present a variation on the bordering-states falsification test. Splitting the sample, we compare the debt policies of firms subject to a state-level tax shock to control firms headquartered in either a neighboring state (column 3) or in a far-away state (column 4). If variation in regional economic conditions rather than tax shocks were the true driver of leverage changes, we would expect no significant treatment effects when restricting control firms to be neighbors.

Again, we find the opposite. Compared to neighboring firms without tax changes, firms increase their long-term leverage by 120 basis points when their home state increases corporate income taxes ( $p=0.002$ ). Compared to firms located farther away, they increase their leverage by 91 basis points ( $p=0.003$ ). Thus, narrowing the sample of control firms to those sharing arguably similar (regional) economic conditions marginally *increases* the economic magnitude of the sensitivity of leverage to tax increases (though this increase is not statistically significant).

Of course, firms in neighboring states may not necessarily share the same economic conditions, for example if they are located at opposite ends of two large states. We can construct a cleaner test by focusing on firms headquartered in adjacent *counties* either side of a state border, where one firm experiences a state income tax change in year  $t$  while the other does not. Such county pairs share plausibly similar (unobserved) local economic conditions while being subject to different tax

treatments. This test is a form of sharp regression discontinuity approach.

We identify a firm's county based on its zip code, using a bridge obtained from the Centers for Disease Control and Prevention.<sup>23,24</sup> Our sample contains 345 county-pair/year clusters involving firms in adjacent county pairs such that in year  $t$ , one or more firms in one county experience a tax shock while one or more firms in the adjacent county do not. The total number of treated and control firms is 2,047 and because the same firm can be hit with multiple tax shocks over time, there are 10,208 firm-years. Of these, 641 involve tax rises in 19 states and 2,289 involve tax cuts in 22 states. Thus, there is a large number of firms per county-pair/year cluster and there is substantial variation in treatment status within each cluster involving a large number of separate tax shocks.

Column 5 includes two sets of fixed effects:<sup>25</sup> A set of county-pair/year fixed effects, to remove unobserved variation in economic conditions affecting firms operating in a pair of adjacent counties, and the set of industry-year fixed effects we used previously to remove unobserved variation in industry conditions that may affect leverage. Economically, column 5 compares the change in industry-adjusted leverage of treated and control firms operating in the same location (but not necessarily in the same industry), holding covariates constant.

Interestingly, the estimated sensitivity of leverage to tax increases in column 5 is twice as large as that in the Table 3 baseline specification: Relative to control firms just the other side of the state border, treated firms increase their (industry-adjusted) leverage by an average of 237 basis points ( $p=0.005$ ) when their home state raises corporate tax rates. This suggests that our simple treatment estimates, which use as controls firms from anywhere in the country (Table 3) or from anywhere in the neighboring state (Table 5, column 3), are conservative. Once we account for time-varying local

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<sup>23</sup> Available at [http://wonder.cdc.gov/wonder/sci\\_data/codes/fips/type\\_txt/cntyxref.asp](http://wonder.cdc.gov/wonder/sci_data/codes/fips/type_txt/cntyxref.asp). In rare cases, a zip code spans two counties, in which case we identify the correct county from a firm's SEC filings or a google search.

<sup>24</sup> We hand-collect historical zip codes from SEC filings for the 1,532 firms that our data checks indicate moved across state lines over our sample period. For the remaining 8,573 sample firms, we use Compustat's current zip codes. This will introduce noise to the extent that these firms moved counties within a state during our sample period. Given the large number of firm-years involved (81,926), hand-collecting historic zip codes for these firms is impracticable. However, our coefficients are quite precisely estimated, so noise does not appear to be a major concern.

<sup>25</sup> This is estimated using Stata's *reg2hdfe* command, which can handle two sets of fixed effects even if the number of units in each dimension is large; see Guimaraes and Portugal (2010).

economic conditions using adjacent-county-pair/year fixed effects, the tax sensitivity doubles.

Why is the estimated tax sensitivity so much greater? Unlike previous specifications, column 5 does not estimate the treatment effect within-industry; instead, it includes two *independent* sets of fixed effects, comparing the change in industry-adjusted leverage of, say, a treated food retailer to the contemporaneous change in industry-adjusted leverage of an untreated shoe manufacturer (requiring only that both be located in the same county-pair). To see if it is this industry mismatch that causes the estimated tax sensitivity to double, column 6 requires controls not only to be located in an adjacent county but also to operate in the same SIC4 industry. This is achieved by including county-pair/industry/year fixed effects, thus holding constant local industry conditions in year  $t$ .

Requiring neighboring firms to operate in the same industry reduces the sample size by nearly 90%, to 1,284 firm-years in 410 county-pair/industry/year triplets. Still, the point estimate proves remarkably stable. Relative to firms in the same industry located just the other side of a state border, treated firms increase their leverage by 254 basis points on average following a tax rise ( $p=0.009$ ).

The fact that the estimates in columns 5 and 6 are nearly identical suggests that industry mismatches cannot account for the observed increase in tax sensitivity. An alternative explanation is that the treated firms in these restricted samples are somehow unusual and thus selected. But that appears not to be the case: Treated firms in border counties increase their industry-adjusted leverage following a tax rise by an average of 100 basis points, which is actually less than the 141 basis-point increase among treated firms in interior counties excluded from our adjacent-counties tests (though this difference is not statistically significant ( $p=0.546$ )). The upshot is that the increase in tax sensitivity must be the result of restricting *control* firms to be located nearby. In particular, to account for the increase, it must be the case that control firms are *cutting* their leverage relative to their industry peers located elsewhere in the country. This dovetails with the significant reduction in leverage we see among control firms when a neighboring state raises taxes (see Table 5, column 2).

What remains after ruling out industry mismatches or selection effects in the restricted treatment

samples is a strong, and hitherto unknown, local determinant of firms' debt policies. Tax rises appear to coincide with changes in local conditions that *absent a tax change* would cause firms to cut leverage relative to their industry peers elsewhere in the country. However, this effect is masked, among treated firms faced with the *same* change in local conditions, by an increase in the marginal (tax) benefit of debt. Failing to control for variation in local conditions, as our baseline models do, underestimates the magnitude of the tax sensitivity by comparing treated firms to controls that are mostly located too far away to be affected by the same local conditions. This, in turn, implies that tax rises plausibly *cause* firms to borrow more.

### 5.6.2 Potential Confound: Unobserved Changes in Investment Opportunities

A potential explanation for the asymmetric tax sensitivity is that tax cuts correlate with better *local* investment opportunities<sup>26</sup> so that treated firms face two exactly offsetting effects: A reduction in the value of tax shields, prompting a cut in debt, and a simultaneous increase in the demand for capital, prompting firms to borrow more. If this explanation were true, we should see a “reverse” treatment effect: *Control* firms exposed to the same improvement in local conditions but no change in the value of tax shields should *increase* their leverage. We see no evidence of this in Table 5. Whether we compare treated firms to controls headquartered in a neighboring state, focus on firms in adjacent-county pairs, or even remove unobserved variation in local *industry* conditions, we find that firms never cut leverage in response to tax cuts.

### 5.6.3 Potential Confounds: Union Power and Political Leanings

We next consider two other potential confounds, starting with labor market conditions. Matsa (2010) finds a positive correlation between union power and leverage which he interprets as evidence that firms use debt strategically to counter their unions' bargaining power. If labor market forces are a first-order determinant of capital structure choices, what looks like a tax-induced change in leverage may in fact be driven by unobserved variation in union power in a given state

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<sup>26</sup> Recall that the only omitted variables that could confound our tests are those that, like the treatment, vary within state across time. Thus, we only need to deal with unobserved *local* improvements in investment opportunities.

which simultaneously causes tax rises and leverage increases.

To test this, we exploit variation in unionization rates across states and time. Columns 1 and 2 of Table 6 partition the sample into firms headquartered in states with either high or low union power. (See Appendix C for details.) Both sets of firms increase leverage significantly when taxes rise. Interestingly, the increase is nearly twice as large among firms located in *low*-union states, at 141 versus 80 basis points. This, together with the results in Table 1 showing that tax-increasing states are no more unionized than other states, casts doubt on the idea firms borrow more not because of a tax rise but to counter union power.

Titman (1984) argues that firms choose their debt to insure workers against unemployment risk. To confound our results, unemployment risk would have to fall at the same time as states increase corporate taxes. To the extent that unemployment risk correlates with unemployment rates, the lack of correlation between tax changes and state unemployment rates shown in Table 1 suggests our tests are unlikely to be confounded in this way. In columns 3 and 4 of Table 6, we conduct a direct test, partitioning the sample into firms headquartered in states that suffer either large or small employment shocks at the time of a tax rise. (See Appendix C for definitions.) We observe a positive and significant tax sensitivity in both groups, averaging 121 and 99 basis points, respectively. Thus, firms increase their leverage in response to tax rises regardless of whether their state has suffered a large employment shock.

Our third potential confound concerns political economy factors. Table 1 showed that states that lean Democratic are significantly more likely to raise corporate income taxes. To examine if this might lead to a spurious correlation between tax rises and leverage increases, columns 5 and 6 of Table 6 partition the sample into firms that are headquartered in states leaning Democratic or Republican, respectively. While larger in Democratic states, we find no evidence that the sensitivity of leverage to tax increases varies significantly with the political leanings of a firm's home state.

Throughout these models, we continue to find that the tax sensitivity of leverage is asymmetric.

### 5.7 Potential Measurement Error: Location of Operations and Sales

Firms are taxed wherever they operate. To the extent that sample firms have operations outside the state in which they are headquartered, our leverage regressions thus underestimate the sensitivity of debt to taxes. Put differently, the tax sensitivity we estimate is the weighted average response to tax changes given the geographic distribution of firms' operations. It will be lower if a firm also operates in states that experience no tax changes. While less of a problem for tax increases, measurement error could *potentially* explain the observed lack of sensitivity to tax cuts.

To illustrate that our estimates represent lower bounds, Table 7 partitions sample firms into multinationals and domestic firms. Consistent with the prediction that multinationals are less sensitive to changes in state taxes than domestic firms, as part of their tax base is abroad, we find that only domestic firms respond to tax increases. While the diff-in-diff estimate for multinationals is an insignificant 28 basis points ( $p=0.505$ ), it is more than five times greater, at 152 basis points, for domestic firms ( $p=0.001$ ). The difference in point estimates is statistically significant ( $p=0.026$ ). This is consistent with our conjecture that unobserved heterogeneity in the geographic location of firms' taxable operations biases the estimated sensitivity to tax increases downward. By contrast, neither domestic nor multinational firms cut their leverage in response to tax cuts. Thus, measurement error does not appear to be the cause of the observed asymmetry in tax sensitivity.

We next test if unobserved heterogeneity in where firms generate their sales attenuates the estimated tax sensitivity. Following Agrawal and Matsa (2012), we partition firms based on whether sales in their NAICS3 industry are predominantly inter-state or intra-state. Firms in industries shipping predominantly outside their home state should respond less to state tax changes than firms selling predominantly in their home state. As columns 3 and 4 show, the data support this prediction. Firms in industries that ship mostly out-of-state do not increase leverage significantly when their home state increases corporate income taxes, while firms in industries that tend to sell mostly within-state do. The point estimates are 37 and 121 basis points, respectively, and the

difference is significant at the 10% level. Again, neither group of firms responds to tax cuts.

### 5.8 Heterogeneous Treatment Effects

Interest tax shields depend on the interplay between personal taxes on interest income ( $\tau_i$ ) and income from equity ( $\tau_e$ ) on the one hand and corporate taxes on profits ( $\tau_c$ ) on the other. The standard textbook tax benefit of debt can be written as  $[(1 - \tau_i) - (1 - \tau_c)(1 - \tau_e)]D$ , where  $D$  denotes the level of debt. Let the (net) cost of debt be represented by a generic quadratic function

$a + bD + cD^2$ . The first-order condition for the optimal debt level  $D^*$  then is  $\frac{dD^*}{d\tau_c} = \frac{1}{2c}(1 - \tau_e)$ .

Thus, higher personal taxes on equity income dampen the impact of a corporate tax change on debt. Because  $\tau_e$  likely varies in the cross-section,<sup>27</sup> treatment effects should be heterogeneous.

$\tau_e$  cannot be measured directly: Not only does it depend on whether a firm's marginal investor is a tax-exempt institution or a wealthy individual subject to the top income tax rate. It also varies across firms as a function of the relative importance of dividend income and capital gains (the latter being taxed at a lower effective rate since they can be deferred and/or offset against capital losses).

This discussion suggests a useful validation test. If the observed tax sensitivity of debt is causal, we expect stronger effects among firms with small  $\tau_e$ . To test this comparative static, Table 8 considers two proxies for  $\tau_e$ : Dividends and institutional ownership. Non-dividend payers have lower  $\tau_e$  than dividend-payers because their investors derive their equity income solely in the form of (lower-taxed) capital gains. And firms that are predominantly owned by institutions have lower  $\tau_e$  than those predominantly owned by retail investors, as institutions are often tax-exempt.

When we split the samples accordingly, we find results consistent with heterogeneous treatment effects. While non-dividend payers increase leverage by 155 basis points after a tax rise ( $p < 0.001$ ), dividend payers increase leverage by only 39 basis points ( $p = 0.411$ ); the difference between these

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<sup>27</sup> We know from the regressions reported in Table 1 that time-series variation in state taxes on personal income and capital gains is unrelated to state corporate income tax changes. Thus, here we focus on cross-sectional variation.



point estimates is statistically significant ( $p=0.034$ ). The ownership test shows qualitatively similar results: Firms with large institutional holdings increase leverage by 142 basis points ( $p=0.008$ ) while firms with large retail holdings increase leverage by 84 basis points ( $p=0.231$ ).

A corollary of a causal interpretation is that the tax sensitivity of debt should vary with profits, as interest-bearing debt offers valuable tax shields only to profitable firms. Columns 5 and 6 partition sample firms according to whether or not they are profitable in the year of the tax rise.<sup>28</sup> Consistent with firms borrowing to take advantage of tax shields, we find that only profitable firms borrow more: When faced with higher taxes in their home state, profitable firms increase leverage by 113 basis points ( $p<0.001$ ), nine times more than the estimated diff-in-diff increase of 12 basis points for loss-making firms ( $p=0.897$ ); the difference is marginally significant ( $p=0.068$ ).<sup>29</sup>

Trade-off theory suggests that the extent to which a firm *can* increase its leverage in response to a tax rise depends on its debt capacity and its likely costs of distress (as captured by  $c$  above). Effectively, its default risk acts as a constraint on its ability to take advantage of further tax shields of debt. To test this prediction, we partition firms into those rated investment-grade (column 7) and those rated junk by S&P, Moody's, or Fitch (column 8). Firms without a credit rating are omitted. We find that investment-grade firms increase their leverage by 126 basis points ( $p=0.018$ ) following a tax rise, whereas riskier borrowers do not increase their leverage at all ( $p=0.946$ ).

Overall, these patterns support a causal interpretation of the observed tax sensitivity of debt.

## 6. Conclusions

The U.S. tax system subsidizes firms' use of debt: Interest payments are tax deductible while retained earnings and dividends are not. Despite decades of scholarship, it is an open question whether taxes are a first-order determinant of capital structure. We overcome the identification challenges that have hampered previous work by using a natural experiment in the form of

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<sup>28</sup> Alternatively, we could condition on marginal tax rates (MTR). While MTR cannot be directly observed, Graham (1996b) and Graham and Mills (2008) provide useful simulations. Using their simulated MTRs, we find qualitatively similar (albeit considerably noisier) results.

<sup>29</sup> Though not reported, we find statistically stronger results if we partition firms based on whether they were profitable or loss-making in every year between  $t = -2$  and  $t = 0$  ( $p=0.043$ ), which may be a better predictor of future profitability.

staggered changes in corporate income tax rates across U.S. states. Our results show that firms react strongly to tax increases but are insensitive to tax cuts. These findings are robust to various potential confounds. Finally, we find evidence of geographic clustering in corporate debt policies.

The asymmetry in tax sensitivity we observe in the data runs counter to standard trade-off theory. It suggests that leverage is sticky on the downside, in the sense that tax increases ratchet up leverage permanently while tax cuts do not subsequently reduce it. What could explain this hysteresis? After all, it is surprising that firms appear quite happy to increase leverage (which increases bankruptcy risk) but reluctant to cut it in response to changes in the tax benefit of debt.

Unless the firm wishes to shrink its balance sheet, reducing leverage involves either issuing equity or cutting the dividend. Thus, one possible explanation for the lack of response to tax cuts is that managers are simply reluctant to issue equity (consistent with pecking-order arguments) or to cut the dividend (to avoid the negative share price reaction that typically results). Another is that firms face some kind of adjustment costs as in Leary and Roberts (2005).

Alternatively, tax cuts could operate on two margins simultaneously: Reducing the marginal tax benefit while increasing the after-tax return on investment and so inducing firms to undertake more (debt-financed) investment. We are skeptical of this story, for three reasons. First, our regression-discontinuity tests do not support it. Second, the argument should apply in reverse to tax rises, so why would the two effects exactly offset each other for tax cuts but not for tax rises? Third, Asker, Farre-Mensa, and Ljungqvist (2011) find that publicly traded firms (such as the ones in our sample) do not increase investment in response to either tax cuts or tax rises.

In a policy paper dealing with the problems of bank recapitalization, Admati et al. (2012) suggest a third possible explanation: Shareholders may be reluctant to reduce leverage because the benefit flows primarily to creditors as the remaining debt becomes safer. Such a debt overhang problem could potentially explain why we find an asymmetric tax sensitivity. We leave further analysis of the causes of hysteresis in leverage to future research.

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## Appendix A. List of State Corporate Tax Increases.

This table lists all state corporate tax increases over the period 1990-2011. In states with more than one tax bracket, we report the change to the top bracket. To identify these changes, we use data obtained from the Tax Foundation (an abbreviated version of which is available at <http://www.taxfoundation.org>) and a search of the “Current Corporate Income Tax Developments” feature published periodically in the *Journal of State Taxation*. We verify all information using each state’s Department of Revenue and State Legislature websites.

State	Year	Description	No. of affected sample firms
KY	1990	Increase in top corporate income tax rate from 7.25% to 8%	13
MO	1990	Increase in top corporate income tax rate from 5% to 6.5%	53
MT	1990	Introduction of 5% tax surcharge on tax liability	3
NE	1990	Increase in top corporate income tax rate from 6.65% to 7.24%	10
OK	1990	Increase in top corporate income tax rate from 5% to 6%	45
AR	1991	Increase in top corporate income tax rate from 6% to 6.5%	17
ME	1991	Introduction of 10% tax surcharge on tax liability	4
NC	1991	Increase in top corporate income tax rate from 7% to 7.75% and introduction of 4% tax surcharge on tax liability	60
NE	1991	Increase in top corporate income tax rate from 7.24% to 7.81% and introduction of 15% tax surcharge on tax liability	10
PA	1991	Increase in top corporate income tax rate from 8.5% to 12.25%	165
RI	1991	Introduction of 11% tax surcharge on tax liability	10
WI	1991	Introduction of 5.5% tax surcharge on tax liability	60
KS	1992	Increase in top corporate income tax rate (including surcharge) from 6.75% to 7.35%	21
MT	1992	Re-introduction of tax surcharge on tax liability at 2.3% rate	2
MO	1993	Increase in top corporate income tax rate from 5% to 6.25%	68
MT	1993	Increase in tax surcharge on tax liability from 2.3% to 4.7%	4
VT	1997	Increase in top corporate income tax rate from 8.25% to 9.75%	9
NH	1999	Increase in top corporate income tax rate from 7% to 8%	19
AL	2001	Increase in top corporate income tax rate from 5% to 6.5%	24
NH	2001	Increase in top corporate income tax rate from 8% to 8.5%	18
CA	2002	Suspension of state net operating loss (NOL) deduction, affecting profitable firms that have tax loss carryovers for California state income tax purposes	148
IA	2002	Introduction of 2.5% tax surcharge	17
KS	2002	Increase in tax surcharge on taxable income from 3.35% to 4.5%	19
KY	2002	Introduction of 3.35% tax surcharge on income > \$50,000	23
NJ	2002	Introduction of Alternative Minimum Assessment tax, under which firms pay the greater of a gross receipts tax and the corporate franchise (net income) tax; suspension of NOL deduction	175
TN	2002	Increase in top corporate income tax rate from 6% to 6.5%	51
AR	2003	Introduction of 3% tax surcharge on tax liability	15
CT	2003	Introduction of 20% tax surcharge on tax liability	87
IN	2003	Repeal of gross income tax (based on revenue rather than profits) and of supplemental income tax; effective adjusted gross income tax rate (on profits) increased from 7.75% to 8.5%	35
CT	2004	Increase in tax surcharge on tax liability to 25%	85
NJ	2006	Introduction of 4% tax surcharge on tax liability	149
MD	2008	Increase in top corporate income tax rate from 7% to 8.25%	58
MI	2008	Introduction of corporate income tax with a top rate of 4.95%; replaces a gross-receipts tax without interest deductibility	54
CT	2009	Introduction of 10% tax surcharge on tax liability for companies with revenues > \$100m	47
NC	2009	Introduction of 3% tax surcharge on tax liability	60
OR	2009	Increase in top corporate income tax rate from 6.6% to 7.9%	26
CT	2011	Unscheduled two-year extension of tax surcharge on tax liability and increase to 20%	49
IL	2011	Increase in top corporate income tax rate from 7.3% to 9.5%	111

## Appendix B. List of State Corporate Tax Cuts.

This table lists all state corporate tax cuts over the period 1990-2011. In states with more than one tax bracket, we report the change to the top bracket. To identify these changes, we use data obtained from the Tax Foundation (an abbreviated version of which is available at <http://www.taxfoundation.org>) and a search of the “Current Corporate Income Tax Developments” feature published periodically in the *Journal of State Taxation*. We verify all information using each state’s Department of Revenue and State Legislature websites.

State	Year	Description	No. of affected sample firms
AZ	1990	Reduction in top corporate income tax rate from 10.5% to 9.3%	44
WV	1990	Reduction in top corporate income tax rate from 9.525% to 9.375%	7
MN	1991	Reduction in the legislated tax increase of 0.4%	146
MT	1991	Repeal of 5% tax surcharge	2
WV	1991	Reduction in top corporate income tax rate from 9.3% to 9.15%	6
MO	1992	Reduction in top corporate income tax rate from 6.5% to 5%	61
WV	1992	Reduction in top corporate income tax rate from 9.15% to 9%	6
DC	1993	Reduction in tax surcharge from 5% to 2.5%	8
ME	1993	Repeal of 10% tax surcharge	5
NE	1993	Repeal of 15% tax surcharge	12
AZ	1994	Reduction in top corporate income tax rate from 9.3% to 9%	56
MT	1994	Repeal of 4.7% tax surcharge	3
NJ	1994	Repeal of 0.375% tax surcharge	221
NY	1994	Reduction in tax surcharge from 15% to 10%	434
PA	1994	Reduction in top corporate income tax rate from 12.25% to 11.99%	200
RI	1994	Repeal of 11% tax surcharge	23
CT	1995	Reduction in top corporate income tax rate from 11.5% to 11.25%	125
DC	1995	Reduction in top corporate income tax rate from 10% to 9.50%	8
NH	1995	Reduction in top corporate income tax rate from 7.5% to 7%	20
NY	1995	Repeal of 10% tax surcharge	435
PA	1995	Reduction in top corporate income tax rate from 11.99% to 9.99%	202
CT	1996	Reduction in top corporate income tax rate from 11.25% to 10.75%	135
CA	1997	Reduction in top corporate income tax rate from 9.3% to 8.84%	942
CT	1997	Reduction in top corporate income tax rate from 10.75% to 10.50%	138
NC	1997	Reduction in top corporate income tax rate from 7.75% to 7.5%	82
AZ	1998	Reduction in top corporate income tax rate from 9% to 8%	70
CT	1998	Reduction in top corporate income tax rate from 10.5% to 9.50%	123
NC	1998	Reduction in top corporate income tax rate from 7.5% to 7.25%	83
WI	1998	Reduction in tax surcharge from 5.5% to 2.75%	64
CO	1999	Reduction in top corporate income tax rate from 5% to 4.75%	140
CT	1999	Reduction in top corporate income tax rate from 9.5% to 8.50%	111
NC	1999	Reduction in top corporate income tax rate from 7.25% to 7%	76
NY	1999	Reduction in top corporate income tax rate from 9% to 8.5%	365
OH	1999	Reduction in top corporate income tax rate from 8.9% to 8.5%	148
AZ	2000	Reduction in top corporate income tax rate from 8% to 7.968%	65
CO	2000	Reduction in top corporate income tax rate from 4.75% to 4.63%	127
CT	2000	Reduction in top corporate income tax rate from 8.5% to 7.50%	102
NC	2000	Reduction in top corporate income tax rate from 7% to 6.9%	72
NY	2000	Reduction in top corporate income tax rate from 8.5% to 8%	381
AZ	2001	Reduction in top corporate income tax rate from 7.968% to 6.968%	55
ID	2001	Reduction in top corporate income tax rate from 8% to 7.6%	8
NY	2001	Reduction in top corporate income tax rate from 8% to 7.5%	325
IA	2003	Repeal of 2.5% tax surcharge	17
KS	2003	Reduction in tax surcharge from 4.5% to 3.35%	20
KY	2003	Repeal of 3.35% tax surcharge	22
ND	2004	Reduction in top corporate income tax rate from 10.5% to 7%	1

AR	2005	Repeal of 3% tax surcharge	14
KY	2005	Reduction in top corporate income tax rate from 8.25% to 7%	19
OH	2005	Tax reform phasing out corp. income tax while phasing in gross receipts tax over period of 5 years	102
CT	2006	Reduction in tax surcharge from 25% to 20%	74
VT	2006	Reduction in top corporate income tax rate from 9.75% to 8.9%	2
KY	2007	Reduction in top corporate income tax rate from 7% to 6%	17
ND	2007	Reduction in top corporate income tax rate from 7% to 6.5%	0
NY	2007	Reduction in top corporate income tax rate from 7.5% to 7.1%	261
VT	2007	Reduction in top corporate income tax rate from 8.9% to 8.5%	2
WV	2007	Reduction in top corporate income tax rate from 9% to 8.75%	6
CT	2008	Repeal of 20% tax surcharge	69
KS	2008	Reduction in tax surcharge from 3.35% to 3.15%	17
TX	2008	Abolition of income tax, replaced with gross receipts tax	300
KS	2009	Reduction in tax surcharge from 3.15% to 3.05%	16
ND	2009	Reduction in top corporate income tax rate from 6.5% to 6.4%	1
WV	2009	Reduction in top corporate income tax rate from 8.75% to 8.5%	5
MA	2010	Reduction in top corporate income tax rate from 9.5% to 8.75%	160
NJ	2010	Repeal of 4% tax surcharge	98
KS	2011	Reduction in tax surcharge from 3.05% to 3%	8
MA	2011	Reduction in top corporate income tax rate from 8.75% to 8.25%	129
OR	2011	Reduction in top corporate income tax rate from 7.9% to 7.6%	25

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## Appendix C. Variable Definitions.

### Dependent variables

**Long-term book leverage** is defined as long-term debt (Compustat item *dltt*) over the book value of assets (Compustat item *at*).

**Long-term book leverage (including current portion of long-term debt)** is defined as the sum of long-term debt (Compustat item *dltt*) and long-term debt due in one year (Compustat item *ddl*), over the book value of assets (Compustat item *at*).

**Total book leverage** is defined as the sum of long-term debt (Compustat item *dltt*) and short-term debt (Compustat item *dlc*), over the book value of assets (Compustat item *at*).

**Market leverage** is defined as long-term debt (Compustat item *dltt*) over the sum of long-term debt and the fiscal-year-end share price (Compustat item *prcc\_f*) times the number of common shares outstanding (Compustat item *csho*).

**Log long-term debt** is defined as the natural logarithm of one plus long-term debt (Compustat item *dltt*), deflated to 2005 dollars using the GDP deflator available at <http://www.bea.gov/national/xls/gdplev.xls>.

### Independent variables: State-level characteristics

**State GSP growth rate** is the real annual growth rate in gross state product (GSP) using data obtained from the U.S. Bureau of Economic Analysis.

**State unemployment rate** is the state unemployment rate, obtained from the U.S. Bureau of Labor Statistics.

**Vote share of Democratic Presidential candidate** is the share of the vote cast by voters in the state for the Democratic candidate in the most recent Presidential election before year  $t$ , and zero otherwise. Election data come from the American Presidency Project at UC Santa Barbara (<http://www.presidency.ucsb.edu>).

**State union membership** is the fraction of private-sector employees in a state who belong to a labor union in year  $t$ . The data come from Hirsch and Macpherson (2003) as updated on their website, [www.unionstats.com](http://www.unionstats.com).

**State sales growth** is the value-weighted mean sales growth among publicly traded firms headquartered in a state, constructed from Compustat data for sales growth and weighted by firms' market values of equity [*prcc\_f*\**csho*].

**State tax on wages** is the maximum state tax rate on wage income, estimated for an additional \$1,000 of income on an initial \$1,500,000 of wage income (split evenly between husband and wife). The taxpayer is assumed to be married and filing jointly. The data come from Daniel Feenberg, available at <http://users.nber.org/~taxsim/state-rates/>.

**State tax on long-term capital gains** is the maximum state tax rate on long-term capital gains. The data come from Daniel Feenberg, available at <http://users.nber.org/~taxsim/state-rates/>.

### Independent variables: Firm-level characteristics

**ROA** (return on assets) is defined as operating income before depreciation (Compustat item *oibdp*) over the book value of assets (Compustat item *at*).

**Firm size** is defined as the natural logarithm of total assets (Compustat item *at*) in year 2005 real dollars (deflated using the GDP deflator available at <http://www.bea.gov/national/xls/gdplev.xls>).

**Tangibility** is defined as net property, plant, and equipment (Compustat item *ppent*), over the book value of assets (Compustat item *at*).



**Market/book** is constructed as in Frank and Goyal (2009). It is defined as (fiscal year-end closing price [*prcc\_f*] times common shares used to calculate earnings per share [*cshpri*] + the liquidation value of preferred stock [*pstkl*] + long-term debt [*dltt*] + short-term debt [*dlc*] – deferred taxes and investment tax credits [*txditc*]) / total assets [*at*].

#### Independent variables: Credit market conditions

**Default spread** is the difference between the yield on Baa and Aaa rated corporate bonds, measured as of the firm's fiscal-year month end. The data are obtained from the Federal Reserve's H15 Report, accessed through WRDS.

#### Conditioning variables

Firms in **bordering states** are firms headquartered in states that border a state that changes its corporate income tax but that do not themselves change their corporate income taxes at the same time.

Firms in **far-away states** are firms that are headquartered two or more states away from a state that changes its corporate income tax.

**States with high union power** is an indicator set equal to one if the firm is headquartered in a state that ranks in the top third of states according to the fraction of private-sector employees who belong to a labor union in year *t*, and zero otherwise. The data come from Hirsch and Macpherson (2003) as updated on their website, [www.unionstats.com](http://www.unionstats.com).

**States with low union power** is an indicator set equal to one if the firm is headquartered in a state that ranks in the bottom third of states according to the fraction of private-sector employees who belong to a labor union in year *t*, and zero otherwise.

**States suffering large employment shocks** is an indicator set equal to one if the firm is headquartered in a state that ranks in the top third of states according to the fraction of private-sector employees (measured as of year *t*-1) who lose their jobs in a mass layoff event in year *t*, and zero otherwise. The data come from the Bureau of Labor Statistics' Mass Layoff Statistics (<http://www.bls.gov/mls/#tables>) and are available only for the period from 1996.

**States suffering no large employment shocks** is an indicator set equal to one if the firm is headquartered in a state that ranks in the bottom third of states according to the fraction of private-sector employees (measured as of year *t*-1) who lose their jobs in a mass layoff event in year *t*, and zero otherwise.

**States leaning Democratic** is an indicator set equal to one if the firm is headquartered in a state that voted for the Democratic candidate in the most recent Presidential election before year *t*, and zero otherwise. Election data come from the American Presidency Project at UC Santa Barbara (<http://www.presidency.ucsb.edu>).

**States leaning Republican** is an indicator set equal to one if the firm is headquartered in a state that voted for the Republican candidate in the most recent Presidential election before year *t*, and zero otherwise.

**Multinational** is constructed as in Foley et al. (2007). It is an indicator set equal to 1 if the firm reports paying foreign income taxes (Compustat variable *txfo* non-zero and non-missing) or reports having foreign income (Compustat variable *pifo* non-zero and non-missing), and zero otherwise.

**Domestic firm** is an indicator set equal to 1 if *multinational* equals 0, and vice versa.

**High inter-state sales** is constructed using data from Agrawal and Matsa (2012). Agrawal and Matsa use data from the 2007 Commodity Flow Survey (CFS) to calculate, for each three-digit NAICS industry covered by the CFS, the fraction of shipments (by value) that stay within-state ("intra-state sales") rather than leave the state ("inter-state sales"). Using these data, we construct an indicator set equal to 1 for industries whose inter-state sales exceed the 67<sup>th</sup> percentile, and zero otherwise.

**Low inter-state sales** is an indicator set equal to 1 for industries whose inter-state sales are below the 33<sup>rd</sup> percentile, and zero otherwise.

**Non-dividend payers** are firms with zero dividends on common stock (Compustat item *dvc*) and on preferred stock (Compustat item *dvp*).

**Dividend payers** are firms with non-zero dividends on either common stock (Compustat item *dvc*) or preferred stock (Compustat item *dvp*).

**High institutional ownership** is an indicator set equal to 1 if the fraction of the firm's outstanding shares that are held by institutional investors filing 13f reports (according to Thomson Reuters) exceeds the 67<sup>th</sup> percentile, and zero otherwise.

**High retail ownership** is an indicator set equal to 1 if the fraction of the firm's outstanding shares that are held by institutional investors filing 13f reports (according to Thomson Reuters) is below the 33<sup>rd</sup> percentile, and zero otherwise.

**Profitable** is an indicator set equal to 1 if *ROA* is strictly positive, and zero otherwise.

**Loss-making** is an indicator set equal to 1 if *ROA* is weakly negative, and zero otherwise.

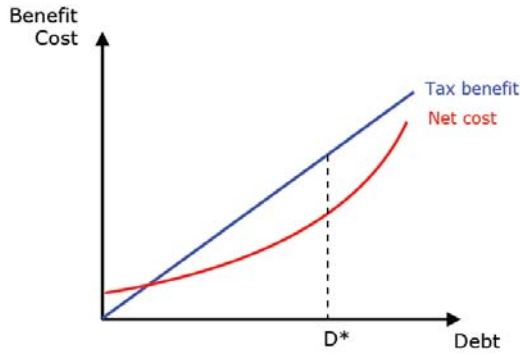
**Investment grade** is an indicator set equal to 1 if in year *t*, the firm has an investment-grade rating from S&P, Moody's, or Fitch, using data obtained from Compustat (variable *splticrm*) and Mergent FISD, and zero otherwise. It is missing for firms without a credit rating.

**Below-investment grade** is an indicator set equal to 1 if *investment grade* equals 0, and vice versa.

### Figure 1. Ideal Experiment and Identification Challenges.

Figure 1a illustrates the standard argument of trade-off theory: Firms choose the level of debt that maximizes the difference between the tax benefit of debt and the net cost of debt. At the optimal debt level  $D^*$ , the marginal tax benefit equals the marginal net cost. The tax benefit of debt depends on the corporate tax rate ( $\tau_c$ ), the personal tax rate on income from debt ( $\tau_i$ ), and the personal tax rate on income from equity ( $\tau_e$ ). Figure 1b illustrates the ideal experiment. Different tax rates ( $MB_1, MB_2, MB_3, \dots, MB_n$ ) are randomly assigned to firms and the resulting debt choices ( $D_1, D_2, D_3, \dots, D_n$ ) are recorded. The random assignment ensures that differences in debt levels cannot be the result of unobserved heterogeneity across firms. It is as if there was a single firm whose marginal cost curve (MC) is traced out by exogenous shifts in the marginal tax benefit. Figure 1c illustrates the identifying assumption for observational data. When comparing two (groups of) firms  $i$  and  $j$  that differ in their effective tax rates, identification requires that both (groups of) firms share the same marginal cost,  $MC_i = MC_j$ . Figure 1d illustrates the identification challenge. Two firms  $i$  and  $j$  can have different levels of debt even if taxes provide no marginal benefit (the null hypothesis), as long as they differ in their marginal costs (a violation of the identifying assumption).

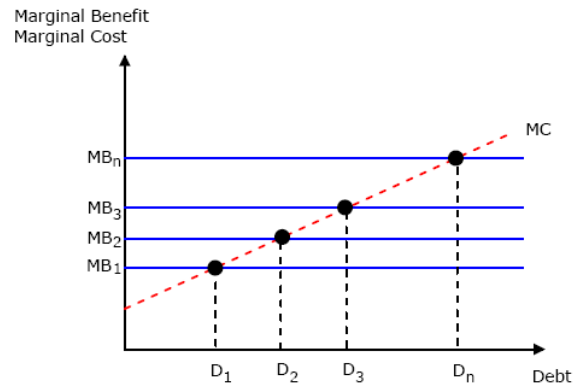
**Figure 1a: Trade-off theory**



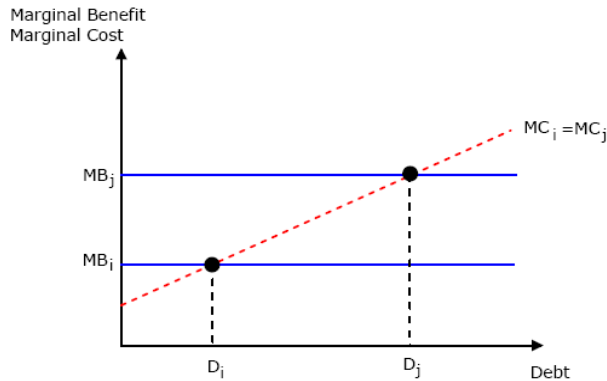
$$\text{Tax benefit} = [(1-\tau_i) - (1-\tau_c)(1-\tau_e)]D$$

$$\text{Net cost} = a + bD + cD^2$$

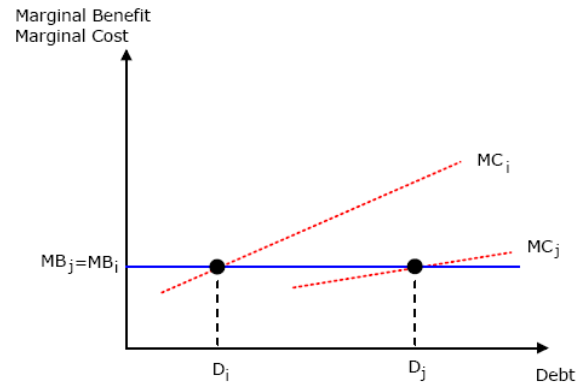
**Figure 1b: The ideal experiment**



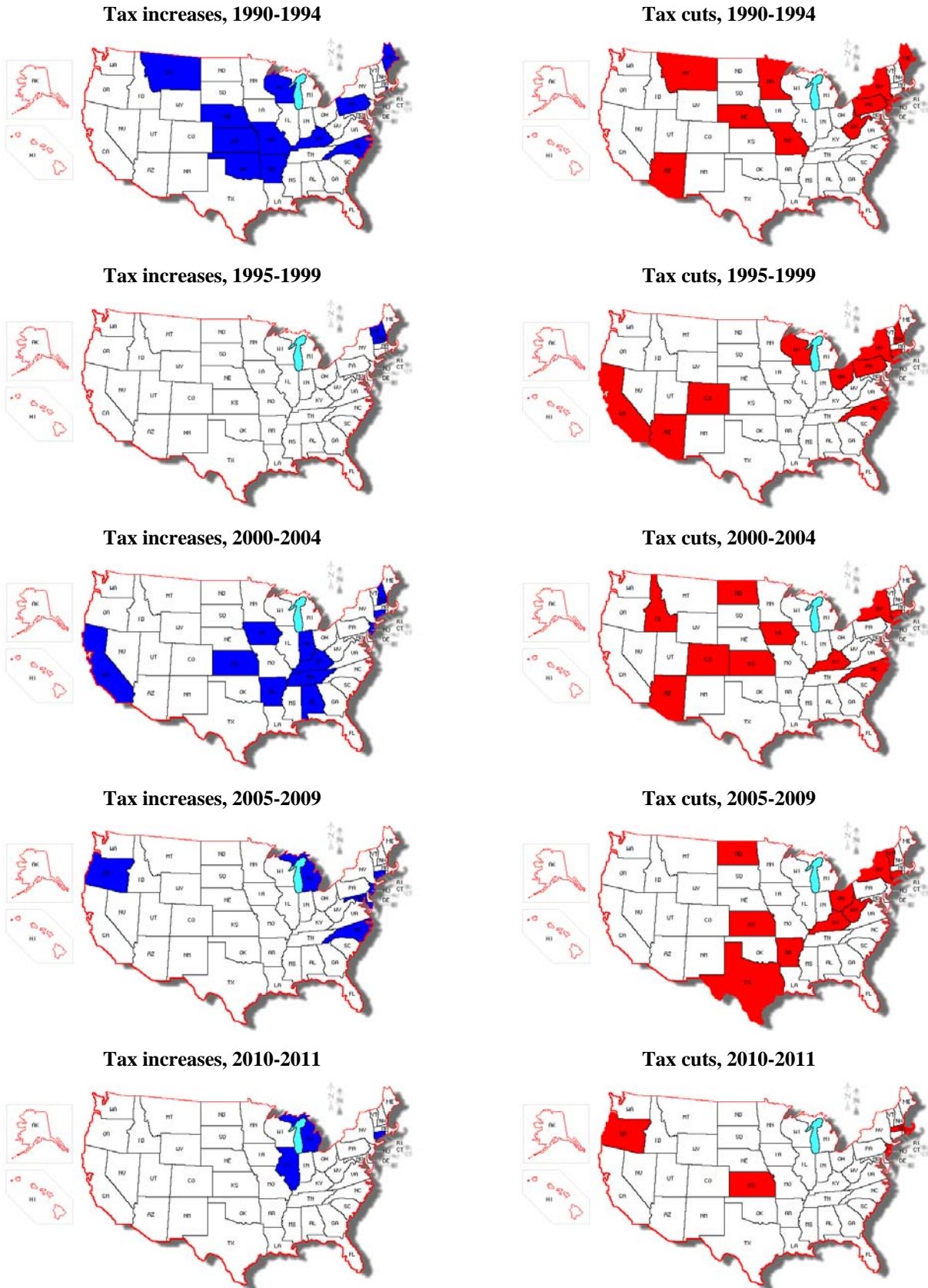
**Figure 1c: Identifying assumption for observational data**



**Figure 1d: Identification challenge**

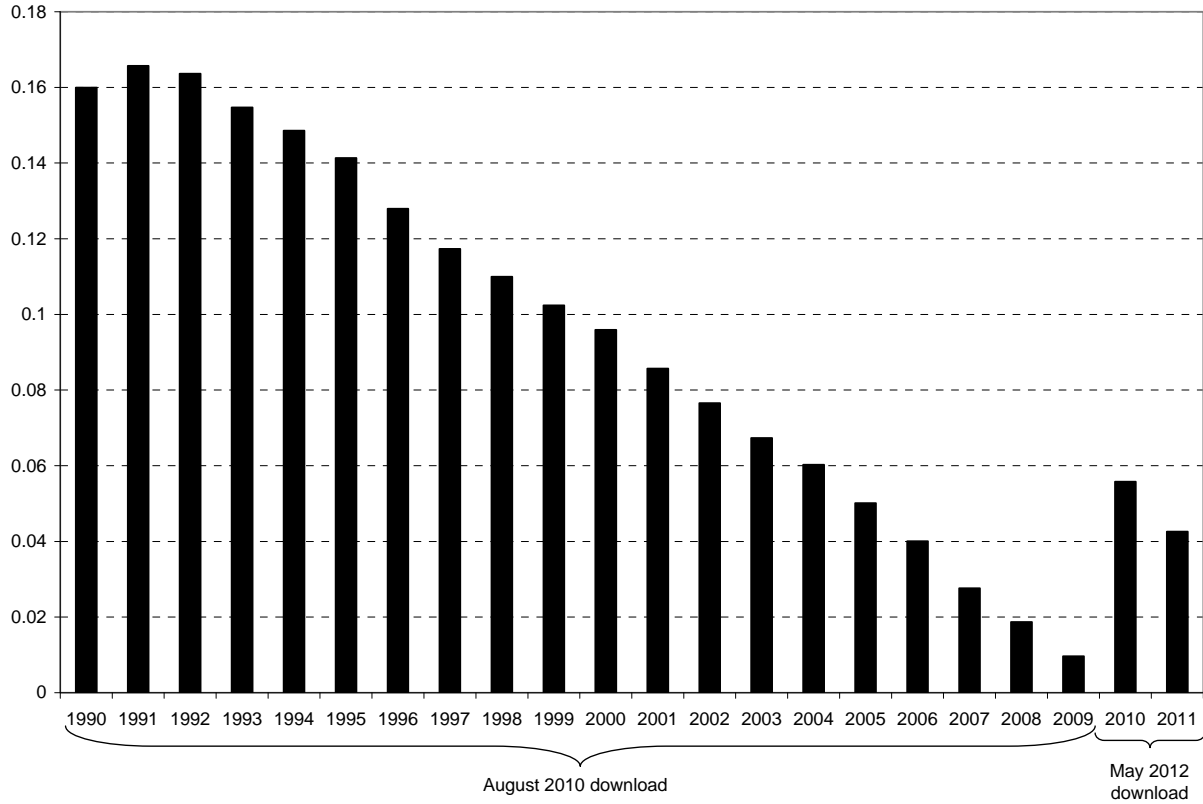


**Figure 2. Geography of State Corporate Income Tax Changes, 1990-2011.**



**Figure 3. Firm-years with Incorrect HQ State Information in Compustat.**

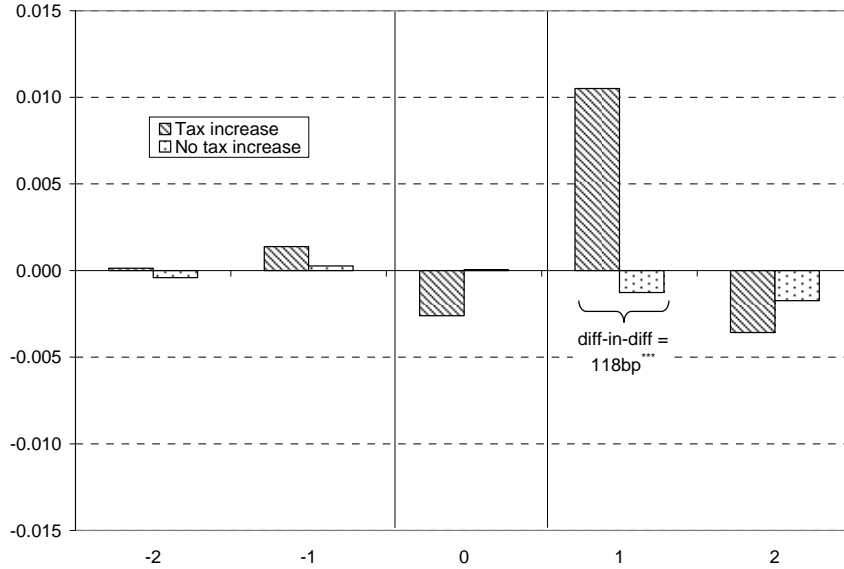
Compustat reports a firm’s current (as opposed to historic) headquarter state. Based on manual corrections using regulatory filings, the figure shows the fraction of non-financial and non-utility companies in the U.S. each year whose headquarters are located in a different state than the one reported by Compustat, for two downloads: One dated August 2010 (covering fiscal years 1990-2009) and another dated May 2012 (covering fiscal years 2010-2011). In the May 2012 download, Compustat frequently fails to record firms’ headquarter states altogether, accounting for the higher error rate.



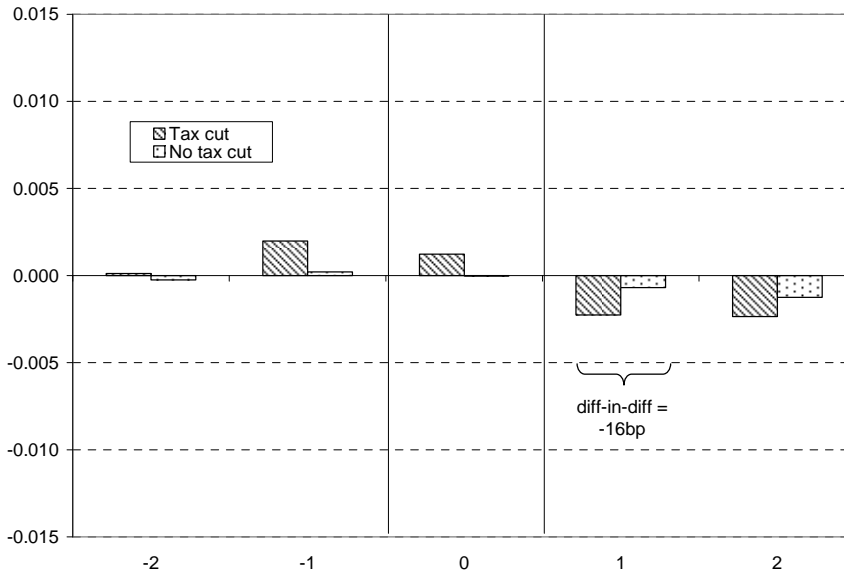
**Figure 4. Annual Changes in Leverage Around State Tax Increases and State Tax Cuts.**

The figures plot the average annual within-firm change in long-term leverage for each year in a five-year window centered on the year a state increases or cuts its corporate income tax (year 0) for treated firms (striped bars) and controls (dotted bars). The difference between the two bars in a given year is the difference-in-difference estimate. The significance of *t*-tests of the null that the diff-in-diff is zero is indicated using asterisks. We use \*\*\*, \*\*, and \* to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. The influence of time-varying changes in industry conditions (and nation-wide variation in business conditions that affect all industries simultaneously) is removed via industry-year fixed effects. To screen out firms with negative equity (distressed firms), we require that leverage be less than 1.

**Figure 4a. Tax Increases**



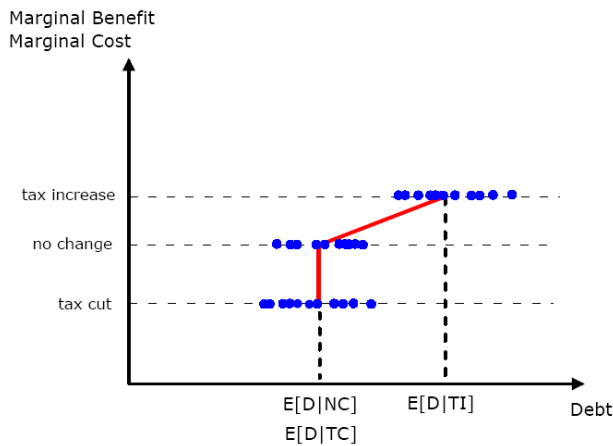
**Figure 4b. Tax Cuts**



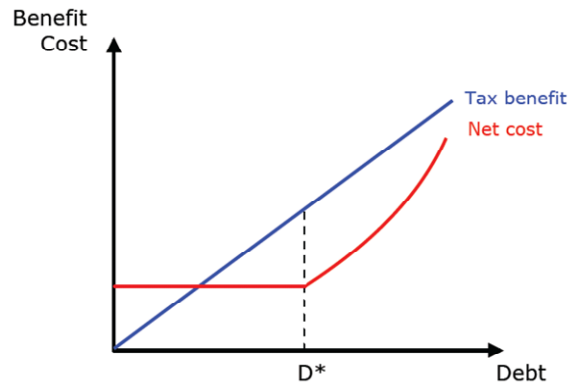
**Figure 5. Asymmetric Tax Sensitivity, Leverage Hysteresis, and the Cost of Debt.**

Figure 5a shows how our natural experiment helps trace out the marginal-cost-of-debt curve for the average firm. There are two treatments (tax increases and tax cuts) and one set of control firms (labeled “no change” and “NC” in the figure). The three clouds of dots represent the two treatment groups and the controls, respectively. Firms treated with a tax increase (TI) increase their leverage from  $E[D|NC]$  to  $E[D|TI]$  on average, whereas firms treated with a tax cut (TC) do not adjust their leverage such that  $E[D|TC]=E[D|NC]$  on average. The marginal-cost-of-debt curve is therefore positively sloped above the pre-treatment level of debt and infinitely sloped below the pre-treatment level of debt. Figure 5b illustrates the implication of this asymmetry in tax sensitivity for the standard trade-off theory of capital structure (Figure 1a). Given the marginal cost curve in Figure 5a, the total net cost is upward sloping and convex above the optimal level of debt but flat below it. Figure 5c illustrates treatment reversals. Before the tax increase, the firm’s debt is at  $D$ , the point that gives the largest difference between the dashed Tax Benefit 1 line and the dashed Net Cost curve (whose flat segment intersects the y-axis at  $C$ ). After the tax increase, the firm’s debt increases to  $D'$ , the point at which the difference between the solid Tax Benefit 2 line and the solid Net Cost curve is largest. A subsequent tax cut returns the firm’s tax benefit to the dashed Tax Benefit 1 line, but the firm’s debt remains at  $D'$ . This implies that the flat segment of the total net cost curve has shifted up from  $C$  to  $C'$ . Note that  $D'$  gives the largest difference between Tax Benefit 1 and the solid Net Cost curve. Leverage is downward sticky and tax shocks ratchet it up irreversibly. As a result, leverage is path-dependent.

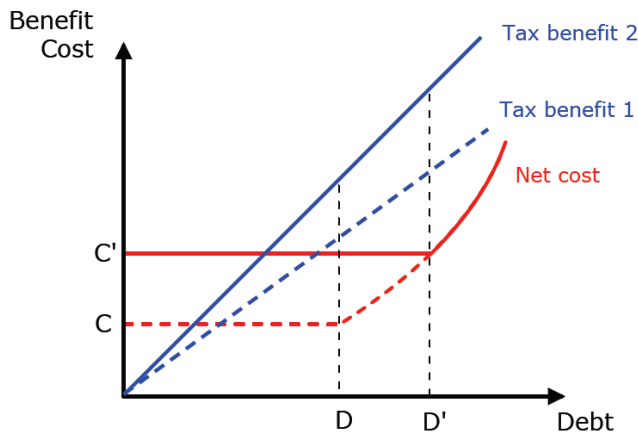
**Figure 5a: Tracing out the marginal cost curve empirically**



**Figure 5b: The modified cost of debt**



**Figure 5c: Leverage hysteresis**



**Table 1. Determinants of State Corporate Income Tax Changes, 1990-2011.**

We relate the probability that a state increases (column 1) or decreases (column 2) its corporate income taxes to the state's lagged growth rate in real gross state product and its lagged unemployment rate; the share of the state's votes going to the Democratic Presidential candidate in the most recent Presidential election; the percentage of private-sector workers in the state who are union members; and changes in the state's taxes on wage income and long-term capital gains. For variable definitions and details of their construction, see Appendix C. We estimate linear probability models with state and year fixed effects. The fixed effects are not shown for brevity. Heteroskedasticity-consistent standard errors clustered at the state level are shown in italics underneath the coefficient estimates. We use \*\*\*, \*\*, and \* to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Tax increase (1)	Tax cut (2)
<b>Economic conditions</b>		
lagged GSP growth rate	0.022 <i>0.308</i>	0.491* <i>0.268</i>
lagged state unemployment rate	0.001 <i>0.006</i>	0.010 <i>0.007</i>
<b>Political conditions</b>		
vote share of Democratic Presidential candidate	0.629*** <i>0.235</i>	-0.253 <i>0.213</i>
<b>Union power</b>		
lagged state union penetration	0.001 <i>0.006</i>	0.001 <i>0.009</i>
<b>Personal taxation</b>		
lagged change in state taxes on wages	-0.019 <i>0.014</i>	-0.010 <i>0.033</i>
lagged change in state taxes on long-term capital gains	-0.006 <i>0.006</i>	-0.005 <i>0.008</i>
<b>Diagnostics</b>		
$R^2$	11.6%	13.3%
Wald test: all coeff. = 0	3.0***	2.3***
No. of states (including DC)	51	51
No. of state-years	1,122	1,122



**Table 2. Summary Statistics.**

The sample consists of 91,172 firm-years for all non-financial and non-utility U.S. companies that are traded on the NYSE, Amex, or Nasdaq over the period 1989-2011, as per the merged CRSP-Compustat Fundamentals Annual database. The table reports summary statistics for our dependent variables and the controls. For variable definitions and details of their construction, see Appendix C. Return on assets, tangibility, firm size, and market/book are winsorized 0.5% in each tail.

	All firm-years ( $N = 91,172$ )					One year before a tax increase ( $N=1,725$ )		One year before a tax cut ( $N=6,506$ )	
	mean	s.d.	percentile			mean	s.d.	mean	s.d.
			25th	50th	75th				
<b>Firm leverage</b>									
long-term debt / assets	0.172	0.264	0.002	0.101	0.275	0.182	0.216	0.171	0.209
long-term debt (incl. current portion) / assets	0.198	0.295	0.007	0.133	0.311	0.207	0.227	0.191	0.221
(short-term and long-term debt) / assets	0.226	0.311	0.019	0.174	0.349	0.233	0.234	0.218	0.228
long-term debt / market value of assets	0.215	0.239	0.010	0.130	0.348	0.240	0.247	0.205	0.226
Long-term debt (\$m)	383.9	2,500.4	0.1	6.9	116.9	574.2	3,270.6	377.0	1,802.5
<b>State characteristics</b>									
GSP growth rate	0.029	0.026	0.012	0.029	0.046	0.014	0.018	0.033	0.024
state unemployment rate	0.058	0.017	0.046	0.054	0.066	0.054	0.017	0.059	0.017
vote share of Democratic Presidential candidate	0.489	0.073	0.438	0.486	0.534	0.515	0.062	0.504	0.070
state union membership	0.097	0.045	0.056	0.097	0.131	0.102	0.035	0.115	0.046
state sales growth rate	0.189	0.183	0.094	0.155	0.232	0.121	0.116	0.184	0.130
<b>Firm characteristics</b>									
ROA	0.034	0.273	0.009	0.104	0.166	0.055	0.243	0.046	0.256
total assets (\$m)	1,676.5	9,530.2	34.1	134.3	625.0	2,334.0	10,364.0	1,707.4	8,980.0
tangibility	0.264	0.224	0.087	0.196	0.379	0.258	0.206	0.244	0.208
market/book	1.841	1.942	0.813	1.210	2.055	1.786	1.933	1.894	2.001
<b>Credit market conditions</b>									
default spread (in %)	0.955	0.466	0.680	0.860	1.080	1.226	0.547	0.813	0.260

**Table 3. Effect of Tax Changes on Leverage.**

We estimate standard leverage regressions to test whether, and by how much, firms change their leverage in response to changes in state corporate income taxes in their headquarter state. For variable definitions and details of their construction, see Appendix C. To screen out firms with negative equity (distressed firms), we require that leverage be less than 1. Except in column 2, we capture tax changes using indicator variables for tax increases and tax cuts. In column 2, we use changes in a state's top marginal tax rate. Note that three tax increases (CA 2002, NJ 2002, and MI 2008) and one tax cut (TX 2008) cannot be summarized in terms of changes in marginal tax rates; see Appendix A and B. The unit of analysis is a firm-year. Column 4 restricts the sample of treated firms to those that suffer first a tax increase and then a subsequent tax cut ("reversals"). All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-year fixed effects to remove industry shocks. The specifications shown in columns 3 and 4 additionally include firm fixed effects in the first-difference equation and are estimated using Stata's *reg2hdfe* command for linear regressions with two high-dimensional fixed effects. The fixed effects are not reported for brevity. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use \*\*\*, \*\*, and \* to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	<i>Dep. var.: Change in long-term book leverage</i>				
	Baseline (1)	Baseline w/ changes in marginal rates (2)	Baseline w/ firm FE (3)	Reversals w/ firm FE (4)	Timing of tax changes (5)
=1 if tax rise at $t = -2$ (in %)					-0.405 <i>0.349</i>
=1 if tax rise at $t = -1$ (in %)	1.142*** <i>0.295</i>		1.155*** <i>0.340</i>	1.631** <i>0.655</i>	1.076*** <i>0.306</i>
=1 if tax rise at $t = 0$ (in %)					-0.344 <i>0.292</i>
=1 if tax rise at $t = +1$ (in %)					-0.076 <i>0.320</i>
=1 if tax rise at $t = +2$ (in %)					-0.414 <i>0.354</i>
=1 if tax cut at $t = -1$ (in %)	-0.036 <i>0.163</i>		-0.135 <i>0.202</i>	0.174 <i>0.646</i>	-0.188 <i>0.169</i>
Lagged increase in tax rate		0.347* <i>0.187</i>			
Lagged cut in tax rate		0.080 <i>0.224</i>			
Lagged change in ...					
ROA	-0.005 <i>0.004</i>	-0.005 <i>0.004</i>	-0.001 <i>0.005</i>	0.004 <i>0.007</i>	-0.005 <i>0.005</i>
firm size	0.007*** <i>0.002</i>	0.007*** <i>0.002</i>	0.003 <i>0.002</i>	0.001 <i>0.003</i>	0.009*** <i>0.002</i>
tangibility	0.037*** <i>0.009</i>	0.037*** <i>0.009</i>	0.026** <i>0.011</i>	0.021 <i>0.016</i>	0.041*** <i>0.010</i>
market/book	0.000 <i>0.000</i>	0.000 <i>0.000</i>	-0.001 <i>0.000</i>	-0.001 <i>0.001</i>	-0.001** <i>0.000</i>
default spread	-0.518*** <i>0.168</i>	-0.538*** <i>0.172</i>	-0.506*** <i>0.178</i>	-0.689** <i>0.310</i>	-0.641** <i>0.265</i>
<b>Diagnostics</b>					
$R^2$	11.2%	11.2%	21.5%	34.0%	13.1%
Wald test: all coeff. = 0	8.2***	6.3**	n.a.	n.a.	5.9***
No. of firms	8,866	8,859	8,866	5,469	7,053
No. of observations	73,547	72,890	73,547	33,915	57,278

**Table 4. Robustness.**

To investigate robustness, columns 1 and 2 split the sample in 2000; column 3 adds state fixed effects; column 4 models total leverage; column 5 models long-term leverage including debt due within one year; column 6 models market leverage; and column 7 models log real debt rather than a leverage ratio. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-year fixed effects to remove industry shocks. (The specification shown in column 3 additionally includes state fixed effects and is estimated using Stata's *reg2hdfe* command for linear regressions with two high-dimensional fixed effects.) The fixed effects are not reported for brevity. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use \*\*\*, \*\*, and \* to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	<i>Dep. var.: Change in book leverage</i>					Change in long-term market leverage (6)	Change in log real long-term debt (7)
	long-term debt, 1990- 1999 (1)	long-term debt, 2000- 2011 (2)	long-term debt (w/ state FE) (3)	short- term and long-term debt (4)	long-term (incl. current portion) (5)		
=1 if tax rise at $t = -1$ (in %, except col. 7)	1.092** <i>0.488</i>	1.163*** <i>0.369</i>	1.122*** <i>0.305</i>	0.693** <i>0.288</i>	1.150*** <i>0.283</i>	0.714** <i>0.353</i>	0.101*** <i>0.029</i>
=1 if tax cut at $t = -1$ (in %, except col. 7)	-0.094 <i>0.223</i>	0.036 <i>0.242</i>	0.013 <i>0.180</i>	0.076 <i>0.169</i>	-0.011 <i>0.165</i>	-0.072 <i>0.182</i>	-0.016 <i>1.408</i>
Lagged change in ...							
ROA	-0.002 <i>0.006</i>	-0.008 <i>0.005</i>	-0.005 <i>0.004</i>	-0.010** <i>0.005</i>	-0.012*** <i>0.004</i>	-0.019*** <i>0.003</i>	-0.016 <i>0.024</i>
firm size	0.005** <i>0.002</i>	0.008*** <i>0.002</i>	0.007*** <i>0.002</i>	0.012*** <i>0.002</i>	0.012*** <i>0.002</i>	0.028*** <i>0.002</i>	0.149*** <i>0.013</i>
tangibility	0.032*** <i>0.012</i>	0.047*** <i>0.014</i>	0.037*** <i>0.009</i>	0.063*** <i>0.010</i>	0.057*** <i>0.010</i>	0.060*** <i>0.009</i>	0.390*** <i>0.069</i>
market/book	0.000 <i>0.001</i>	-0.001* <i>0.001</i>	0.000 <i>0.000</i>	-0.001** <i>0.000</i>	-0.001 <i>0.000</i>	0.001*** <i>0.000</i>	0.012*** <i>0.003</i>
default spread	-0.416 <i>0.684</i>	-0.539*** <i>0.170</i>	-0.518*** <i>0.168</i>	-0.679*** <i>0.170</i>	-0.564*** <i>0.160</i>	-1.369*** <i>0.211</i>	-5.726*** <i>1.961</i>
<b>Diagnostics</b>							
$R^2$	10.7%	11.7%	11.3%	11.3%	11.1%	20.1%	11.4%
Wald test: all coeff. = 0	2.5**	6.6***	n.a.	15.4***	15.9***	45.2***	26.0***
No. of firms	6,967	5,559	8,866	8,839	8,850	8,892	8,866
No. of observations	37,130	36,417	73,547	73,259	73,388	74,084	73,547

**Table 5. Potential Confound: Local Business Cycle Effects.**

States may change corporate tax rates and firms may change leverage in response to unobserved changes in local business conditions. To examine this potential confound, column 1 adds lagged changes in state GSP growth rates, state unemployment rates, and state sales growth rates. Column 2 estimates a falsification test, asking whether firms respond to tax changes that occur in a neighboring state. Columns 3 and 4 compare the debt policies of firms subject to a state-level tax shock to control firms headquartered in bordering states only (column 3) and those located in far-away states (column 4). Column 5 uses a restricted sample consisting of firms in adjacent counties either side of a state border, such that in year  $t$ , one or more firms in one county experience a tax shock while one or more firms in the adjacent county do not. The effect of common local economic shocks are then removed by including county-pair/year fixed effects. Column 6 additionally requires that firms in adjacent county pairs operate in the same SIC4 industry in year  $t$ . The unit of analysis is a firm-year. All specifications except column 6 are estimated using OLS in first differences with industry-year fixed effects (not shown for brevity). Column 6 instead includes county-pair/industry/year fixed effects. For variable definitions and details of their construction, see Appendix C. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use \*\*\*, \*\*, and \* to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	<i>Dep. var.: Change in long-term book leverage</i>					
	Full sample		Firms in treated states versus ...		Firms in adjacent border counties	
	(1)	(2)	firms in bordering states (3)	firms in far-away states (4)	county-pair/year FE & industry/year FE (5)	county-pair/industry/year FE (6)
=1 if tax rise at $t = -1$ (in %)	1.113*** <i>0.295</i>	1.010*** <i>0.299</i>	1.204*** <i>0.386</i>	0.910*** <i>0.311</i>	2.370*** <i>0.849</i>	2.540*** <i>0.967</i>
=1 if tax cut at $t = -1$ (in %)	-0.012 <i>0.163</i>	-0.007 <i>0.166</i>	-0.001 <i>0.965</i>	-0.033 <i>0.169</i>	-0.170 <i>0.580</i>	0.224 <i>0.626</i>
=1 if tax rise in a bordering state at $t = -1$ (in %)		-0.305** <i>0.140</i>				
=1 if tax cut in a bordering state at $t = -1$ (in %)		0.061 <i>0.103</i>				
Lagged change in ...						
ROA	-0.005 <i>0.004</i>	-0.005 <i>0.004</i>	-0.017 <i>0.012</i>	-0.003 <i>0.004</i>	0.005 <i>0.014</i>	-0.002 <i>0.025</i>
firm size	0.007*** <i>0.002</i>	0.007*** <i>0.002</i>	0.007 <i>0.005</i>	0.006*** <i>0.002</i>	-0.001 <i>0.007</i>	-0.024** <i>0.011</i>
tangibility	0.037*** <i>0.009</i>	0.037*** <i>0.009</i>	0.021 <i>0.025</i>	0.037*** <i>0.010</i>	0.052 <i>0.038</i>	-0.057 <i>0.081</i>
market/book	0.000 <i>0.000</i>	0.000 <i>0.000</i>	-0.001 <i>0.001</i>	0.000 <i>0.000</i>	0.000 <i>0.001</i>	-0.001 <i>0.002</i>
default spread	-0.519*** <i>0.168</i>	-0.521*** <i>0.168</i>	-0.554 <i>0.338</i>	-0.517*** <i>0.196</i>	-0.914 <i>0.785</i>	0.209 <i>2.146</i>
GSP growth rate	0.021 <i>0.023</i>	0.021 <i>0.023</i>	0.056 <i>0.076</i>	0.024 <i>0.025</i>	0.247 <i>0.187</i>	0.194 <i>0.210</i>
state unemployment rate	0.168* <i>0.095</i>	0.175* <i>0.096</i>	0.781*** <i>0.299</i>	0.087 <i>0.106</i>	-0.009 <i>0.011</i>	-0.008 <i>0.015</i>
state sales growth rate	0.000 <i>0.002</i>					
<b>Diagnostics</b>						
$R^2$	11.2%	11.2%	30.8%	12.1%	49.4%	33.1%
Wald test: all coeff. = 0	6.0***	6.1***	2.9***	4.9***	n.a.	1.6*
No. of firms	8,866	8,866	5,033	8,780	2,047	448
No. of observations	73,547	73,547	10,522	64,510	10,208	1,284

**Table 6. Potential Confounds: Union Power and Political Leanings.**

Prior literature documents a positive correlation between leverage and union power, which in turn may correlate with a state's decision to raise corporate taxes. To investigate this potential confound, we partition the sample into firms that are headquartered in states with either high or low union power (columns 1 and 2) or in states suffering large or no large employment shocks (columns 3 and 4). Table 1 shows that states that lean Democratic are more likely to increase corporate taxes. To examine if this leads to a spurious correlation between tax increases and leverage increases, columns 5 and 6 partition the sample into firms that are headquartered in states leaning Democratic or Republican, respectively. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences with industry-year fixed effects (not shown for brevity). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use \*\*\*, \*\*, and \* to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. (Reflecting the signed nature of the predictions, the test for equal tax sensitivity is one-sided.)

	<i>Dep. var.: Change in long-term book leverage</i>					
	States with ...		States with ...		States leaning ...	
	high union power (1)	low union power (2)	large employment shocks (3)	no large employment shocks (4)	Democratic (5)	Republican (6)
=1 if tax rise at $t = -1$ (in %)	0.795** <i>0.353</i>	1.413*** <i>0.482</i>	1.212*** <i>0.451</i>	0.987*** <i>0.355</i>	1.381*** <i>0.449</i>	0.892* <i>0.519</i>
=1 if tax cut at $t = -1$ (in %)	-0.076 <i>0.139</i>	0.317 <i>0.485</i>	0.215 <i>0.267</i>	0.156 <i>0.288</i>	0.168 <i>0.266</i>	0.797 <i>0.598</i>
Lagged change in ...						
ROA	-0.012** <i>0.006</i>	0.007 <i>0.009</i>	-0.004 <i>0.005</i>	0.000 <i>0.007</i>	-0.004 <i>0.005</i>	-0.015 <i>0.010</i>
firm size	0.005* <i>0.002</i>	0.006 <i>0.005</i>	0.007*** <i>0.001</i>	0.010*** <i>0.003</i>	0.006** <i>0.003</i>	0.008* <i>0.005</i>
tangibility	0.030** <i>0.011</i>	0.031 <i>0.027</i>	0.052*** <i>0.011</i>	0.039 <i>0.023</i>	0.047*** <i>0.016</i>	0.002 <i>0.020</i>
market/book	0.000 <i>0.001</i>	-0.001 <i>0.001</i>	0.000 <i>0.000</i>	-0.001 <i>0.001</i>	-0.001 <i>0.001</i>	0.000 <i>0.001</i>
default spread	-0.446* <i>0.226</i>	-0.242 <i>0.492</i>	-0.389** <i>0.165</i>	-0.607*** <i>0.188</i>	-0.527*** <i>0.202</i>	-0.535 <i>0.456</i>
<b>Diagnostics</b>						
$R^2$	18.8%	28.3%	16.9%	20.3%	17.1%	23.1%
Wald test: all coeff. = 0 ( $F$ )	5.9***	3.3***	10.5***	7.3***	4.6***	1.6
Equal tax sensitivity? ( $F$ )		1.14		0.13		0.52
No. of firms	5,490	2,882	5,213	3,852	5,079	4,114
No. of observations	37,403	19,089	28,374	20,389	30,909	17,786

**Table 7. Potential Measurement Error: Location of Operations And Sales.**

Firms are taxed wherever they operate. To the extent that sample firms have operations outside the state in which they are headquartered, our leverage regressions will underestimate the sensitivity of debt to taxes. To illustrate how this potential measurement error biases our estimates downwards, we use two sample partitions. The first partitions sample firms into multinationals and domestic firms. Multinationals should be less sensitive to changes in state taxes than domestic firms. The second partitions sample firms based on whether sales in their three-digit NAICS industry are predominantly inter-state or intra-state. Firms shipping predominantly outside their home state should be less sensitive to changes in state taxes than firms producing predominantly for their headquarter state. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences with industry-year fixed effects (not shown for brevity). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use \*\*\*, \*\*, and \* to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. (Reflecting the signed nature of the predictions, the test for equal tax sensitivity is one-sided.)

	<i>Dep. var.:</i> Change in long-term book leverage			
	multi- nationals (1)	domestic firms (2)	high inter-state sales (3)	low inter- state sales (4)
=1 if tax rise at $t = -1$ (in %)	0.281 <i>0.422</i>	1.521*** <i>0.472</i>	0.370 <i>0.577</i>	1.209** <i>0.480</i>
=1 if tax cut at $t = -1$ (in %)	-0.186 <i>0.246</i>	0.120 <i>0.281</i>	-0.351 <i>0.264</i>	0.107 <i>0.297</i>
Lagged change in ...				
ROA	-0.020** <i>0.009</i>	-0.026** <i>0.012</i>	-0.015** <i>0.006</i>	0.003 <i>0.007</i>
firm size	0.008*** <i>0.003</i>	0.013*** <i>0.003</i>	0.006** <i>0.003</i>	0.007** <i>0.003</i>
tangibility	0.063*** <i>0.020</i>	0.022 <i>0.039</i>	0.057*** <i>0.017</i>	0.025 <i>0.022</i>
market/book	-0.001* <i>0.001</i>	-0.001 <i>0.001</i>	0.000 <i>0.001</i>	0.000 <i>0.001</i>
default spread	-0.608*** <i>0.216</i>	-2.194 <i>1.713</i>	-0.537** <i>0.257</i>	-0.604* <i>0.327</i>
<b>Diagnostics</b>				
$R^2$	21.6%	11.2%	10.3%	12.5%
Wald test: all coeff. = 0 ( $F$ )	5.1***	7.6***	4.2***	2.7***
Equal tax sensitivity? ( $F$ )		3.80**		2.13*
No. of firms	4,032	6,627	2,387	2,353
No. of observations	32,005	40,843	21,217	20,621

**Table 8. Heterogeneous Treatment Effects.**

Higher taxes on equity income ( $\tau_e$ ) dampen the impact of corporate tax changes on leverage. To test this comparative static, we split the sample according to two proxies for  $\tau_e$ : Dividends and institutional ownership. Non-dividend payers have lower  $\tau_e$  than dividend-payers because their investors derive their equity income solely in the form of (lower-taxed) capital gains. And firms that are predominantly owned by institutional investors have lower  $\tau_e$  than those predominantly owned by retail investors, as institutions are often tax exempt. A corollary of a causal interpretation of the observed tax sensitivity of debt is that it should vary with profits. Columns 5 and 6 partition sample firms according to whether they are profitable or loss-making in year 0. The extent to which a firm can increase its leverage when faced with a tax increase depends on its debt capacity. Columns 7 and 8 partition firms into those rated investment-grade and those rated below-investment-grade by a credit rating agency. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences with industry-year fixed effects (not shown for brevity). Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use \*\*\*, \*\*, and \* to denote significance at the 1%, 5%, and 10% level (two-sided), respectively. (Reflecting the signed nature of the predictions, the test for equal tax sensitivity is one-sided.)

	<i>Dep. var.: Change in long-term book leverage</i>							
	non-dividend payers (1)	dividend payers (2)	institutional ownership (3)	high retail ownership (4)	profitable (5)	loss-making (6)	investment grade (7)	below investment grade (8)
=1 if tax rise at $t = -1$ (in %)	1.550*** <i>0.420</i>	0.391 <i>0.476</i>	1.418*** <i>0.536</i>	0.840 <i>0.702</i>	1.133*** <i>0.313</i>	0.117 <i>0.905</i>	1.264** <i>0.535</i>	-0.100 <i>1.482</i>
=1 if tax cut at $t = -1$ (in %)	0.166 <i>0.223</i>	-0.196 <i>0.297</i>	0.168 <i>0.313</i>	-0.068 <i>0.340</i>	-0.143 <i>0.176</i>	-0.022 <i>0.511</i>	0.027 <i>0.306</i>	0.008 <i>0.806</i>
Lagged change in ...								
ROA	-0.011** <i>0.005</i>	0.007 <i>0.011</i>	0.003 <i>0.012</i>	-0.008 <i>0.006</i>	-0.008 <i>0.007</i>	0.003 <i>0.007</i>	-0.034 <i>0.025</i>	0.002 <i>0.044</i>
firm size	0.007*** <i>0.002</i>	0.005 <i>0.004</i>	0.009** <i>0.004</i>	0.005* <i>0.003</i>	0.008*** <i>0.002</i>	0.009*** <i>0.004</i>	-0.002 <i>0.007</i>	-0.006 <i>0.007</i>
tangibility	0.038*** <i>0.011</i>	0.028 <i>0.018</i>	0.067*** <i>0.024</i>	0.029** <i>0.014</i>	0.026** <i>0.011</i>	0.067*** <i>0.017</i>	0.026 <i>0.025</i>	0.065* <i>0.038</i>
market/book	0.000 <i>0.000</i>	-0.003*** <i>0.001</i>	0.000 <i>0.001</i>	0.000 <i>0.001</i>	-0.001 <i>0.000</i>	0.000 <i>0.001</i>	-0.004** <i>0.001</i>	-0.012*** <i>0.004</i>
default spread	-0.429** <i>0.217</i>	-0.757** <i>0.329</i>	-0.718*** <i>0.241</i>	-0.779* <i>0.464</i>	-0.629*** <i>0.187</i>	-0.308 <i>0.420</i>	-0.162 <i>0.313</i>	-0.924 <i>0.674</i>
<b>Diagnostics</b>								
$R^2$	16.3%	24.5%	23.5%	22.4%	14.5%	26.8%	12.5%	45.3%
Wald test: all coeff. = 0 ( $F$ )	6.5***	2.8***	4.0***	1.9*	7.2***	3.4***	9.0***	2.4**
Equal tax sensitivity? ( $F$ )		3.34**		1.01		2.29*		0.24
No. of firms	7,360	3,896	3,643	5,232	7,227	4,585	713	1,729
No. of observations	46,183	27,097	24,270	24,270	57,772	15,775	6,986	9,682