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# AS CERTAIN AS DEBT AND TAXES:
# ESTIMATING THE TAX SENSITIVITY OF LEVERAGE FROM EXOGENOUS STATE TAX CHANGES

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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 and time to show that tax considerations are a first-order determinant of firms' capital structure choices. Over the period 1990-2011, firms increase 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 due to unobserved variation in local business conditions or investment opportunities, union power, or states' political leanings. Treatment effects are heterogeneous, with greater tax sensitivity among profitable and investment-grade firms which have a greater marginal tax benefit and lower marginal cost of issuing debt, respectively.

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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 in the economy simultaneously, many states change their corporate income tax and they do so at different times, creating many treatment and control groups.[1] 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%. This is equivalent to an extra $62.1 million of debt for the average firm; it is also at least three times larger than the effect of standard determinants of leverage used in empirical capital structure studies.

Interestingly, we find that the estimated tax sensitivity of debt is asymmetric. While firms increase leverage in response to tax increases, tax cuts do not lead to a corresponding decrease in

[1] Asker, Farre-Mensa, and Ljungqvist (2011) use this natural experiment to model exogenous shocks to firms' after-tax returns on investment.

the use of debt. This is true even within-firm: Tax increases that are later reversed nonetheless lead to permanent increases in a firm's leverage – an unexpected form of path-dependence or hysteresis that has not previously been documented. The asymmetry in tax sensitivity requires a modification of the standard trade-off theory of capital structure. Rather than being linear as theory assumes, the marginal-cost-of-debt curve has a kink at the firm's pre-treatment level of debt: Reductions in debt appear to be infinitely costly at the margin. This is our second contribution. We speculate in the Conclusions what might cause capital structure to be downward sticky in this way.

To put these contributions in context, consider Figure 1. Figure 1a presents the capital structure argument typically found in any corporate finance textbook.[2] 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.[3] The optimal level of debt equates the marginal tax benefit and the marginal cost. Figure 1b illustrates the ideal experiment: Randomly assigning different tax rates to different firms and then comparing their debt policies to see if higher tax rates lead to higher leverage. Random assignment would ensure that differences in observed debt policies could not be caused by unobserved heterogeneity among the firms. This, in turn, would allow us to estimate the marginal-cost curve from shifts in the marginal (tax) benefit curve.

Observational data is, of course, not randomized. Instead, studies typically compare different firms subject to different effective tax rates. This approach is fraught with difficulties. It risks falsely attributing differences in leverage to differences in taxes when other unobserved differences across firms are also likely to affect leverage. For example, previous studies have compared firms with high and low profits. Higher profits put firms into a higher tax bracket. But higher leverage among high-profit firms is only evidence of a tax benefit of debt if both groups of firms share the same marginal cost function (see Figure 1c). Only then does the group of low-profit firms provide a

[2] See, for example, Brealey, Myers, and Allen (2011), chapter 18.
[3] Debt is costly due to bankruptcy costs (Kraus and Litzenberger (1973), Scott (1976), and DeAngelo and Masulis (1980)) 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, for example, van Binsbergen, Graham and Yang (2010)).

valid counterfactual for the behavior of high-profit firms.

But high-profit firms likely have a lower probability of default and so lower expected bankruptcy costs than low-profit firms. A simple regression cannot tell whether high-profit firms have higher leverage because debt provides valuable tax shields or because their marginal cost of debt is lower. Such unobserved heterogeneity 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. 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 in cross-sectional regressions. This is the challenge our natural experiment is designed to overcome.

The following example illustrates the essence of our identification strategy. 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.[4] 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 changes in leverage among North Carolina firms to the contemporaneous changes in leverage among firms that operate in the same industry but are located in states that did not change their corporate tax rates 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

[4] 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.

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.

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 experiences a recession and tax revenues fall, it may increase corporate income taxes to make up for the shortfall. In response to the same recession, firms in that state may have to borrow more in order 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 business conditions determine both.

We address this important concern in four separate ways. First, we show that states do not raise corporate taxes in response to local business conditions (such as state growth rates or state unemployment rates). Second, we show that observed variation in local business conditions cannot explain the observed variation in leverage. Third, we actually find *stronger* treatment effects when we restrict control firms to those located in a state bordering a state that raises corporate tax. To the extent that firms in neighboring states share similar economic conditions, this result suggests that far from leading us to overestimate the sensitivity of debt to tax rises, unobserved variation in local business conditions leads to a downward bias in our estimates. Fourth, a sharp regression-discontinuity test using firms located in adjacent counties either side of a state border confirms this: The estimated tax sensitivity more than doubles, to around 260 basis points. We show that this increase reflects a tendency for tax rises to coincide with unobserved changes in local economic conditions that would normally cause firms to reduce their leverage absent the tax change. By implication, there must be strong geographic clustering of 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.

Economic theory suggests that the value of tax shields varies with the interplay of personal and corporate taxes (Miller (1977)), a firm's profitability, and its debt capacity (i.e., the slope of the marginal-cost curve). Thus, the estimated tax treatment effects should be heterogeneous, which in turn 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 the impact of tax increases on corporate debt policies is concentrated among profitable firms; loss-making firms do not borrow more in response to tax increases. This conforms to theory since loss-making firms have no income to shield from taxes. (In fact, they can use their losses as tax shields instead.) 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

The literature on taxes and capital structure is vast.[5] Nonetheless, and perhaps surprisingly, ours is the first study to examine how taxes affect firms' capital structure decisions using 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 capital structure.

The early empirical literature found inconclusive or counter-intuitive 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) criticize cross-sectional studies that rely on firm-level estimates of marginal tax rates for being vulnerable to endogeneity concerns to the extent that firms' effective tax rates correlate with omitted variables such as profitability.

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 rely more heavily on debt

[5] Graham's (2008) survey cites more than 200 published articles.

financing. Similarly, Booth et al. (2001) find a positive relation between country-level tax rates and country-level leverage averages in a sample of 17 countries. Faccio and Xu (2011) use variation in tax rates across and within 29 OECD countries over the period 1981-2009, showing that leverage increases with taxes only in countries with low tax-evasion rates.

A common concern in cross-country studies is that treated and control firms are located in different countries. Identification assumes that, say, a Korean carmaker is a valid control for a Swiss pharma company that experiences a tax change, in the specific sense that both share the same marginal cost function (see Figure 1c). This assumption is problematic to the extent that firms in different countries differ systematically in ways that affect their debt policies.

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)).[6] But since this change in federal taxes affected all firms at (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).[7] 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:

$$\Delta D_{ijst} = \beta \Delta T^{+}_{st-1} + \gamma \Delta T^{-}_{st-1} + \delta \Delta X_{it-1} + \theta \Delta Z_{st-1} + \alpha_{jt} + \varepsilon_{ijst} \tag{1}$$

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;

[6] Lin and Flannery (2012) study the 2003 Bush cuts in personal taxes, finding an effect on firm leverage.
[7] Van Binsbergen, Graham, and Yang (2010) cleverly exploit slight timing differences in exposure to the tax reform due to variation in firms' fiscal-year ends.

$\alpha_{jt}$ 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.[8]

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 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 $X_{ist}$ and industry-year fixed effects, the tax change in a state 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.

[8] 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).

## 3. State Corporate Income Taxes

### *3.1 Overview*

Most states tax corporate activities within their borders,[9] and most do so using a tax on profits.[10] Firms are subject to state taxes if they have "nexus" with a state, usually meaning they derive income from customers in the state, have employees in the state, or own or lease property in the state.[11] 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 a comprehensive search of the "Current Corporate Income Tax Developments" feature published periodically in the *Journal of State Taxation*, we identify 38 tax increases in 25 states affecting 1,824 firms over the period 1990-2011 (see Appendix A for details). For example, in 1999, New Hampshire increased its top rate from 7% to 8%. The average shock increases top state tax rates by 90 basis points, or 13% relative to the previous year's top tax rate (though because more firms are located in states with larger tax increases, the average treated firm experiences a tax rise 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, absent a response, or a total of $3.8 billion across all treated (profitable) firms.

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

[10] 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.

[11] 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.

Eighteen of the 38 tax increases occurred in the 1990s and 20 in the 2000s. As we will show, we obtain nearly identical results in both decades. Figure 2 maps affected states over consecutive five-year periods to show the geographic and time-series distribution of the tax shocks. Geographically, tax increases rarely cluster: 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 increase taxes 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 increases could coincide with economic downturns. If firms borrow more in downturns (perhaps because their cash flows are lower and equity markets are depressed), this could lead to a spurious (as opposed to causal) correlation between tax increases and leverage. Alternatively, corporate tax increases may reflect strong union power in the state. This could lead to a spurious correlation between taxes and leverage if firms use leverage as a strategic variable to improve their bargaining power relative to labor unions, as argued by Matsa (2010). Finally, corporate tax changes could coincide with changes in personal taxes that could either amplify or attenuate the effects of corporate taxes on leverage (assuming, of course, 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 tax shields on debt.

To investigate these concerns, Table 1 relates the probability that a state changes its corporate income taxes 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 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 isn't causal. Since changes in political leanings are observable, we will 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 state corporate taxation coincide with changes in state taxes on income or capital gains.[12]

*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

[12] 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.

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.[13]

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 taxes. The only variable that has 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 meaningful but not large: 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 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),

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

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.

Because leverage measures are ratios, firm-level 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 (though the median is considerably lower, at $6.9 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 are 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 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*

The location data available from Compustat suffer from a major flaw: Rather than reporting a firm's historic headquarter location, Compustat only reports the address of a firm's *current* principal executive offices. Many authors ignore this source of measurement error, arguing that it would simply result in noise. However, as we will show, it is more likely to induce bias, for two reasons. 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 actually are not – will seem to fail to respond to a tax change (though of course there was none). Collectively, these two effects would bias the tests in favor of an incorrect null.

To remedy this, we manually 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-K, 10-Q, or S-1 filing just prior to the fiscal year-end. Filings are accessed via the SEC's EDGAR service (mostly, from May 1996 onwards) and Thomson ONE Banker (between 1990 and May 1996).[14]

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 individual firms (15.2% of all non-financial and non-utility U.S. firms in Compustat). 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 many times quite different from where they were a decade or two ago. 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 (though such blanks can usually be filled in using Compustat's zip code data).

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.

[14] 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, 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.

## 5. Empirical Results

### *5.1 Graphical Evidence*

Figure 4 provides an ocular test of the hypothesis that firms respond to tax changes by adjusting their leverage. Letting $t = 0$ denote the year in which a state changes its corporate income tax, we plot the average annual within-firm leverage change in years $t = –2$ to $t = +2$ for the group of firms experiencing a tax change at $t = 0$ ('treated' firms) and, for comparison, the group of firms not subject to a tax change in their headquarter state ('control' firms). We remove time-varying changes in industry conditions (and nation-wide variation in business conditions that affect all industries simultaneously) by including industry-year fixed effects.

Figure 4a shows responses to state tax rises. In the two years before a tax rise, changes in long-term leverage are tiny and statistically insignificant among both treated and control firms, suggesting there are no pre-trends to worry about. In the year of the tax rise, neither treated nor control firms change their leverage significantly. In year +1, on the other hand, we see sizeable and significant increases in leverage among treated firms, averaging 105 basis points ($p$<0.001), while that of control firms falls by an insignificant 13 basis points on average. The difference-in-difference estimate of 118 basis points is highly significant ($p$<0.001) and is consistent with the interpretation that firms react to an increase in corporate taxes in their home state by increasing their use of debt with a one-year lag. The effect is sizeable: Relative to the unconditional pre-increase mean of 18.2%, firms increase their long-term leverage by 6.5% in response to a tax increase (=0.0118/0.182). This is equivalent to $64.4 million more debt per firm on average.[15] 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

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

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.

*5.2 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 (so we remove firm fixed effects by estimating within-firm changes) which include a full set of SIC4-year effects (to remove the effects of unobserved time-varying industry shocks).[16]

The variables of interest in column 1 are two indicators capturing state tax increases and state tax cuts, respectively. 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.[17] 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 increase ($p$<0.001), relative to other firms in the same industry at the same time.[18] The estimated treatment effect is nearly identical to the unconditional increase of 118 basis points we saw in Figure 4a. This indicates that the covariates we control for in the regression change little around the time states increase their corporate taxes.[19] A 114 basis-point increase in leverage is economically meaningful. Relative to the average pre-treatment leverage ratio of 18.2% (see Table

[16] The industry-year effects also capture nationwide shocks that affect all industries at the same time. As we will see, their inclusion is not essential: Results are little changed if we instead include only year fixed effects.
[17] 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.
[18] Adjusting for industry makes little difference. Replacing the industry-year effects with simple year effects yields an estimated tax sensitivity of 102 basis points ($p$<0.000).
[19] We find that firms increase leverage when profitability has fallen, size has increased, assets have become more tangible, and investment opportunities (as captured by market-to-book) have deteriorated. Firms also increase leverage as conditions in the credit market improve (as captured by a reduced default spread). Economically, however, these effects are modest. One standard deviation changes in these covariates are associated with at most a 30 basis point change in leverage, i.e., no more than a quarter of the observed sensitivity to tax increases.

2), it represents an increase of 6.3% (=0.0114/0.182) or $62.1 million on average.

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

Could the observed sensitivity to tax increases simply be random? The standard errors suggest no, 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, 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.[20] The results mirror those in our baseline model: Leverage increases as tax rates go up. The coefficient estimate of 0.347 ($p$=0.063) implies that the average tax increase (which involves a 13% increase in the top rate) is followed by a 4.5% increase in leverage (=0.347*0.13), all else equal. For the average firm, this corresponds to an increase in long-term debt of $45.5 million.

*5.3 Asymmetry: Sensitivity to Tax Cuts*

In stark contrast to firms' responses to tax increases, we find no evidence that firms reduce their

[20] 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.

leverage when their home state cuts corporate income taxes. Column 1 shows an average tax-cut treatment effect of only minus 3.6 *basis* points. This has the expected negative sign but is economically tiny and not statistically significant ($p$=0.826). Conditioning on the size of the tax cut, in column 2, yields a *positive* coefficient, though again the effect is small and insignificant. These patterns mirror Figure 4b. They suggest that the tax sensitivity of debt is asymmetric: Firms increase leverage when taxes rise but 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 tax cuts remain 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 within-firm: 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.[21]

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, which we illustrated earlier in Figure 1a, does not predict this kink in the marginal cost of debt. Our

[21] 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.

results thus imply that standard trade-off theory needs to be modified as shown in Figure 5b: The total net cost of debt is upward sloping and concave above the optimal level of debt, as in standard trade-off theory, but it is flat below it.

Figure 5c illustrates the dynamics of optimal 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 its 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 ratchet it up permanently. This irreversibility is a novel form of hysteresis that has not previously been documented. We discuss possible explanations for these debt dynamics in the Conclusions.

*5.4 Pre-trends and Drift*

Column 5 of Table 3 considers the timing of the relation between tax increases and changes in leverage. It is the multivariate analog to the univariate diff-in-diff results shown in Figure 4. As in that figure, 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 the possibility that the failure to respond to tax cuts simply reflects delays, perhaps caused by significant adjustment costs incurred in reducing leverage. Such delays would imply that the coefficient on the tax cut indicator 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 annual 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 robustness tests. First, to assess possible structural breaks, columns 1 and 2 partition the sample by decade. This reveals a very modest (though insignificant) increase in the sensitivity of debt to tax rises over time: The estimated diff-in-diff is 109 basis points between 1990 and 1999 ($p$=0.025) and 116 basis points after 2000 ($p$=0.0026). Leverage is insensitive to tax cuts in both subsamples, so the tax sensitivity of debt is asymmetric throughout our sample period. By implication, our results are not driven by the tax changes adopted in any single state.

We next investigate whether the observed tax sensitivity of leverage 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 firms in, say, Utah whose tax regime stays constant. 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 marginal 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 their total leverage by 69 basis points in response to a state tax rise, relative to other firms operating in the same industry at the same time but located in other states (*p*=0.016). Even this smaller treatment effect is economically meaningful: Relative to the average pre-treatment ratio of 23.3% (see Table 2), it represents an increase of 3%.

In column 5, we exclude short-term debt but include the current portion of long-term debt (due within a year). This has practically no effect on the estimated treatment effect: On average, firms increase their 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 increases 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 to the numerator (outstanding debt). This can especially bias regressions modeling market leverage, because the same factors that cause a firm to adjust leverage (say, changes in its union's bargaining power) will 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: Following a tax rise, firms increase their long-term debt significantly (*p*=0.001) but they do not cut their long-term debt in response to a tax cut (*p*=0.256). Economically, the sensitivity to tax increases is quite large: All else equal, firms increase their outstanding debt by 10.7% following a tax rise.

### *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 (nor, implicitly, by time-varying macroeconomic shocks that hit all industries at the same time). 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. In this section, we consider three leading potential confounds: Changes in economic conditions within a state; changes in local labor market conditions; and changes in a state' 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 may need to borrow more simply because their cash flows are lower in recessions), the observed correlation between tax increases and increases in leverage may be spurious. While the 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.

Lagged GSP growth has no significant effect on leverage. The effect of lagged state unemployment rates, on the other hand, is positive, consistent with firms increasing leverage when economic conditions in their home state deteriorate, but it is only marginally significant ($p$=0.077). It is also small economically: A one-standard deviation increase in the state unemployment rate is associated with only a 17 basis point increase in leverage. Finally, variation in the state's sales growth rate has exactly no effect on leverage ($p$=0.844). 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 observed state-level economic conditions does not appear to 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 the variation in regional economic conditions is orthogonal to our controls for GSP growth, state unemployment, and variation in state sales growth rates. To isolate the potential effect of regional economic conditions, we consider a type of falsification test. Column 2 includes indicator variables capturing tax increases 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 economic conditions (orthogonal to our state-level controls) and firms in fact respond to these changes in conditions rather than to the tax changes. Then we should see firms apparently "reacting" to a tax change in a *bordering* state by changing their leverage, at least to the extent that they are exposed to similar regional economic conditions as the state next door.

Column 2 shows that, as before, firms increase leverage 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 that borders a state 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, columns 3 and 4 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 (active in the same industry and observed in the same year) that are either headquartered in a neighboring state (column 3) or that are headquartered in far-away states (column 4). If variation in regional economic conditions rather than tax shocks were the true driver of the observed sensitivity to tax increases, 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 that arguably share similar (regional) economic conditions marginally *increases* the economic magnitude of the sensitivity of leverage to tax increases (though this increase is not statistically significant).

The fact that control firms show no sign of responding to a tax increase in a bordering state (i.e., that the effects of tax changes do not spill across borders) supports a causal interpretation of the observed tax sensitivity of debt. But the estimated sensitivity may be downward biased, given the increase in treatment effect when we focus on geographically proximate control firms.

Of course, firms in neighboring states may not necessarily share the same economic conditions, for example when 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 likely share similar local economic conditions. This test, which is a form of regression discontinuity approach, is potentially quite powerful, in that it allows us to remove unobserved time-varying local economic conditions by focusing on firms in adjacent counties.

We identify a firm's county based on its zip code, using a zip-county bridge obtained from the Centers for Disease Control and Prevention.[22] We hand-collect historical zip codes from SEC filings for the 1,532 firms that our data checks indicate moved across state lines during our 1989-2011 sample period. For the remaining 8,573 sample firms, we use current zip codes as reported in Compustat. 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, as we will see, the coefficients are quite precisely estimated, so noise does not appear to be a major concern.

Over our sample period, there are 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 increases 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 cluster involving a large number of separate tax shocks.

Column 5 includes two sets of fixed effects: A set of county-pair/year fixed effects, to remove unobserved variation in economic conditions affecting all firms operating in a pair of adjacent counties located either side of a state border, and the set of industry-year fixed effects we used previously to remove unobserved variation in industry conditions that may affect leverage. To estimate this, we us 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). Economically,

[22] This is available at 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.

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

Why is the estimated tax sensitivity so much greater? Unlike all our previous specifications, column 5 does not estimate the treatment effect within-industry, due to the presence of two *independent* sets of fixed effects. In other words, it compares the change in industry-adjusted leverage of, say, a treated shoe manufacturer in year $t$ to the contemporaneous change in industry-adjusted leverage of an untreated business services provider, 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. Despite this reduction in sample size, the point estimate proves remarkably stable. Compared to firms in the same industry located just the other side of the state border, treated firms increase their leverage by an average of 254 basis points ($p$=0.009) in response to a state tax rise.

The fact that the estimates in columns 5 and 6 are nearly identical is consistent with the interpretation that local conditions have a large economic impact on tax sensitivity regardless of which industry a firm operates in. The only alternative explanation for the observed increase in tax sensitivity would be that the treated firms in these reduced samples are somehow unusual and so 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, such control firms must *cut* their leverage relative to their industry peers located elsewhere in the country. This dovetails with the significant reduction in leverage we saw among control firms when a neighboring state raises taxes (see Table 5, column 2).

Thus, what remains after excluding an industry mismatch or selection effects in the reduced treatment samples is a strong, and hitherto unknown, local business-cycle determinant of firms' debt policies. Columns 5 and 6 show that treated firms *increase* their leverage when other firms in their industry cut theirs, whereas control firms operating in the same area as the treated firms *cut* their leverage relative to their respective industry peers located farther away. This divergence in behavior implies that tax increases coincide with changes in local economic conditions that would normally cause firms to drastically reduce their leverage absent the tax change. Failing to control for variation in local economic 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 and therefore do not cut their leverage.

The adjacent-counties tests in Table 5 provide strong identification of a causal effect of taxes on debt. They show that tax increases tend to coincide with changes in unobserved local economic conditions that cause (control) firms to cut their leverage relative to their industry peers elsewhere in the country, while treated firms, faced not only with the *same* change in local conditions but also a tax rise in their home state, increase their leverage relative to their industry peers. This implies that tax rises plausibly *cause* firms to take on more debt.

*5.6.2 Potential Confound: Unobserved Changes in Investment Opportunities*

A possible explanation for the asymmetric tax sensitivity is that tax cuts correlate with better *local* investment opportunities[23] so that firms are faced with two potentially offsetting effects: A reduction in the value of tax shields, which should prompt a cut in leverage, and a simultaneous increase in their demand for capital, to which the response may be to issue more debt.

Table 5 casts significant doubt on this explanation. Whether we compare treated firms to controls headquartered in a neighboring state, focus only on firms in adjacent county pairs, or even remove unobserved variation in local *industry* conditions, we find that firms show no tendency to reduce leverage in response to tax cuts. If the alternative story were true, we would expect to see a "reverse" treatment effect, to the extent that neighboring firms share similar investment opportunities: If tax cuts did coincide with improvements in local opportunities, *control* firms should *increase* their leverage. We find no evidence of this.

*5.6.3 Potential Confounds: Union Power and Political Leanings*

We have seen that rather than making our results go away, controlling for unobserved variation in local economic conditions strengthens our results considerably. We next consider two other leading potential confounds, starting with labor market conditions. Matsa (2010) documents a positive correlation between union power and firm 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 firms' 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 which simultaneously causes tax rises and leverage increases.

Since our empirical specifications include industry-year fixed effects, they already deal with unobserved cross-industry variation in union power. To confound our results, any remaining

[23] 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.

unobserved variation in union power would thus have to be within-industry.[24] Nonetheless, it is useful to partition the sample into firms that operate in industries with either high or low union power. If we find a significant tax sensitivity of leverage even in industries with low union power, it is unlikely that the observed tax sensitivity is driven by unobserved variation in union power. As columns 1 and 2 of Table 6 show, the sensitivity of leverage to tax increases is virtually identical in both high and low union-power industries.

To test directly whether union power is likely to confound our results, we exploit variation in unionization rates across states and time. We already saw, in Table 1, that tax-increasing states are *not* more unionized than other states. This casts doubt on the possibility that states raise corporate taxes in response to lobbying by unions and thus on the hypothesis that firms increase leverage not because of the tax rise but to counter union power. To further examine this story, columns 3 and 4 of Table 6 partition the sample into firms headquartered in states with either high or low union power. (See Appendix C for details.) We find that 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, which is hard to reconcile with a union-power confound.

A related labor-market theory of capital structure holds that firms choose their debt with a view to insuring their workers against unemployment risk (see Titman (1984) for a formal model and Agrawal and Matsa (2012) for empirical evidence). To confound our results, unemployment risk would have to increase at the same time as states increase corporate taxes. To the extent that unemployment risk increases in 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 5 and 6 of Table 6, we conduct a direct test, partitioning the sample into firms headquartered in states that suffer either large or no large employment shocks at the time of a tax rise. (See Appendix C for definitions.) We observe a positive and significant tax sensitivity in both

[24] More specifically, to bias our tests it would have to vary within-state in a way that coincides with the tax changes.

groups of firms, 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.

The third potential confound we consider concerns political economy factors. Our earlier discussion of Table 1 showed that states that lean Democratic are significantly more likely to raise corporate income taxes. To examine whether this might lead to a spurious correlation between tax increases and leverage increases, columns 7 and 8 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*

It is important to note that our estimates are conservative. The reason is that 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 thus underestimate the sensitivity of debt to taxes. In fact, the estimated tax sensitivity is the weighted average response to tax changes given the geographic distribution of firms' operations. It will be lower to the extent that a firm also has operations 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. All else equal, multinationals should be less sensitive to changes in state taxes than domestic firms, as part of their tax base is abroad. Using Compustat data to identify multinationals, we find that only domestic firms' leverage responds significantly 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.

As for tax cuts, neither domestic nor multinational firms show any tendency to reduce their leverage in response. Thus, measurement error does not appear to be the cause of the observed asymmetry in tax sensitivity.

Firms are taxed not only where they produce but also where they sell. This implies that unobserved heterogeneity in where firms generate their sales could also attenuate the estimated tax sensitivity of leverage. Our second test attempts to capture this. Following Agrawal and Matsa (2012), we partition sample firms based on whether sales in their three-digit NAICS industry are predominantly inter-state or intra-state. Firms in industries shipping predominantly outside their home state should be less sensitive to changes in state taxes than firms producing predominantly for their home state. As columns 3 and 4 show, the data support this prediction. Firms in industries that ship predominantly 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 in point estimates is significant at the 10% level. This result is consistent with our conjecture that unobserved heterogeneity in the geographic location of firms' sales biases the estimated sensitivity to tax increases downward. Again, neither group of firms responds significantly 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 $\frac{dD^*}{d\tau_c}=\frac{1}{c}(1-\tau_e)$.

Thus, higher taxes on equity income dampen the impact of a corporate tax change on leverage. Because $\tau_e$ likely varies in the cross-section,[25] treatment effects should be heterogeneous. However, we cannot directly condition on $\tau_e$ in our tests as it cannot easily be measured: Not only will $\tau_e$ depend on whether a firm's marginal investor is a tax-exempt institution or a wealthy individual subject to the top rate of income tax, it will also vary 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, especially for dividends. While non-dividend payers increase leverage by 155 basis points following a tax rise ($p$<0.001), dividend payers increase leverage by an insignificant 39 basis points ($p$=0.411). The difference in tax sensitivities between these two groups is 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 an insignificant 84 basis points ($p$=0.231).

A corollary of a causal interpretation of the observed tax sensitivity of debt is that the sensitivity should vary with profits, as interest-bearing debt offers valuable tax shields only to profitable firms. Columns 5 and 6 of Table 8 partition sample firms according to whether they are profitable or loss-

[25] 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.

making in the year of the tax rise.[26] Consistent with debt being increased to take advantage of additional tax shields, we find that only profitable firms increase their leverage: When faced with a tax rise 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 among loss-making firms ($p=0.897$). This difference is marginally significant ($p=0.068$).[27]

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 (i.e., its marginal cost of debt). 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 effectively 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 staggered changes in corporate income tax rates across U.S. states and time. Our results show that firms react strongly to tax increases but are insensitive to tax cuts. These findings are robust to various potential confounds. The economic magnitudes we estimate are large, but even so they are a lower bound on the true tax sensitivity of debt owing to measurement error in the location of firms' operations. Finally, we find that geography matters for financial decisions in the sense that

[26] 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.

[27] 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 $t = 0$ ($p=0.043$).

firms behave more alike the physically closer they are to each other.

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.[28]

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 reactions that typically results). Admati et al. (2012) suggest another possible explanation: Shareholders are reluctant to reduce leverage because the benefit flows primarily to lenders as the remaining debt becomes safer. We leave further analysis of the causes of hysteresis in leverage to future research.

[28] Of course, firms do cut leverage in practice, though 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).

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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 |

## 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 *dd1*), 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.

***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 Adam and Goyal (2008). 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*].

<u>Independent variables: Credit market conditions</u>

***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.

<u>Conditioning variables</u>

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.

***Industry with high union power*** is an indicator set equal to one if the fraction of employees who belong to a labor union in the firm's Census Industry Code (CIC) exceeds the 67$^{th}$ percentile across CIC industries in year *t*, and zero otherwise. NAICS codes are mapped to CIC codes using the crosswalks provided by the U.S. Bureau of Labor Statistics and on www.unionstats.com. The data come from Hirsch and Macpherson (2003) as updated on their website, www.unionstats.com.

***Industry with low union power*** is an indicator set equal to one if the fraction of employees who belong to a labor union in the firm's Census Industry Code (CIC) is below the 33$^{rd}$ percentile across CIC industries in year *t*, and zero otherwise.

***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.

***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 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$^{th}$ 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$^{rd}$ 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$^{th}$ 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$^{rd}$ 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.

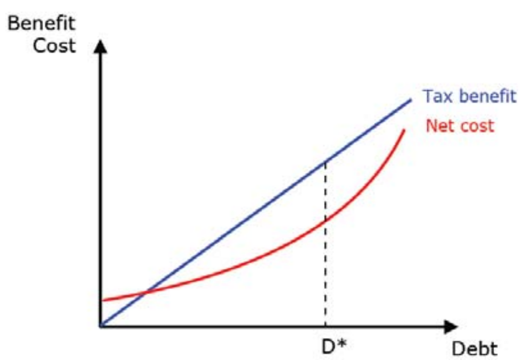
***Investment grade*** is an indicator set equal to 1 if in year *t*, the firm has an investment-grade rating from S&P, Moody's, or Fitch, using data obtained from Compustat (variable *splticrm*) and Mergent FISD, and zero otherwise. It is missing for firms without a credit rating.

***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 ($MB_1$, $MB_2$, $MB_3$,…, $MB_n$) are randomly assigned to firms and the resulting debt choices ($D_1$, $D_2$, $D_3$,…, $D_n$) 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**




Tax benefit = $[(1-\tau_i) - (1-\tau_c)(1-\tau_e)]D$
Net cost = $a + bD + cD^2$

**Figure 1b: The ideal experiment**

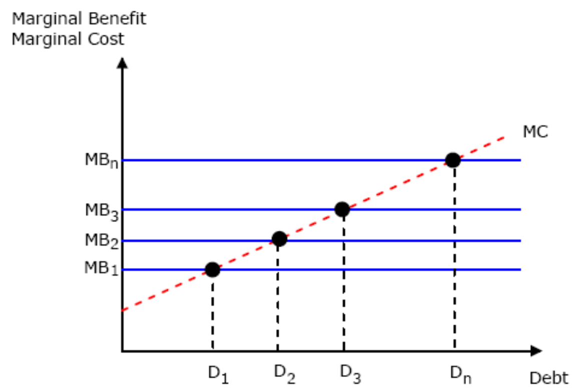



**Figure 1c: Identifying assumption for observational data**

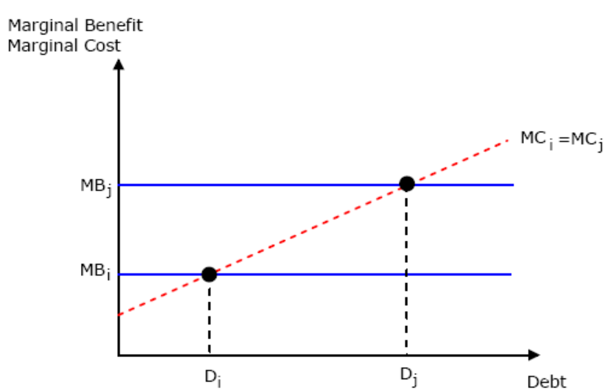



**Figure 1d: Identification challenge**

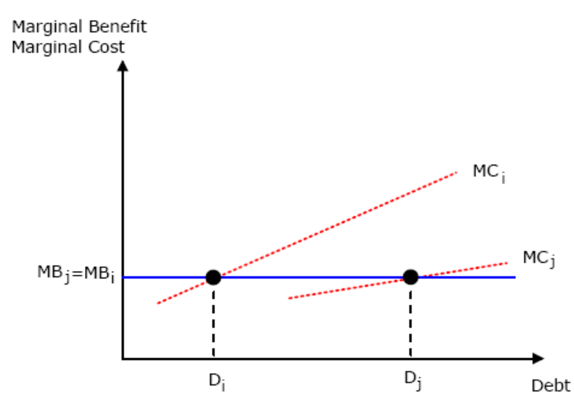


**Figure 2. Geography of State Corporate Income Tax Changes, 1990-2011.**

**Tax increases, 1990-1994**

**Tax cuts, 1990-1994**

**Tax increases, 1995-1999**

**Tax cuts, 1995-1999**

**Tax increases, 2000-2004**

**Tax cuts, 2000-2004**

**Tax increases, 2005-2009**

**Tax cuts, 2005-2009**

**Tax increases, 2010-2011**

**Tax cuts, 2010-2011**

**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.

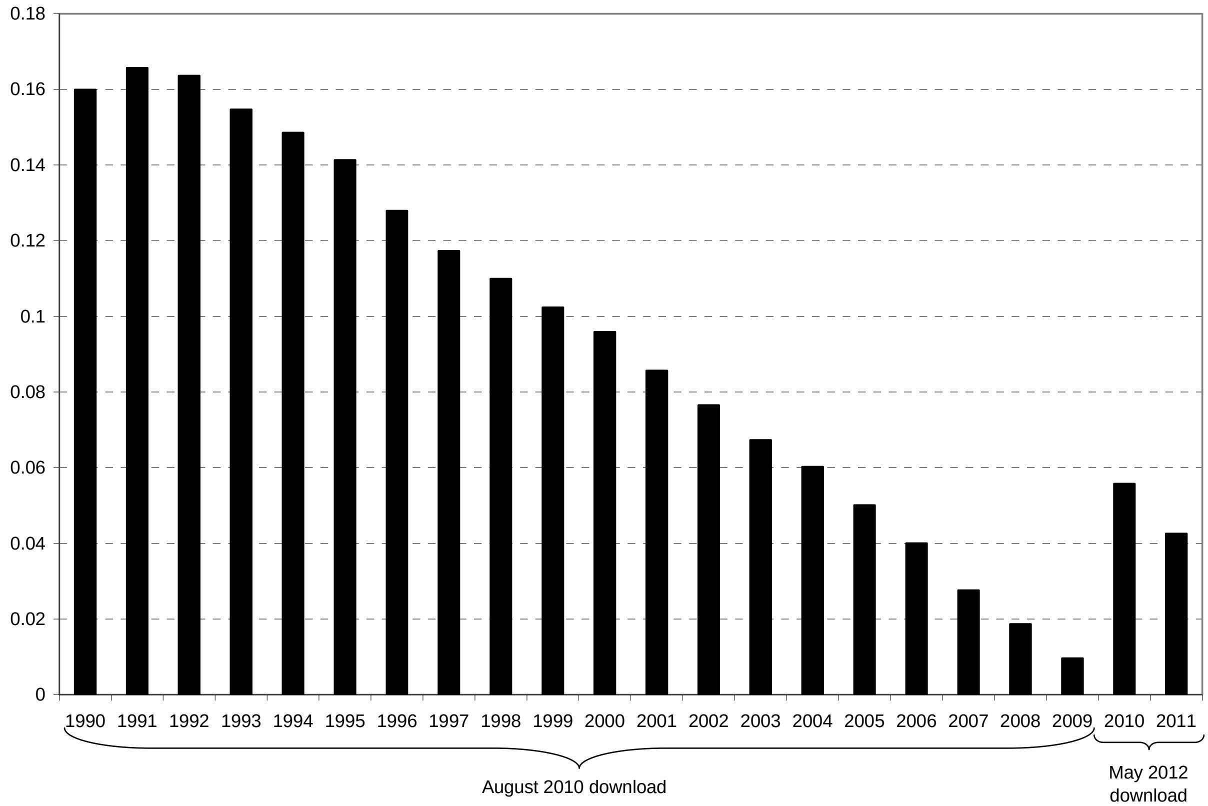


**Figure 4. Annual Changes in Leverage Around State Tax Increases and State Tax Cuts.**
The figures plot the average annual within-firm change in long-term leverage for each year in a five-year window centered on the year a state increases or cuts its corporate income tax (year 0) for treated firms (striped 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**

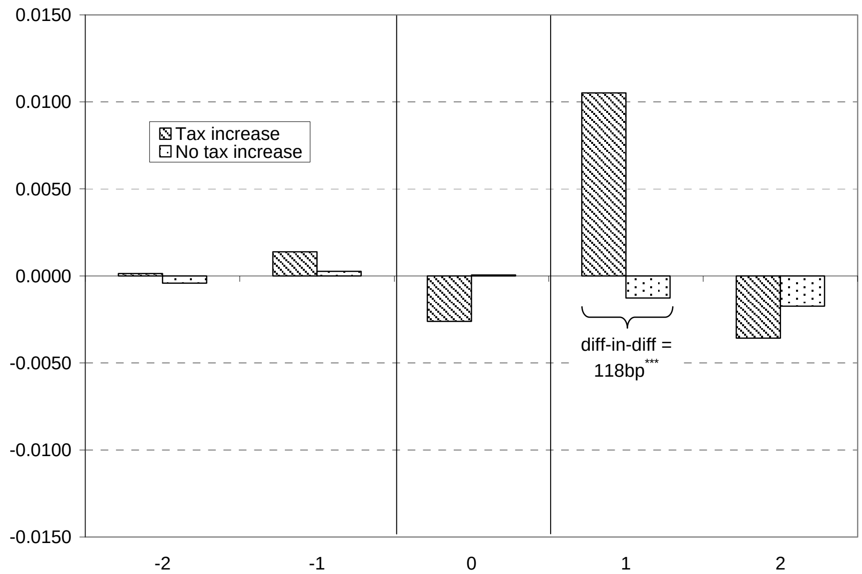



**Figure 4b. Tax Cuts**

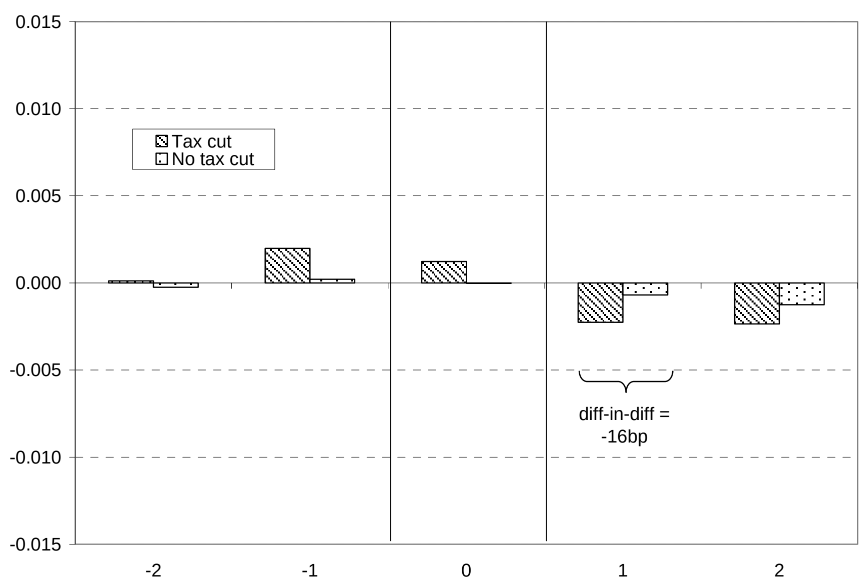


**Figure 5. Asymmetric Tax Sensitivity, Leverage Hysteresis, and the Cost of Debt.**
Figure 5a shows how our natural experiment helps trace out the marginal-cost-of-debt curve for the average firm. There are two treatments (tax increases and tax cuts) and one set of control firms (labeled "no change" and "NC" in the figure). Firms treated with a tax increase (TI) increase their leverage from E[D|NC] to E[D|TI], whereas firms treated with a tax cut (TC) do not adjust their leverage such that E[D|TC]=E[D|NC]. 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 concave 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**

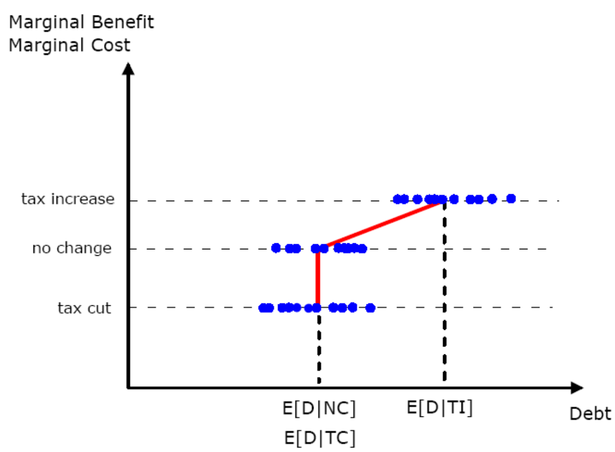



**Figure 5b: The modified cost of debt**

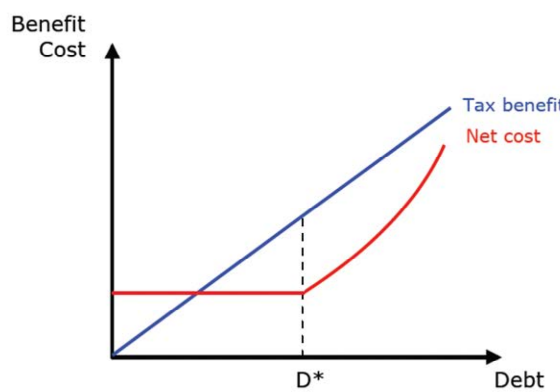



**Figure 5c: Leverage hysteresis**

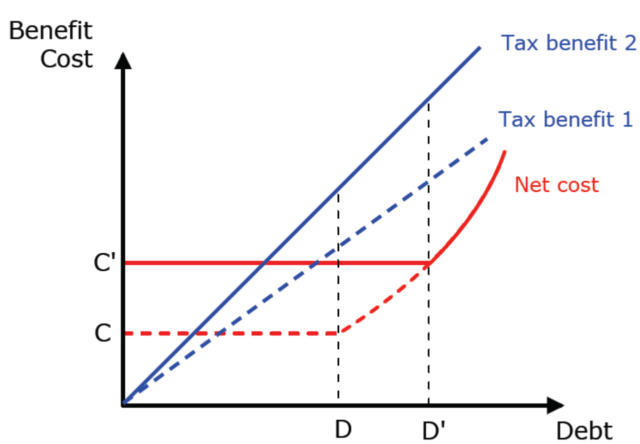


**Table 1. Determinants of State Corporate Income Tax Changes, 1990-2011.**
We relate the probability that a state increases (column 1) or decreases (column 2) its corporate income taxes to the state's lagged growth rate in real gross state product and its lagged unemployment rate; the share of the state's 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) |
|---|---|---|
| Economic conditions | | |
| lagged GSP growth rate | 0.022 | 0.491* |
| | *0.308* | *0.268* |
| lagged state unemployment rate | 0.001 | 0.010 |
| | *0.006* | *0.007* |
| Political conditions | | |
| vote share of Democratic Presidential candidate | 0.629*** | -0.253 |
| | *0.235* | *0.213* |
| Union power | | |
| lagged state union penetration | 0.001 | 0.001 |
| | *0.006* | *0.009* |
| Personal taxation | | |
| lagged change in state taxes on wages | -0.019 | -0.010 |
| | *0.014* | *0.033* |
| lagged change in state taxes on long-term capital gains | -0.006 | -0.005 |
| | *0.006* | *0.008* |
| **Diagnostics** | | |
| $R^2$ | 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) | |
|---|---|---|---|---|---|---|---|---|---|
| | | | percentile | | | | | | |
| | mean | s.d. | 25th | 50th | 75th | mean | s.d. | mean | s.d. |
| Firm leverage | | | | | | | | | |
| 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 ($m) | 383.9 | 2,500.4 | 0.1 | 6.9 | 116.9 | 574.2 | 3,270.6 | 377.0 | 1,802.5 |
| State characteristics | | | | | | | | | |
| GSP growth rate | 0.029 | 0.026 | 0.012 | 0.029 | 0.046 | 0.014 | 0.018 | 0.033 | 0.024 |
| state unemployment rate | 0.058 | 0.017 | 0.046 | 0.054 | 0.066 | 0.054 | 0.017 | 0.059 | 0.017 |
| vote share of Democratic Presidential candidate | 0.489 | 0.073 | 0.438 | 0.486 | 0.534 | 0.515 | 0.062 | 0.504 | 0.070 |
| state union membership | 0.097 | 0.045 | 0.056 | 0.097 | 0.131 | 0.102 | 0.035 | 0.115 | 0.046 |
| state sales growth rate | 0.189 | 0.183 | 0.094 | 0.155 | 0.232 | 0.121 | 0.116 | 0.184 | 0.130 |
| Firm characteristics | | | | | | | | | |
| ROA | 0.034 | 0.273 | 0.009 | 0.104 | 0.166 | 0.055 | 0.243 | 0.046 | 0.256 |
| total assets ($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 |
| Credit market conditions | | | | | | | | | |
| 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-specific 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.

| | *Dep. var.:* Change in long-term book leverage | | | | |
|---|---|---|---|---|---|
| | Baseline<br>(1) | Baseline w/ changes in marginal rates<br>(2) | Baseline w/ firm FE<br>(3) | Reversals w/ firm FE<br>(4) | Timing of tax changes<br>(5) |
| =1 if tax rise at $t$ = –2 (in %) | | | | | -0.405 |
| | | | | | *0.349* |
| =1 if tax rise at $t$ = –1 (in %) | 1.142$^{***}$ | | 1.155$^{***}$ | 1.631$^{**}$ | 1.076$^{***}$ |
| | *0.295* | | *0.340* | *0.655* | *0.306* |
| =1 if tax rise at $t$ = 0 (in %) | | | | | -0.344 |
| | | | | | *0.292* |
| =1 if tax rise at $t$ = +1 (in %) | | | | | -0.076 |
| | | | | | *0.320* |
| =1 if tax rise at $t$ = +2 (in %) | | | | | -0.414 |
| | | | | | *0.354* |
| =1 if tax cut at $t$ = –1 (in %) | -0.036 | | -0.135 | 0.174 | -0.188 |
| | *0.163* | | *0.202* | *0.646* | *0.169* |
| Lagged increase in tax rate | | 0.347$^{*}$ | | | |
| | | *0.187* | | | |
| Lagged cut in tax rate | | 0.080 | | | |
| | | *0.224* | | | |
| Lagged change in … | | | | | |
| ROA | -0.005 | -0.005 | -0.001 | 0.004 | -0.005 |
| | *0.004* | *0.004* | *0.005* | *0.007* | *0.005* |
| firm size | 0.007$^{***}$ | 0.007$^{***}$ | 0.003 | 0.001 | 0.009$^{***}$ |
| | *0.002* | *0.002* | *0.002* | *0.003* | *0.002* |
| tangibility | 0.037$^{***}$ | 0.037$^{***}$ | 0.026$^{**}$ | 0.021 | 0.041$^{***}$ |
| | *0.009* | *0.009* | *0.011* | *0.016* | *0.010* |
| market/book | 0.000 | 0.000 | -0.001 | -0.001 | -0.001$^{**}$ |
| | *0.000* | *0.000* | *0.000* | *0.001* | *0.000* |
| default spread | -0.518$^{***}$ | -0.538$^{***}$ | -0.506$^{***}$ | -0.689$^{**}$ | -0.641$^{**}$ |
| | *0.168* | *0.172* | *0.178* | *0.310* | *0.265* |
| **Diagnostics** | | | | | |
| $R^2$ | 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-specific 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.

| | *Dep. var.:* Change in book leverage | | | | | | |
|---|---|---|---|---|---|---|---|
| | 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) | Change in long-term market leverage (6) | Change in log real long-term debt (7) |
| =1 if tax rise at $t$ = –1 (in %, except col. 7) | 1.092** | 1.163*** | 1.122*** | 0.693** | 1.150*** | 0.714** | 0.101*** |
| | *0.488* | *0.369* | *0.305* | *0.288* | *0.283* | *0.353* | *2.917* |
| =1 if tax cut at $t$ = –1 (in %, except col. 7) | -0.094 | 0.036 | 0.013 | 0.076 | -0.011 | -0.072 | -0.016 |
| | *0.223* | *0.242* | *0.180* | *0.169* | *0.165* | *0.182* | *1.408* |
| Lagged change in … | | | | | | | |
| ROA | -0.002 | -0.008 | -0.005 | -0.010** | -0.012*** | -0.019*** | -0.016 |
| | *0.006* | *0.005* | *0.004* | *0.005* | *0.004* | *0.003* | *0.024* |
| firm size | 0.005** | 0.008*** | 0.007*** | 0.012*** | 0.012*** | 0.028*** | 0.149*** |
| | *0.002* | *0.002* | *0.002* | *0.002* | *0.002* | *0.002* | *0.013* |
| tangibility | 0.032*** | 0.047*** | 0.037*** | 0.063*** | 0.057*** | 0.060*** | 0.390*** |
| | *0.012* | *0.014* | *0.009* | *0.010* | *0.010* | *0.009* | *0.069* |
| market/book | 0.000 | -0.001* | 0.000 | -0.001** | -0.001 | 0.001*** | 0.012*** |
| | *0.001* | *0.001* | *0.000* | *0.000* | *0.000* | *0.000* | *0.003* |
| default spread | -0.416 | -0.539*** | -0.518*** | -0.679*** | -0.564*** | -1.369*** | -5.726*** |
| | *0.684* | *0.170* | *0.168* | *0.170* | *0.160* | *0.211* | *1.961* |
| **Diagnostics** | | | | | | | |
| $R^2$ | 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. Hetero-skedasticity-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.

| | *Dep. var.:* Change in long-term book leverage | | | | | |
|---|---|---|---|---|---|---|
| | | | Firms in treated states versus … | | Firms in adjacent border counties | |
| | Full sample | | firms in bordering states | firms in far-away states | county-pair/year FE & industry/ year FE | county-pair/ industry/ year FE |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| =1 if tax rise at $t = -1$ (in %) | 1.113*** | 1.010*** | 1.204*** | 0.910*** | 2.370*** | 2.540*** |
| | *0.295* | *0.299* | *0.386* | *0.311* | *0.849* | *0.967* |
| =1 if tax cut at $t = -1$ (in %) | -0.012 | -0.007 | -0.001 | -0.033 | -0.170 | 0.224 |
| | *0.163* | *0.166* | *0.965* | *0.169* | *0.580* | *0.626* |
| =1 if tax rise in a bordering state at $t = -1$ (in %) | | -0.305** | | | | |
| | | *0.140* | | | | |
| =1 if tax cut in a bordering state at $t = -1$ (in %) | | 0.061 | | | | |
| | | *0.103* | | | | |
| Lagged change in … | | | | | | |
| ROA | -0.005 | -0.005 | -0.017 | -0.003 | 0.005 | -0.002 |
| | *0.004* | *0.004* | *0.012* | *0.004* | *0.014* | *0.025* |
| firm size | 0.007*** | 0.007*** | 0.007 | 0.006*** | -0.001 | -0.024** |
| | *0.002* | *0.002* | *0.005* | *0.002* | *0.007* | *0.011* |
| tangibility | 0.037*** | 0.037*** | 0.021 | 0.037*** | 0.052 | -0.057 |
| | *0.009* | *0.009* | *0.025* | *0.010* | *0.038* | *0.081* |
| market/book | 0.000 | 0.000 | -0.001 | 0.000 | 0.000 | -0.001 |
| | *0.000* | *0.000* | *0.001* | *0.000* | *0.001* | *0.002* |
| default spread | -0.519*** | -0.521*** | -0.554 | -0.517*** | -0.914 | 0.209 |
| | *0.168* | *0.168* | *0.338* | *0.196* | *0.785* | *2.146* |
| GSP growth rate | 0.021 | 0.021 | 0.056 | 0.024 | 0.247 | 0.194 |
| | *0.023* | *0.023* | *0.076* | *0.025* | *0.187* | *0.210* |
| state unemployment rate | 0.168* | 0.175* | 0.781*** | 0.087 | -0.009 | -0.008 |
| | *0.095* | *0.096* | *0.299* | *0.106* | *0.011* | *0.015* |
| state sales growth rate | 0.000 | | | | | |
| | *0.002* | | | | | |
| **Diagnostics** | | | | | | |
| $R^2$ | 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 firm 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 operate in industries with either high or low union power (columns 1 and 2); that are headquartered in states with either high or low union power (columns 3 and 4); or that are headquartered in states suffering large or no large employment shocks (columns 5 and 6). 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 7 and 8 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.)

| | *Dep. var.:* Change in long-term book leverage | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | Industries with … | | States with … | | States with … | | States leaning … | |
| | high union power | low union power | high union power | low union power | large employment shocks | no large employment shocks | Democratic | Republican |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| =1 if tax rise at $t = -1$ (in %) | 1.254** | 1.182** | 0.795** | 1.413*** | 1.212*** | 0.987*** | 1.381*** | 0.892* |
| | *0.539* | *0.522* | *0.353* | *0.482* | *0.451* | *0.355* | *0.449* | *0.519* |
| =1 if tax cut at $t = -1$ (in %) | -0.374 | -0.111 | -0.076 | 0.317 | 0.215 | 0.156 | 0.168 | 0.797 |
| | *0.328* | *0.315* | *0.139* | *0.485* | *0.267* | *0.288* | *0.266* | *0.598* |
| Lagged change in … | | | | | | | | |
| ROA | -0.019 | -0.003 | -0.012** | 0.007 | -0.004 | 0.000 | -0.004 | -0.015 |
| | *0.012* | *0.006* | *0.006* | *0.009* | *0.005* | *0.007* | *0.005* | *0.010* |
| firm size | 0.003 | 0.009*** | 0.005* | 0.006 | 0.007*** | 0.010*** | 0.006** | 0.008* |
| | *0.004* | *0.003* | *0.002* | *0.005* | *0.001* | *0.003* | *0.003* | *0.005* |
| tangibility | 0.038** | 0.011 | 0.030** | 0.031 | 0.052*** | 0.039 | 0.047*** | 0.002 |
| | *0.016* | *0.018* | *0.011* | *0.027* | *0.011* | *0.023* | *0.016* | *0.020* |
| market/book | -0.002* | 0.000 | 0.000 | -0.001 | 0.000 | -0.001 | -0.001 | 0.000 |
| | *0.001* | *0.001* | *0.001* | *0.001* | *0.000* | *0.001* | *0.001* | *0.001* |
| default spread | -0.472 | -0.474* | -0.446* | -0.242 | -0.389** | -0.607*** | -0.527*** | -0.535 |
| | *0.364* | *0.245* | *0.226* | *0.492* | *0.165* | *0.188* | *0.202* | *0.456* |
| **Diagnostics** | | | | | | | | |
| $R^2$ | 20.4% | 16.7% | 18.8% | 28.3% | 16.9% | 20.3% | 17.1% | 23.1% |
| Wald test: all coeff. = 0 (*F*) | 3.1*** | 2.9*** | 5.9*** | 3.3*** | 10.5*** | 7.3*** | 4.6*** | 1.6 |
| Equal tax sensitivity? (*F*) | 0.01 | | 1.14 | | 0.13 | | 0.52 | |
| No. of firms | 3,366 | 4,603 | 5,490 | 2,882 | 5,213 | 3,852 | 5,079 | 4,114 |
| No. of observations | 19,621 | 23,531 | 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.)

| | *Dep. var.:* Change in long-term book leverage | | | |
|---|---|---|---|---|
| | multi-nationals | domestic firms | high inter-state sales | low inter-state sales |
| | (1) | (2) | (3) | (4) |
| =1 if tax rise at $t = -1$ (in %) | 0.281 | 1.521*** | 0.370 | 1.209** |
| | *0.422* | *0.472* | *0.577* | *0.480* |
| =1 if tax cut at $t = -1$ (in %) | -0.186 | 0.120 | -0.351 | 0.107 |
| | *0.246* | *0.281* | *0.264* | *0.297* |
| Lagged change in … | | | | |
| ROA | -0.020** | -0.026** | -0.015** | 0.003 |
| | *0.009* | *0.012* | *0.006* | *0.007* |
| firm size | 0.008*** | 0.013*** | 0.006** | 0.007** |
| | *0.003* | *0.003* | *0.003* | *0.003* |
| tangibility | 0.063*** | 0.022 | 0.057*** | 0.025 |
| | *0.020* | *0.039* | *0.017* | *0.022* |
| market/book | -0.001* | -0.001 | 0.000 | 0.000 |
| | *0.001* | *0.001* | *0.001* | *0.001* |
| default spread | -0.608*** | -2.194 | -0.537** | -0.604* |
| | *0.216* | *1.713* | *0.257* | *0.327* |
| **Diagnostics** | | | | |
| $R^2$ | 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.)

| | *Dep. var.:* Change in long-term book leverage | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | non-dividend payers | dividend payers | high institutional ownership | high retail ownership | profitable | loss-making | investment grade | below investment grade |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| =1 if tax rise at $t$ = –1 (in %) | 1.550*** | 0.391 | 1.418*** | 0.840 | 1.133*** | 0.117 | 1.264** | -0.100 |
| | *0.420* | *0.476* | *0.536* | *0.702* | *0.313* | *0.905* | *0.535* | *1.482* |
| =1 if tax cut at $t$ = –1 (in %) | 0.166 | -0.196 | 0.168 | -0.068 | -0.143 | -0.022 | 0.027 | 0.008 |
| | *0.223* | *0.297* | *0.313* | *0.340* | *0.176* | *0.511* | *0.306* | *0.806* |
| Lagged change in … | | | | | | | | |
| ROA | -0.011** | 0.007 | 0.003 | -0.008 | -0.008 | 0.003 | -0.034 | 0.002 |
| | *0.005* | *0.011* | *0.012* | *0.006* | *0.007* | *0.007* | *0.025* | *0.044* |
| firm size | 0.007*** | 0.005 | 0.009** | 0.005* | 0.008*** | 0.009*** | -0.002 | -0.006 |
| | *0.002* | *0.004* | *0.004* | *0.003* | *0.002* | *0.004* | *0.007* | *0.007* |
| tangibility | 0.038*** | 0.028 | 0.067*** | 0.029** | 0.026** | 0.067*** | 0.026 | 0.065* |
| | *0.011* | *0.018* | *0.024* | *0.014* | *0.011* | *0.017* | *0.025* | *0.038* |
| market/book | 0.000 | -0.003*** | 0.000 | 0.000 | -0.001 | 0.000 | -0.004** | -0.012*** |
| | *0.000* | *0.001* | *0.001* | *0.001* | *0.000* | *0.001* | *0.001* | *0.004* |
| default spread | -0.429** | -0.757** | -0.718*** | -0.779* | -0.629*** | -0.308 | -0.162 | -0.924 |
| | *0.217* | *0.329* | *0.241* | *0.464* | *0.187* | *0.420* | *0.313* | *0.674* |
| **Diagnostics** | | | | | | | | |
| $R^2$ | 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 |