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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 121 staggered changes in corporate income tax rates across U.S. states to show that tax considerations are a first-order determinant of firms' capital structure choices. Over the period 1990-2011, firms increase long-term leverage by 104 basis points on average (or $32.5 million in extra debt) in response to an average tax increase of 131 basis points. Contrary to static trade-off theory, the tax sensitivity of leverage is asymmetric: firms do not reduce leverage in response to tax cuts. Using treatment reversals, we find this to be true even within-firm: tax increases that are later reversed nonetheless lead to permanent increases in a firm's leverage – an unexpected and novel form of hysteresis. Our findings are robust to various confounds such as unobserved variation in local business conditions, union power, or unemployment risk. Treatment effects are heterogeneous and confirm the tax channel: tax sensitivity is greater among profitable and investment-grade firms which respectively have a greater marginal tax benefit and lower marginal cost of issuing debt.

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As is well known, debt confers a tax benefit on firms when interest payments can be deducted from taxable income. While this tax advantage of debt has been a cornerstone of corporate finance since at least Modigliani and Miller (1963), its empirical relevance continues to be disputed: opinions in the literature range from irrelevance to the belief that taxes are the key driver of debt policy.[1]

We contribute to this debate by providing well-identified evidence showing that taxes are a first-order determinant of U.S. firms' capital structure choices. In the past, identifying the effect of taxes on capital structure has proven to be empirically challenging owing to a variety of endogeneity problems. We address this identification challenge by exploiting variation in corporate income tax rates across U.S. states and time. Unlike federal tax changes, which occur infrequently and affect all firms simultaneously, many states change their corporate tax rates (we identify 121 changes over the period 1989 to 2011). Importantly, they do so at different times. The staggered nature of the tax changes provides a set of counterfactuals for how leverage would have evolved in the absence of the tax changes and so allows us to disentangle the effect of taxes on leverage from other forces shaping debt policy. Because the tax changes vary firms' marginal benefit of debt without, as we will show, affecting their marginal cost of debt, we can trace out the marginal-cost-of-debt curve for U.S. firms.

Our first contribution is to show that leverage responds to tax changes. Using a difference-in-difference approach, 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. Total assets are unchanged, implying that firms swap debt for equity when tax rates rise.

Interestingly, the estimated tax sensitivity is asymmetric: while firms borrow more in response to tax rises, tax cuts do not lead to a corresponding cut in leverage. This is true even within-firm: tax rises that are later reversed nonetheless increase leverage permanently. This dynamic pattern suggests that leverage not only responds asymmetrically to tax changes but also is path-dependent and so exhibits hysteresis. While hard to reconcile with static models, asymmetry (arising from

[1] See Graham and Leary (2011) for a recent survey documenting this range of opinions.

differences in adjustment costs) is often imposed by assumption in dynamic models of capital structure; our results lend support to this assumption. Hysteresis is a dynamic implication of asymmetry: if shareholders have a preference for leverage increases, leverage will ratchet up over time (see Admati et al. (2013) for a model rationalizing our findings). Uncovering asymmetry and hysteresis, neither of which has previously been documented, is our second contribution.

To understand our identification strategy and relate our findings to the literature, consider the ideal experiment. The capital structure argument typically found in finance textbooks[2] decomposes the value of a levered firm into the value of the unlevered firm plus the tax benefit of debt minus the (net) cost of debt.[3] The optimal level of debt then equates the marginal tax benefit and the marginal cost. Figure 1a illustrates this textbook "trade-off theory" of capital structure. The ideal experiment to test the trade-off theory is shown in Figure 1b: it consists of randomly assigning different tax rates to firms and then comparing their debt policies to see if higher tax rates lead to higher leverage. Random assignment would ensure that observed differences in leverage could not be caused by unobserved firm-level heterogeneity. This, in turn, would allow us to estimate the marginal-cost curve from shifts in the marginal (tax) benefit curve.

Observational data are, of course, not random. Many empirical studies relate observed differences in debt policies among firms to differences in their actual tax rates. This approach is fraught with difficulties; it risks falsely attributing observed differences in leverage to differences in taxes when other unobserved differences across firms also likely affect leverage. For example, prior work exploits the fact that higher profits put firms into a higher tax bracket. As a result, high-profit firms may borrow more to take advantage of tax shields, as shown in Figure 1c. But it is equally possible that they borrow more because their default risk is lower than that of low-profit firms. Figure 1d illustrates the extreme case in which the null hypothesis of irrelevance (i.e., taxes have no effect on leverage) is true. As drawn, we would falsely reject the null, as all of the observed change

[2] See, for example, Brealey, Myers, and Allen (2011), chapter 18.
[3] Debt is costly due to bankruptcy (Kraus and Litzenberger (1973)) and debt-overhang inefficiencies (Myers (1977)). To isolate the tax benefit of debt, non-tax benefits of debt (e.g., curbing free-cash flow problems (Jensen (1986)) are usually counted as negative costs for expositional purposes (see, e.g., van Binsbergen, Graham, and Yang (2010)).

in leverage in Figure 1d is due to differences in the marginal cost of debt. Studies of this kind can thus not identify the causal relation between leverage and taxes. The upshot is that in the presence of unobserved differences in marginal costs, the effect of taxes on debt is not identified. This is the challenge our natural experiment is designed to overcome.

A simple example serves to illustrate the essence of our identification strategy and potential challenges to it. In 1991, North Carolina raised its top corporate income tax rate from 7% to 8.06%. Following this tax rise, firms headquartered in NC increased long-term leverage from 18.8% to 20.8% on average. The tax rise is plausibly exogenous from the viewpoint of an individual firm in NC: for a start, firms presumably do not lobby for tax increases.[4] But this is not sufficient to establish causality since other coincident developments could be responsible for the leverage increase. For example, NC may be home to firms from an industry that suffered some other, non-tax-related leverage-increasing shock in 1991. Or investment opportunities in NC may have changed in 1991 in a way that made an increase in debt desirable, regardless of the tax rise.

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

The identifying assumption central to a causal interpretation of our diff-in-diff estimates is that treated and control firms share parallel trends. Our tests show that their pre-treatment trends are indeed indistinguishable. The question, as in any diff-in-diff set-up, is whether post-treatment trends would have continued to be parallel had it not been for the tax change. Our empirical design takes

[4] 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.

several steps to mitigate this concern.

First, we include industry-year fixed effects. This ensures that we are comparing treated and control firms operating in the same industry, allowing us to difference away unobserved time-varying industry shocks to post-treatment trends in leverage. Second, we condition on changes in standard firm-level covariates of leverage (such as profits or asset tangibility) that could cause trends to diverge post-treatment for reasons unrelated to the tax changes. We find that adding such controls has virtually no effect on the estimated tax sensitivity. This implies that the tax shocks are close to random at the firm level, such that they do not coincide systematically with changes in firm characteristics. Third, we exploit the fact that many firms are treated repeatedly over our long sample period. This allows us to difference away unobserved firm-specific trends in leverage levels.

These design choices deal with firm- and industry-level challenges to the parallel-trends assumption. Two further challenges remain. The first is that state tax changes are triggered by some observed or unobserved factor that in turn causes firms to adjust their leverage for reasons unrelated to the tax change itself. For example, we show that tax increases are more likely when the state runs a budget deficit, and vice versa for tax cuts. This suggests that tax changes are countercyclical at the state level. Korajczyk and Levy (2003) show that leverage also tends to move countercyclically. It is thus possible that local business cycle variation triggers both tax changes and leverage changes.

To disentangle how much of the observed leverage changes comes from changes in local business conditions and how much comes from tax changes, we first condition on observed variation in local business conditions, such as changes in state growth or unemployment rates. (Nationwide business conditions are held constant through the inclusion of industry-year fixed effects.) This reveals that leverage is neither countercyclical nor procyclical with respect to state growth, though leverage increases in state unemployment rates.

The geographic richness of our data allows us to go further, removing the effects of *unobserved* variation in local economic conditions. We exploit the fact that economic conditions are likely to be similar in neighboring states whereas the effects of state tax policy stop at the state's border. This policy discontinuity allows us to difference away unobserved local variation in economic conditions

and so to disentangle what part of the observed leverage change is due to tax changes. In other words, by comparing treated firms to their neighbors, we can ensure that leverage trends are parallel after removing the effects (if any) of variation in local economic conditions.[5]

Interestingly, we find a roughly 30% *stronger* treatment effect when we difference away unobserved changes in local economic conditions by restricting control firms to those located in states bordering a treated state or by focusing on treated and control firms located in contiguous counties on either side of a state border. To the extent that neighboring firms share similar economic conditions, this shows that unobserved changes in local conditions cannot be driving the observed sensitivity of leverage to tax increases. At the same time, the observed increase in tax sensitivity is of independent interest. It suggests that treated firms' leverage responses to tax increases spill across state borders, inducing their untreated neighbors to reduce their leverage in response. Since this finding holds within-industry, treated firms' neighbors are likely to be product-market competitors, which may account for the spillover (see Brander and Lewis (1985)). Documenting evidence of such geographic interdependencies in corporate debt policies is our third contribution.

The only remaining confound our research design cannot difference away is an omitted variable that varies within states across time in a way that coincides with the tax changes and whose influence on leverage stops at the state border. An obvious example is the possibility that when changing tax rates, states systematically enact other (non-tax) legislation that plausibly causes firms to adjust their leverage. A thorough review of the political economy surrounding each of the major tax changes finds no evidence that corporate tax changes coincide systematically with other policy changes that could affect leverage independently. Nor do we find that union strength – which could influence tax policy and so have an indirect effect on leverage that stops at the state border – drives our results. But the caveat, as in any research design that uses policy variation, is that we cannot rule out the existence of unexplored confounds whose influence coincides geographically with that of the variation in tax policy we exploit for identification.

The magnitude of the estimated tax sensitivity is economically meaningful. In our baseline

[5] Dube, Lester, and Reich (2010) use this logic to test whether higher minimum wages destroy low-skilled jobs.

model, the average treated firm responds to the average tax rise of 131 basis points by increasing its long-term leverage by 104 basis points, from 18.3% to 19.34%. This corresponds to an extra $32.5 million of debt and a tax elasticity of 0.53. The implied marginal cost of debt curve is fairly flat, with a slope of $403 in extra cost for every $1 million in new debt, consistent with the low leverage ratios listed firms tend to have in the U.S.: on average, listed firms have ample debt capacity.

Our elasticity estimate assumes that the average firm is exposed to the full amount of the change in its state tax rate. This would not be the case if it could deduct state taxes from federal taxes; however, the federal Corporate Alternative Minimum Tax limits such deductions. It also would not be the case if a sizeable portion of its tax base lay outside its home state. Firms do not disclose which states they are taxed in, but when we explore a relevant proxy, we find that the tax elasticity of firms taxed predominantly in their home states is 0.63, only a little larger than the estimated baseline elasticity of 0.53.

Theory suggests that the value of tax shields varies with the interplay of personal and corporate taxes (Miller (1977)), profitability, marginal tax rates, and debt capacity. This suggests four 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 a proxy for personal taxes that varies in the cross-section, we find evidence to support this comparative static. The second test shows that unlike profitable firms, loss-making firms do not borrow more in response to tax rises. This is consistent with a link between taxes and leverage since loss-making firms have no profits to shield from taxes. The third test shows that firms with higher effective marginal tax rates respond more strongly to tax increases. The fourth test shows that the sensitivity of debt to tax increases is concentrated among investment-grade firms (which have flatter marginal-cost curves) and absent among firms rated junk. Each of these validation tests supports the presence of a tax channel.

1. Related Literature

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

the estimated effect of taxes on firms' capital structure decisions.

The early empirical literature found inconclusive results, leading 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."[6] Subsequent work has tried to fill the gap, but at least Fama (2011) remains unconvinced, commenting that "the big open challenge in corporate finance is to produce evidence on how taxes affect … optimal financing decisions." This is the challenge we seek to take up.

To clarify our contribution, we briefly discuss prior attempts at exploiting variation in tax rates to identify the tax sensitivity of debt, beginning with cross-country studies. Rajan and Zingales (1995) find that firms in countries with higher corporate tax rates use more debt. Similarly, Booth et al. (2001) find a positive relation between country-level tax rates and country averages of leverage in a sample of 17 countries. Faccio and Xu (2013) use variation in tax rates across and within 29 OECD countries to show that leverage increases with taxes but only in countries with low tax evasion. (Unlike us, they do not allow for asymmetry and do not study dynamics.)

A common concern in cross-country studies is that treated and control firms are located in different countries and so may differ in unobserved ways that affect 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)).[7] 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).[8] Using state-level tax changes, which are staggered across states and time, thus provides potentially cleaner identification.

[6] MacKie-Mason (1990) and Graham (1996a,b) find a positive correlation between estimates of a firm's estimated or simulated marginal tax rate and its debt policy. However, Fama and French (1998) caution that cross-sectional studies of this kind are vulnerable to endogeneity biases as firms' effective tax rates may correlate with omitted variables.
[7] Alternative one-off shocks that have been used include the 2003 Bush cuts in personal taxes (Lin and Flannery (2013)) and Belgium's 2006 introduction of a notional tax charge benefiting the use of equity (Panier et al. (2013)).
[8] Van Binsbergen, Graham, and Yang (2010) cleverly exploit slight timing differences in exposure to the 1986 tax reform due to variation in firms' fiscal-year ends.

2. State Corporate Income Taxes

Most states tax corporate activities within their borders,[9] usually by taxing profits.[10] Firms are taxed in every state they have "nexus" with, i.e., where they have a physical presence. States distinguish between multi-state firms (those with nexus with multiple states) and single-state firms. Under the Uniform Division of Income for Tax Purposes Act, multi-state firms pay taxes in each nexus state. This involves apportioning the firm's federal taxable income to each nexus state, typically using an apportionment formula based on an average of the fractions of the firm's total sales, payroll, and property located in that state.[11] The federal Corporate Alternative Minimum Tax limits firms' ability to deduct their state taxes when computing their federal income tax liability.

Data on which states each firm has nexus with are not available. Clearly, a multi-state firm will have less exposure to a given state's tax change than a single-state firm. While this does not matter for identification, it does affect the interpretation of the estimated economic magnitudes. We return to this issue in Section 5.3.2.

State taxes are a meaningful part of U.S. firms' overall tax burden. For the average (median) sample firm, state taxes account for 21% (13.7%) of total income taxes paid. 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.[12]

2.1 Variation in State Corporate Income Taxes

Appendix A lists 43 tax increases in 24 states affecting 2,212 sample firms in fiscal years 1989-2011. For example, in 1999, New Hampshire increased its top corporate tax rate from 7% to 8% for

[9] The exceptions, as of 2012, are NV, SD, and WY.
[10] The exceptions, as of 2012, are OH, TX, and WA, which use a gross receipts tax assessed on revenue rather than on income. In a gross-receipts tax environment, interest expenses are not tax-deductible.
[11] The focus on sales, payroll, and property reduces the scope for firms shifting profits into low-tax states by way of transfer pricing arrangements.
[12] For the purposes of our experiment, it is immaterial that state taxes are small relative to federal taxes. Variation in state tax rates directly translates into variation in the total taxes a firm pays. In other words, what is important for our experiment is the magnitude of the variation in tax rates, not the level of state tax rates vs. federal tax rates.

firms with fiscal years ending on or after July 1, 1999.[13] The average tax shock increases rates by 93 basis points, a 13.8% increase from a year earlier. The tax rise experienced by the average treated firm is larger, at 131 basis points, because more sample firms are located in states with larger tax rises. (The firm-level median is 89 basis points and the standard deviation is 117 basis points.)

Appendix B lists 78 tax cuts in 27 states affecting 7,283 firms. For example, in 2001, New York cut its tax rate from 8% to 7.5% for firms with fiscal years beginning on or after July 1, 2001. The average tax cut in our sample reduces state tax rates by 55 basis points. Given the geographic distribution of firms, this corresponds to an average cut of 69 basis points for the average sample firm. (The firm-level median is 46 basis points and the standard deviation is 90 basis points.)

To put these numbers into perspective, consider the effect on firms' tax bills. In the year before a tax rise, the average (profitable) sample firm headquartered in that state earns pre-tax income of $224.9 million. Relative to this baseline, a tax rise would cost it an additional $2.9 million in taxes a year, absent a response. In the year before a tax cut, the average (profitable) firm earns $226.6 million. A tax cut would save it $3.1 million in taxes a year, all else equal.

Figure 2 maps treated states over consecutive five-year periods to show the time-series and geographic distribution of the tax shocks. Twenty-four of the 43 tax increases occurred in 1989-1999 and 19 after 2000, while 45 of the 78 tax cuts occurred in 1989-1999 and 33 after 2000. (As we will show, our results are nearly identical in either time period.) The busiest quinquennia for tax increases are 1989-1993, 1999-2003, and 2009-2012. Tax cuts are spread fairly evenly across the sample period. Figure 2 reveals little geographical clustering for either tax increases or cuts.[14]

2.2 Do State Tax Changes Coincide With Other Leverage-Relevant Changes?

Our identification strategy assumes that absent a tax change, treated firms' leverage would have evolved in the same way as that of suitable control firms. This parallel-trends assumption would be violated if state corporate tax changes coincided systematically with variation in the business cycle

[13] In coding which firms are affected by tax changes when, we are careful to capture whether a tax change affects firms with fiscal years *ending* or *beginning* on or after the effective date. This affects when it makes sense for a firm to react.
[14] There are only two cases of neighboring states raising taxes at the same time: IL/KY (1989) and MO/NE/OK (1990). Of the 78 tax cuts, 60 are isolated cases. The remaining 18 form 9 pairs: AZ/CO (1990), CO/NE (1993), ME/NH (1993), NJ/PA (1994), NY/CT (1999), AZ/CO (2000), NY/CT (2000), KY/OH (2005), and NY/VT (2007).

or labor market conditions or with changes in other taxes or state policies that affect firms' demand for debt regardless of the corporate tax change. For example, if states raise taxes in economic downturns, and downturns induce firms to borrow more as Korajczyk and Levy (2003) argue, we would observe a spurious (rather than causal) correlation between taxes and leverage. Alternatively, tax increases may reflect strong union power in the state. This could lead to a spurious correlation if firms use leverage strategically when bargaining with their unions, as Matsa (2010) argues.

To get a sense of the scope for such confounds, we relate states' tax policies to their economic and political conditions. Columns 1 to 3 of Table 1 report summary statistics of our explanatory variables. (For all variable definitions and details of their construction, see Appendix C.) As expected, economic and political conditions play an important role in states' tax policies. Compared to states that cut taxes, those that raise taxes are significantly more often governed by a Democrat (62.2% versus 39.2%, p=0.022), tend to run budget deficits (averaging 2%, compared to surpluses averaging 4.3% among tax-cutting states), are more likely to have suffered a credit-rating downgrade, and experience slower growth in gross state product (GSP, averaging 1.8% versus 3.0%). Unemployment rates and union penetration rates, on the other hand, are statistically similar in tax-cutting and tax-raising states.

States' fiscal policies do not exist in a vacuum: the fiscal policies of their neighbors also likely play a role. To capture the effects of tax competition among states, we compare each state's corporate tax rate to the rates levied by the states it borders. This reveals an interesting pattern. States that raise tax rates previously charged substantially lower taxes than their neighbors, while states that cut taxes were previously among the highest-taxing states in their region. To illustrate, before raising tax rates, the average state's rate is two percentage points below the highest rate levied by its neighbors. By contrast, before cutting taxes, the average state's tax rate is only 50 basis points below that of its highest-taxing neighbor. This suggests that states cut taxes when their rates are uncompetitive relative to their neighbors and feel freer to raise taxes when their neighbors levy higher taxes still, all else equal.

Next, we estimate linear-probability models of states' decisions to raise or cut corporate taxes.

The models include year and state fixed effects and cluster the standard errors at the state level. Column 5 shows that the variable with the largest economic effect on tax rises is the election cycle: states are 5.3 percentage points more likely to raise taxes if the next gubernatorial election is three years away than in an election year (p=0.028). Given an unconditional probability of taxes rising of 3.3% per year, this is a sizeable effect. Tax rises are also more likely the larger (more negative) the state's budget deficit the year before (p=0.05) and less likely if the state's tax rate is already high relative to its neighbors' rates (p=0.003). None of the other economic conditions has a significant effect, nor do Republican and Democratic governors differ in their propensity to raise taxes.

The election cycle is also the largest determinant of corporate tax cuts in column 6. Cuts are six percentage points more likely in the year before an election than in other years (p=0.045). Further evidence that politics plays a role in tax cuts comes from the governor's party affiliation: Democratic governors are 3.7 percentage points less likely to cut taxes than are Republican governors (p=0.026). Budget surpluses increase the likelihood of a tax cut (p=0.067) while a ratings downgrade reduces it by a sizeable amount (5.9 percentage points, p=0.024). Tax competition also has a large economic effect: for every percentage point by which a state's tax rate exceeds that of its neighbors, the likelihood of a tax cut increases by 3.6 percentage points (p<0.001). This is large relative to the unconditional mean of 6.6%.

Columns 7 to 9 model the magnitude (rather than likelihood) of the tax changes, with similar results: the main factors are the election cycle, the governor's political affiliation, the state's budget balance, ratings downgrades, and tax competition.

While the political factors revealed in Table 1 have no obvious link to corporate capital structure decisions, the economic factors do. As a result, our diff-in-diff specifications will control for observed variation in economic conditions at the state level. In a separate test using firms that are geographically close, we exploit the inherent discontinuity of tax policy (i.e., the fact that it stops at the state's border) to difference away unobserved variation in local economic conditions.

The only challenge to the parallel-trends assumption that remains is that corporate tax changes might coincide with other important state-level changes that could affect firms' demand for debt. To

investigate this, we collect data on changes in state taxes on personal income, capital gains, or banks as well as data on changes in state investment incentive programs (i.e., tax credits for investment, R&D, and job creation). Changes in personal taxes could either amplify or attenuate the effects of corporate taxes on leverage (Miller (1977)). Changes in bank taxes affect the supply of credit in a state (Farre-Mensa and Ljungqvist (2013)). And changes in investment incentive programs could change firms' demand for debt independent of any contemporaneous tax effect.

Panel B of Table 1 tabulates the results. Around half of the 43 tax rises coincide with increases in personal income and capital gains tax rates, but as we will show, these do not confound our results. Corporate tax cuts rarely coincide with changes personal taxes, and when they do, personal taxes are about as likely to go up as down. 28 of the 43 corporate tax rises coincide with increases in bank taxes, so firms' demand for debt often increases at the same time as the supply of loans in their state falls. For firms that are financially dependent on local banks, this could affect their ability to borrow more in response to tax rises and so reduce the estimated sensitivity to corporate tax increases.[15] Our estimates may thus be conservative (to the extent that our sample includes a substantial number of treated firms that are financially dependent on local banks). There is little evidence that corporate tax changes coincide with changes in state investment incentive programs.

Finally, the Internet Appendix provides thumbnail sketches of the political economy surrounding the largest tax changes (those with 100 or more treated firms). These show that the largest tax changes in our sample do not systematically coincide with anything in particular.

3. Sample and Data

3.1 Firm-level Sample

Our firm-level sample consists of all U.S. companies traded on the NYSE, Amex, or Nasdaq in the 1989-2011 fiscal years that satisfy the following filters. From the merged CRSP-Compustat Fundamentals Annual database, we exclude financial firms (SIC=6; 43,105 observations), utilities (SIC=49; 4,950 observations), public-sector entities (SIC=9; 1,271 observations), non-U.S. firms

[15] More problematic would be if corporate tax rises coincided with bank tax cuts, for then we could not be sure whether an observed increase in leverage reflected the effect of corporate tax on capital structure or the effect of increased local loan supply on financially dependent firms. Luckily, corporate and bank taxes are always changed in the same direction.

(14,040 observations), and firms traded OTC or in the Pink Sheets (1,648 observations). We also drop firm-years with negative or missing total assets (138 observations) or missing return on assets (568 observations), and firms with a single panel year (968 observations) or a CRSP share code >11 (REITS etc.; 1,255 observations). Finally, while cleaning up firms' headquarter states (see below), we filter out 1,000 observations of firms that were headquartered outside the U.S. The final sample consists of 91,487 firm-years for 10,112 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).

3.2 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 (measured either in book or market value terms) and focus either on total leverage (the sum of short-term and long-term debt over total assets) or on long-term leverage.

Our results are robust to using any of these measures, but there are at least two good reasons to expect long-term book leverage to be the most sensitive to tax changes. First, short-term debt is often used 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. Second, with a positively sloped yield curve, long-term debt generates larger tax shields for a given amount of new borrowing than does short-term debt.[16]

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, we obtain similar results using market leverage). As Table 2 shows, long-term book leverage averages 17.2% in the overall sample, 18.3% before a tax rise, and 17.2% before a tax cut.

It is useful to model not just leverage but also debt levels. Being ratios, variation in leverage measures could capture variation in the denominator (the book or market value of assets) rather than

[16] Long-term debt 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. As we will show, we obtain similar results in either case.

the numerator (the level of debt). This could give a misleading picture of a firm's response to tax changes, for example if firms systematically sell assets when faced with a tax rise.

3.3 Control Variables

We control for the standard firm-level 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 $1,683 million in total assets, 26.4% of which is tangible, and trades at a market-to-book ratio of 1.84. In addition, we use the default spread (the difference between the yield on Baa and Aaa rated corporate bonds, measured in each firm's fiscal-year-end month) to control for conditions in the credit markets. This averages 0.956%. Finally, we control for economic conditions in a firm's home state using the growth in gross state product and the state unemployment rate. These average 2.9% and 5.8%, respectively.

3.4 Firm Headquarter Locations

Compustat's location data suffer from a major flaw: Compustat reports the address of a firm's *current* principal executive office, not its historic headquarter location. Ignoring this will lead to bias. If the null of no association between tax and leverage is false, false negatives (firms that are in fact located in a tax-change state but appear not to be) will reduce the estimated tax sensitivity, as their leverage changes despite the (apparent) absence of a tax change. Similarly, false positives (firms that appear to be located in a tax-change state but in fact are not) will seem to fail to respond to a tax change (though of course there was none). Either will bias the test in favor of a false null.

To remedy this, we extract historic headquarter states from regulatory filings. Specifically, for each fiscal year, we look up each sample firm's HQ state as listed in the firm's most recent 10-Q prior to the fiscal year-end using the SEC's EDGAR service (from May 1996 onwards) and Thomson Research (between 1989 and May 1996). Errors prove widespread, affecting a non-trivial fraction of the Compustat universe. Overall, Compustat's location data are incorrect in 9,268 firm-years (10.1% of the total) affecting 1,541 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: while

4% of firms' HQ states are misrecorded for fiscal year 2011, 16.6% are misrecorded 20 years earlier.[17] Thus, where firms are located today is often quite different from where they were located a decade or two ago. Cleaning up these data allows us to remedy 192 false positive and 286 false negative tax increases and 596 false positive and 635 false negative tax cuts.

4. Empirical Strategy

We use a standard difference-in-difference approach to examine the effect of changes in states' corporate income tax rates on firms' use of debt. To inform our empirical specification, Figure 3 tests graphically whether and when leverage responds to tax changes. It plots the average annual within-firm change in long-term leverage in years $t = -2$ to $t = +2$ for the group of firms experiencing a corporate income tax change in their home state at $t = 0$ ('treated' firms) and, for comparison, the group of firms not subject to a tax change in their home state ('control' firms). Time-varying changes in industry conditions are removed by including industry-year fixed effects.

Figure 3a shows responses to tax rises. In the two years before a tax rise, leverage changes are tiny and statistically insignificant for both treated and control firms, confirming there are no pre-trends. In the year of the tax rise, neither group adjusts leverage much. In year t+1, on the other hand, we see sizeable and significant increases in leverage among treated firms, averaging 96 basis points (p<0.001 clustered by state), while the leverage of control firms falls by an insignificant 13 basis points on average. The diff-in-diff estimate of 109 basis points is highly significant (p=0.001). It is consistent with the interpretation that firms respond to higher taxes in their home state by borrowing more, with a one-year lag. There is no evidence in the figure that firms subsequently reverse these leverage increases in year t+2.

The effect of tax cuts, shown in Figure 3b, is quite different. Neither treated nor control firms change their leverage by much, if at all, in the five years surrounding a tax cut. In year 0, treated

[17] The COMPHIST file available on WRDS, which provides limited historical HQ information, unfortunately starts only in 2007. A new database, the WRDS SEC Analytics Suite, aims to provide users "historical information on state of incorporation and headquarters", among other items. SEC Analytics appears to pull HQ information not from the filing itself, but from EDGAR's "filing detail page." Unfortunately, this page is frequently out of date for years at a time, apparently because the SEC does not update its database on firm locations in a timely fashion. SEC Analytics also has problems matching filings to the correct gvkey, for example (but not exclusively) when two firms merge. As a result, SEC Analytics misses around one third of the corrections we make to Compustat's HQ location variable.

firms reduce their leverage by a little relative to control firms, but the diff-in-diff estimate measures only 9 basis points. One year later, treated firms increase their leverage by 1 basis point on average, and then reduce is again by 2 basis points in year 2. None of these diff-in-diff estimates is statistically significant. Overall, firms appear to respond to tax changes asymmetrically.

The patterns in Figure 3a could potentially be driven by coincident changes in firms' financial characteristics that are unrelated to the tax changes or they could reflect variation in state-level conditions that cause both tax changes and leverage changes. To control for these, we estimate regressions of the following form:

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

where *i*, *j*, *s*, and *t* index firms, industries, states, and years; Δ is the first-difference operator; D_{ist} is a measure of debt usage; ΔT^{+}_{st-1} and ΔT^{-}_{st-1} are indicators equaling 1 if state *s* increased or cut its top marginal corporate income tax rate in year *t*–1, respectively;[18] X_{it-1} and Z_{st-1} are time-varying firm- and state-level control variables; α_{jt} are SIC4 industry-year fixed effects which remove unobserved time-varying industry shocks;[19] and ε_{ijst} is the usual error term. Standard errors are clustered at the state level; later, they will be validated using randomly generated pseudo shocks.

First-differencing removes unobserved firm-specific fixed effects in the corresponding levels equation and, unlike a levels specification with firm fixed effects, can easily accommodate repeated treatments (i.e., the possibility that a firm experiences a sequence of tax increases or tax cuts over its time in the panel), treatment reversals (a tax increase followed some time later by a tax cut, or vice versa), and asymmetry in firms' responses to tax changes.

Regression (1) generalizes the illustrative example in the introduction in two ways. First, it exploits variation in taxes across many states and years, rather than just North Carolina's 1991 tax

[18] We also relate changes in leverage to the magnitude of the tax change, but when we do so, we lose two tax increases (CA 2002 and NJ 2002) and one tax cut (TX 2008) which cannot be quantified in terms of changes in marginal tax rates. Their directional effects on firms' tax shields, however, are unambiguous (see Appendix A and B), allowing us to include them when estimating equation (1).

[19] 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).

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 correlates 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.

The key identifying assumption is that conditional on covariates X_{it-1} and Z_{st-1} and on industry-year fixed effects, treated and control firms share parallel trends in the absence of a tax change. In that case, the estimates of β and γ in regression (1) give the causal treatment effects of tax increases and tax cuts on debt. We examine the plausibility of this identifying assumption in Section 5.6.

5. The Effects of Tax Changes on Corporate Debt Policies

5.1 Sensitivity to Tax Rises

Table 3 reports our baseline results. The estimates show that following a tax rise, firms increase their long-term leverage by 104 basis points on average ($p<0.001$), measured relative to other firms in the same industry that are not subject to tax changes in their own headquarter state that year. Compared to the average pre-treatment leverage ratio of 18.3% (see Table 2), this represents an increase of 5.7% (=0.0104/0.183).

The coefficient estimate is nearly identical to the unconditional leverage increase of 109 basis points found in Figure 3a, despite adding firm-level controls. This indicates that state tax changes are essentially random at the firm level, such that they do not coincide systematically with changes in firm characteristics. Economically, the firm-level controls have quite modest effects: one standard deviation changes in profitability, size, tangibility, or market-to-book are associated with at most a 25 basis point change in leverage – less than a quarter of the observed sensitivity to tax rises. The effect of lagged changes in state unemployment rates is positive and statistically significant, consistent with firms borrowing more when economic conditions in their home state worsen. But it is fairly small: a one-standard deviation increase is associated with only a 17 basis point increase in

leverage. Lagged changes in GSP growth have no significant effect on leverage (p=0.207).[20]

To assess the impact of cleaning up firms' historic HQ locations, column 2 uses Compustat's backfilled location data. This yields an estimated sensitivity to tax increases of 75 basis points (p=0.001), 29 basis points below the "true" estimate of 104 basis points shown in column 1. This confirms our conjecture that measurement error in firms' HQ locations leads to attenuation bias.

Could the observed sensitivity to tax increases simply be random? The standard errors suggest not, but an alternative way to answer this question is to generate "pseudo shocks" as in Bertrand, Duflo, and Mullainathan (2004). Specifically, we randomly generate 1,000 sets of 43 "pseudo tax increases" and 78 "pseudo tax cuts" (to match the observed number of actual tax shocks). Since the pseudo shocks are random, we know that the null of no tax sensitivity is true. Indeed, the mean of the 1,000 estimates of the effect of the pseudo tax increases or pseudo tax cuts on leverage is zero. More interestingly, we *never* see coefficients as large as those estimated using the actual tax increases. Thus, based on these simulations, there is a zero in 1,000 chance of randomly observing the Table 3 coefficients when the null of no tax sensitivity is in fact true. This suggests that the clustered standard errors in Table 3 are, if anything, slightly conservative.

So far, we have lumped all tax changes – large and small – together by focusing on binary tax-change indicators. The reason we do so is that two tax increases (CA 2002 and NJ 2002) and one tax cut (TX 2008) cannot be quantified in terms of changes in marginal tax rates, though their directional effects on tax shields are unambiguous (see Appendix A and B). Columns 3 and 4 exclude treated firms affected by these three tax changes to explore whether firms respond more strongly to larger tax changes. Column 3 includes separate indicators for tax changes in the bottom, middle, and top terciles.[21] The coefficients line up as expected: not surprisingly, we see the largest leverage increase, averaging 131 basis points (p=0.001), following the largest tax increases. The sensitivity to medium-sized tax increases is smaller, at 91 basis points (p=0.083), and the sensitivity to the smallest tax increases is lower still, at an insignificant 71 basis points (p=0.179). Column 4, in

[20] Conditioning on the state's political conditions, as in Table 1, changes the tax sensitivity by a single basis point.
[21] The cut-offs are 50 and 125 basis points for tax increases and –50 and –37.5 basis points for tax cuts, respectively.

turn, regresses changes in long-term leverage on the actual change in tax rates. This confirms the results of column 3: leverage increases by more, the more tax rates go up (p=0.025).

As noted in Section 2, firms are taxed where they operate. To the extent that sample firms have operations outside their headquarter state, our leverage regressions will underestimate the sensitivity of debt to taxes. This is because the tax sensitivity we estimate is the weighted average response to tax changes given the geographic distribution of a firm's operations. The response will be lower if a firm also operates in states that experience no tax changes.

A useful proxy for the extent to which a firm's profits are exposed to changes in its home-state tax rate is the location of its sales. Due to the way states apportion taxable profits, all else equal, firms selling predominantly at home should respond more strongly to tax changes than firms selling predominantly in other states. Firm-level data on the location of sales are not available, so instead we follow Agrawal and Matsa (2012) and partition firms based on whether sales in their NAICS3 industry are predominantly inter-state or intra-state. The results in column 5 confirm our prior. Compared to the average Compustat firm in column 4, firms in industries that tend to sell mostly within-state increase their leverage by somewhat more when their home state increases taxes.

5.2 Sensitivity to Tax Cuts

In contrast to their sensitivity to tax rises, we find no evidence that firms cut leverage following a tax cut. The point estimate for tax cuts in column 1 has the expected negative sign but it is tiny: a single *basis* point (p=0.943). It is true that over our sample period, tax cuts are smaller, on average, than tax increases. This could potentially account for the lack of response to tax cuts: given positive fixed adjustment costs, firms have no reason to react to small tax cuts. However, the specification in column 3 shows that firm do not even react to the very largest tax cuts: the point estimate for the 'large tax cut' indicator is only 0.009 – less than one basis point (p=0.970). The tax cuts in this bin average 89 basis points, with a range from 50 to 350 basis points. Using Compustat's backfilled location data (column 2), conditioning continuously on the size of the tax cut (in column 4), or restricting the sample to those selling predominantly in their home state (column 5) does not alter this finding.

Some states announce a sequence of annual tax cuts spread over a few years. (For example, NY cut the top corporate income tax rate from 9% to 7.5% in three annual half-point increments starting in 1999.) Column 6 restricts the tax-cut treatment to the first in such a sequence to investigate whether firms respond more strongly to the first cut (in anticipation of future cuts) than to later cuts. This increases the magnitude of the leverage response, from –1 basis point to –15.3 basis points, but the effect remains economically small and statistically insignificant (p=0.248).

Overall, these patterns mirror the unconditional diff-in-diff results shown in Figure 3b. They suggest that the tax sensitivity of debt could be asymmetric: firms increase leverage when taxes rise but apparently do not reduce leverage when taxes are cut. However, 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 apparent asymmetry. Column 7 adds firm fixed effects to the first-difference specification (alongside the industry-year effects already included). It thereby removes unobserved firm-specific trends in leverage levels (or equivalently, unobserved time-invariant firm characteristics in first differences). Even when we look within-firm, we find an asymmetric tax sensitivity: while the coefficient for tax increases becomes marginally larger, the effect of tax cuts remains economically small, at –10 basis points, and statistically insignificant (p=0.629).

Including firm fixed effects helps rule out spurious asymmetry, but our data permit an even stronger test. We can restrict the treatment sample to 765 firms experiencing treatment *reversals*, meaning they first face a tax increase and then, some time later, a tax cut (possibly in another state, if they have moved in the meantime). Using this treatment group and again including firm fixed effects to ensure we capture within-firm reversals, column 8 shows evidence of an interesting form of dynamic asymmetry: when hit with a tax rise, firms increase their leverage strongly and significantly but when later experiencing a tax cut, the same firms fail to reduce leverage again.

The apparent irreversibility of tax-induced leverage increases is a novel form of hysteresis that has not previously been documented. It implies that tax rises – but not tax cuts – leave permanent marks on firms' capital structures. To test this, we estimate a cross-sectional leverage regression in

levels, using only the last panel year τ for each firm.[22] The variables of interest count how many tax rises and tax cuts a firm has experienced since the start of our panel in 1989 (or since going public, if later). The results, including standard controls and industry-year fixed effects with standard errors clustered at the state level, are as follows:

$$\begin{aligned} leverage_{i\tau} = & \underset{(0.006)}{0.010}\, number\_tax\_increases_{i\tau-1} - \underset{(0.002)}{0.001}\, number\_tax\_cuts_{i\tau-1} \\ & - \underset{(0.012)}{0.050}\, ROA_{i\tau-1} + \underset{(0.022)}{0.155}\, tangibility_{i\tau-1} + \underset{(0.002)}{0.011}\, size_{i\tau-1} - \underset{(0.002)}{0.004}\, market\_to\_book_{i\tau-1} \\ & - \underset{(0.022)}{0.002}\, default\_spread_{i\tau-1} - \underset{(0.178)}{0.233}\, GSPgrowth_{s\tau-1} - \underset{(0.002)}{0.007}\, unemployment_{s\tau-1} + \nu_{jt} \\ R^2 = 43.4\% \qquad & No.obs. = 6{,}780 \end{aligned}$$

The estimates confirm that the echoes of past tax increases (but not of past tax cuts) can still be felt in today's capital structures: all else equal, a firm's leverage is one percentage point higher for every tax increase it has experienced since 1989 or since going public (p=0.088). Interestingly, this point estimate is nearly identical to the treatment effects estimated in Table 3.

5.3 Discussion

5.3.1 Asymmetry and Hysteresis

Static trade-off models predict neither asymmetry nor hysteresis. The reason is that they assume a smooth marginal cost curve (see Figure 1a). But asymmetry implies a kink in the marginal cost curve. Figure 4a illustrates how the standard textbook treatment would have to be modified to accommodate such a kink: the total net cost of debt is upward sloping and convex above the current debt level, as in static trade-off theory, but would need to be flat below it. Further, to square trade-off theory with hysteresis, the kink would need to ratchet up with tax increases but not come down in response to a tax cut.[23] Figure 4b illustrates these dynamics. A firm hit with an increase in its marginal tax benefit increases its debt from D to D', but a subsequent cut in its marginal tax benefit leaves debt unchanged at D'. This implies that the flat segment of the net cost curve moves up from C to C' so that the kink moves up and to the right with each tax increase, leaving the firm at the

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

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

kink. Leverage is thus downward sticky and tax increases ratchet it up permanently.

Dynamic models of capital structure often impose asymmetry exogenously, by assuming that firms find it costlier to reduce leverage than to increase it. This is consistent with an asymmetric response to tax changes. The key insight is that once leverage is in place, shareholders may not benefit from reducing it voluntarily even when doing so would increase overall firm value. Fischer, Heinkel, and Zechner (1989), for example, note informally that when shareholders are unable to commit to a future capital structure policy, they will have no incentive to reduce debt. Leland (1994) similarly observes that "although the total value of the firm would be increased by the [debt] restructuring, equity holders cannot benefit from [a debt] repurchase," as leverage gives shareholders an option to default. Reducing leverage reduces the value of this put option. Taking this logic to the extreme, Goldstein, Ju, and Leland (2001) and Chen (2010) go as far as ruling out leverage reductions by assumption. Our findings lend some justification to this assumption.

The literature also suggests some institutional reasons why firms may find it more difficult to reduce leverage than to increase it. One is co-ordination failures: debt-holders may engage in free-riding if the debt is widely dispersed (Bolton and Scharfstein (1996)). Lowering leverage reduces default risk and so increases the value of the remaining debt. Anticipating the incentive of others not to tender their debt in the repurchase, each debt-holder's optimal strategy is to hold out as well. Another reason involves legal transactions costs. The 1939 Trust Indenture Act requires unanimous consent from all bondholders in case of a material change to any feature of a public bond, such as principal, interest, or maturity (Denis and Mihov (2003)). Finally, lowering leverage requires the firm to reduce its cash balance, sell assets, cut its dividend, or issue equity. Each of these funding options likely involves significant costs: reducing cash balances may reduce the firm's ability to withstand future shocks or to pursue investment opportunities (Almeida, Campello, and Weisbach (2004)); asset sales may involve discounts to fundamental value, due to adverse selection or asset specificity (Shleifer and Vishny (1992)); cutting the dividend risks sending a negative signal to investors (John and Williams (1985)); and issuing equity can involve a lemons discount (Myers and Majluf (1984)). None of these problems arises when a firm increases leverage.

We are not aware of prior work predicting hysteresis. In contemporaneous work, Admati et al. (2013) construct a model to rationalize the ratchet effect we document. The model builds on the conflict of interest between shareholders and dispersed debt-holders familiar from dynamic capital structure theories: to reduce debt, a firm must buy it back at the prevailing market price, but shareholders resist a debt buy-back because it reduces the value of their put option. Moreover, shareholders have an incentive to issue more debt as they do not internalize the increase in bankruptcy costs faced by the existing creditors. Admati et al. show how shareholders' preferences for leverage increases create dynamics matching those we document empirically.

5.3.2 Economic Magnitudes

A handy way to measure the economic magnitude of the observed tax sensitivity in Table 3 is as an elasticity, i.e., $(\partial L / \partial \tau_c)/(L / \tau_c)$ where L denotes leverage. The numerator is the relevant coefficient estimate from column 4 (i.e., 0.39 for increases and 0.054 for cuts). For the denominator, we set τ_c and L equal to their respective sample means.[24] This gives an elasticity of 0.53 for tax increases and 0.07 for tax cuts.

These calculations make two assumptions: 1) that a firm's overall tax rate changes by the full amount of the change in its state tax rate; and 2) that all of the average treated firm's profits is exposed to the tax change (i.e., that the average firm operates – and is taxed – only in its home state). The first assumption reflects the fact that the federal Corporate Alternative Minimum Tax limits firms' ability to deduct state taxes from federal taxes. The specification shown in column 5 shows that the second assumption is not unreasonable. The tax sensitivity of firms that sell predominantly in their home state, and so come close to having 100% exposure to home-state tax changes, is moderately larger than for the average Compustat firm in column 4. This implies that the average firm in our baseline sample has less than 100% exposure to home-state taxes, as a result of having nexus with other states. Our baseline elasticity of 0.53 with respect to tax increases, which

[24] A firm's effective tax rate τ_c equals its total income expense (Compustat variable *txt*) divided by its pre-tax income (Compustat variable *pi*). In our sample, effective tax rates for treated firms average 25% and 23.7% in the year before a tax rise and tax cut, respectively. See Table 2 for the relevant pre-treatment sample means of L.

assumes a 100% exposure, is thus downward biased, but not by much. Using the restricted sample of firms that predominantly sell at home to get closer to 100% exposure, we estimate an elasticity of 0.63.

Another measure of the economic magnitude is the average dollar increase in a treated firm's debt holdings. To estimate this, column 9 of Table 3 models log debt rather than leverage. The estimated coefficient of 0.057 implies that the average treated firm increases its debt holdings by 5.7% following a tax rise (p=0.03). Given average pre-treatment debt of $570.2 million (see Table 2), this amounts to an additional $32.5 million in debt on average (=$570.2m*0.057).[25]

Prior studies of U.S. firms report no estimates to compare our elasticities to, but we note that they are similar to Faccio and Xu's (2013) who estimate a (symmetric) elasticity of 0.4 across OECD countries. Imposing symmetry, the estimated elasticity in our setting would be 0.33. Our estimated elasticities are thus similar to international estimates.

An elasticity of 0.53 for tax increases suggests that the average firm's marginal cost curve is fairly flat. This seems plausible, in light of the relatively low average levels of leverage in our sample. (In Section 5.7, we will show that firms with below-investment-grade credit ratings face steeper marginal cost curves and as a result do not increase leverage in response to a tax rise.) Our empirical design allows us to estimate the slope of the average firm's marginal cost curve. The average firm adds $32.5 million of debt in response to an increase in the marginal tax benefit averaging 131 basis points. The slope thus averages 1.31%/$32.5 million, i.e., marginal costs increase by $403 for every $1 million in new debt (=0.0131/32.5*$1m). Prior literature contains no estimates of the slope of the marginal cost curve but a comparison to prior estimates of marginal *adjustment* costs suggests we are in the right ballpark.[26]

[25] The fact that both leverage and debt increase by around 5.7% from their respective pre-treatment means implies that total assets remain approximately unchanged. In other words, firms respond to a tax rise by swapping debt for equity, leaving the size of their balance sheet largely unchanged. Tax increases thus trigger a pure capital structure change. This echoes the results of Asker, Farre-Mensa, and Ljungqvist (2013), who find that stock market-listed firms in the U.S. do not vary their investment spending in response to exogenous changes in state taxes.

[26] Altinkilic and Hansen (2000) report a slope of the marginal adjustment cost curve of $22 for every $1 million in new debt. Since the slope of the marginal cost curve equals the sum of the slopes of the marginal bankruptcy and adjustment cost curves, this implies that the marginal cost of *bankruptcy* increases by $381 (=$403–$22) for every $1 million in new debt. A large share coming from bankruptcy costs is consistent with Almeida and Philippon (2007).

We can also place an upper bound on the fixed adjustment costs involved in raising debt. Fixed costs cause firms to leave capital structure unchanged in response to small shocks, consistent with the patterns shown in column 3 of Table 3. They are not directly observable, but we can argue by revealed preference: how large would the fixed costs have to be to cause inaction?

Firms will only respond to a tax rise if the net benefit of more debt (the additional tax shield less the increase in bankruptcy and variable adjustment costs) exceeds fixed adjustment costs. In Figure 1c, the net benefit is the area of the triangle above the marginal cost curve MC_i, below the marginal benefit curve MB_j, and to the right of the vertical line through D_i. It amounts to \$212,875 (=\$32.5m *1.31%/2). The average firm's debt increase is therefore plausible as long as fixed adjustment costs do not exceed this number on average. Altinkilic and Hansen (2000) report fixed adjustment costs of \$227,000. This number comes from large bond issues involving syndicates of banks. The fixed costs of such bond issues are likely to be higher than average. Hence, the observed increase in debt in response to tax increases seems plausible given typical fixed costs of raising debt.

In sum, our findings seem economically plausible. The implied leverage-to-tax elasticity and slope of the marginal cost curve are consistent with the literature. Moreover, our tax shocks are large enough on average to overcome typical fixed adjustment costs of raising debt.

5.4 Reversals, Delayed Reactions, and Pre-trends

Table 4 includes additional lags to test for possible post-shock reversals (for tax increases) or delayed reactions (for tax cuts). As in our baseline regression in Table 3, we find that only the first lag of tax increases is statistically significant, with a point estimate in column 1 of 117 basis points (p<0.001). Over the next three years, leverage decreases a little, though this decrease is relatively modest (a cumulative 29 basis points) and not statistically significant (p=0.561). Four years after a tax increase, leverage is thus 88 basis points higher than before the tax increase relative to control firms (=117−29), and this is statistically significant (p=0.03). This indicates 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.)

The tax sensitivity in column 1 is somewhat greater, at 117 basis points, than in our baseline

model in Table 3. The reason is that including leads and lags results in the loss of early treatments (those occurring in a firm's first few panel years) and late treatments (those occurring at the end of the panel). To see if this might be driving our results, column 2 drops the lead and column 3 drops the fourth lag. This changes the point estimates a little but leaves our inferences unchanged.

As for tax cuts, the apparent asymmetry could reflect simple delays, perhaps caused by adjustment costs incurred in reducing leverage. Delays would imply that the tax-cut coefficient in our earlier regressions understates the full effect of tax cuts on leverage. This does not appear to be the case: the coefficients for the various lags of the tax cut indicator are tiny in all three columns, for cumulative changes of –21, +6, and –7 basis points (none of which is statistically significant). Thus, we find no evidence that firms react to tax cuts with any kind of reasonable lag.

Table 4 also investigates pre-trends. The coefficients in the year before a tax increase or tax cut in column 1 are fairly small (at –12 and +14 basis points, respectively) and not statistically significant. This means that pre-trends do not differ significantly between treated and control firms, confirming the graphical evidence in Figure 3. This finding has three implications. First, it 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, our estimates could be biased. Second, the absence of significant lead effects means that treated firms do not anticipate future tax changes. One reading 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 extra 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.

5.5 Robustness

Table 5 reports key robustness tests. First, to identify structural breaks and potentially influential outliers, columns 1 and 2 partition the sample by decade. This reveals a small (and insignificant) fall in the sensitivity of debt to tax rises over time, from 107 basis points in 1990-2000 (p=0.002) to

103 basis points after 2000 (p<0.001). Leverage is insensitive to tax cuts in both subsamples, so the tax sensitivity is asymmetric throughout our sample period. Because we obtain nearly identical results in both periods, our results cannot be driven by any single tax-change event.

Might our results be due to unobserved time-invariant differences between states? If firms choose where to locate based on unobserved state attributes that correlate with their debt policies, we should not compare, say, Michigan firms suffering a tax shock to control firms in, say, Utah. The solution is to add state fixed effects to our baseline specification. Column 3 shows that doing so yields point estimates that are nearly identical to the baseline estimates shown in Table 3.

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, we find attenuation in the estimated tax sensitivity: on average, firms raise total leverage by 83 basis points after a tax rise (p=0.001). Even this smaller effect is economically meaningful: relative to the mean pre-treatment ratio of 23.6% (Table 2), it represents an increase of 3.5%. Column 5 excludes short-term debt but includes the current portion of long-term debt (due within a year). This increases the tax sensitivity somewhat: on average, firms raise leverage by 112 basis points (p<0.001), compared to the baseline estimate of 104 basis points. As before, we see no sensitivity to tax cuts. Column 6 models long-term market leverage; the point estimate for tax increases is 66 basis points (p=0.059) and we continue to find no reaction to tax cuts.

Finally, columns 7 and 8 explore the extensive margin. Column 7 shows that firms are significantly more likely to issue long-term debt after a tax increase (p=0.001), whereas tax cuts have no effect on debt issuance decisions (p=0.594). Column 8 shows that tax cuts do not induce firms to repurchase long-term debt: following a tax cut, the probability of a repurchase is only one *basis* point higher than normal (p=0.907). These findings are consistent with the asymmetric effect of tax changes on leverage seen in our baseline specifications.

5.6 Causality

A causal interpretation of the observed sensitivity of leverage to tax increases (and of the failure to cut leverage in response to tax cuts) depends on the plausibility of the parallel-trends assumption.

Figure 3 and Table 4 confirm that treated and control firms start off on parallel trends. The question then is if they would have continued on parallel trends had it not been for the tax changes. Because our leverage regressions include industry-year effects, we know that our results are not driven by time-varying industry shocks to leverage trends. They condition on changes in firm-level correlates of leverage, such as changes in profitability, and so remove the influence of observed firm-level trend shifters. We can even remove unobserved firm-level trend shifters, by including firm fixed effects in our first-differenced specification, without affecting the results.

The two main remaining challenges to a causal interpretation are that the state tax changes occur because of some omitted factor which simultaneously drives state-level changes in taxes and firm-level changes in leverage; or that they coincide with some other state-level policy change that is in fact responsible for the leverage adjustment.

5.6.1 Potential Confound: Local Economic Conditions

States may change taxes because of changes in local economic conditions. To the extent that local shocks independently affect firms' debt policies (say, because firms borrow more when cash flows fall in recessions), part or all of the observed leverage increase among treated firms may reflect the effects of the local economic shock rather than the tax change. Fortunately, our setting allows us to disentangle the two.

Our specifications already control for observed state-level economic conditions, in the form of state growth and unemployment rates. Any remaining confound must thus be orthogonal to these state-level controls. To isolate such a potential confound, we first consider a simple falsification test. Its logic is as follows. Suppose tax changes are driven by unobserved changes in local conditions (orthogonal to our state-level controls) and it is these changes – rather than the tax changes – that firms in reality respond to. Then both firms in treated states *and* their neighbors in untreated states just across the state border will spuriously appear to "react" to the tax change, as long as economic conditions, unlike state tax law, have a tendency to spill across state borders. Our baseline tests would obscure this by including as controls firms from far-away states that are not subject to the local economic shock and so do not adjust their leverage.

To examine this possibility, column 1 in Table 6 includes both home-state tax-change indicators and indicators for tax changes that occur in a *neighboring* state. As before, firms borrow more following home-state tax rises and fail to cut leverage following home-state tax cuts. Importantly, firms located in a state that does not change its own tax rate but borders one that does do *not* mirror this behavior. This supports a causal interpretation of the home-state tax treatment effect.

But there is more. When a neighboring state raises taxes, control firms actually *reduce* their leverage significantly, by 42 basis points on average (p=0.005), relative to their industry peers in far-away states. Assuming firms in neighboring states share similar economic conditions, this behavior is hard to reconcile with the conjectured confound: it goes in the wrong direction.

To investigate this further, columns 2 and 3 drop all far-away controls, restricting the set of controls to firms located in a state neighboring either a tax-rise state (column 2) or a tax-cut state (column 3). Narrowing the sample of control firms to those sharing arguably similar local economic conditions increases the economic magnitude of the sensitivity of leverage to tax rises by 45 basis points from the baseline, to 149 basis points in column 2 (p<0.001). (We continue to find that firms do not respond to tax cuts: the point estimate in column 3 measures only –6 basis points (p=0.762).)

Of course, firms in adjacent states may not necessarily share the same economic conditions; for example, they may be located at opposite ends of two large states. We can construct a cleaner test by focusing on border regions, i.e., on firms headquartered in contiguous *counties* on either side of a state border. Such county pairs share plausibly similar (unobserved) local economic conditions while being subject to different tax treatments. The identifying assumption of this test is that firms in a narrow interval around the state border (here, those located in contiguous counties) are only randomly different from each other and so would experience parallel trends absent the tax shock.

We identify a firm's county based on its zip code, using a bridge obtained from the Centers for Disease Control and Prevention.[27,28] Our sample contains 401 county-pair/year clusters involving

[27] 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.

firms in contiguous 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 contiguous county do not. The total number of treated and control firms is 2,278 and because the same firm can be hit with multiple tax shocks over time, there are 12,822 firm-years. Of these, 868 involve tax rises in 19 states and 2,195 involve tax cuts in 20 states. Thus, there is a large number of firms per county-pair/year cluster and there is substantial variation in treatment status within each cluster involving a large number of separate tax shocks.

Column 4 includes two sets of fixed effects: a set of county-pair/year fixed effects, to remove unobserved variation in economic conditions affecting firms operating in a pair of contiguous counties, and the set of industry-year fixed effects we used previously to remove unobserved variation in industry conditions that may affect leverage. We find that relative to control firms just the other side of the state border, treated firms increase their (industry-adjusted) leverage by an average of 131 basis points when their home state raises corporate tax rates (p=0.05).

As in column 2, which uses as controls firms from *anywhere* in the neighboring state, the point estimate in the contiguous-border-counties test in column 4 is larger than in our baseline model. But this is not quite a fair comparison: unlike all our previous specifications, column 4 does not estimate the treatment effect within-industry; instead, it includes two *independent* sets of fixed effects, thereby comparing the change in industry-adjusted leverage of, say, a treated food retailer to the contemporaneous change in industry-adjusted leverage of an untreated shoe manufacturer (requiring only that both be located in the same county-pair).

To take out industry variation in leverage, column 5 requires controls not only to be located in a contiguous 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 1,520 firm-years in 479 county-pair/industry/year triplets. And yet the point estimate is virtually

[28] We hand-collect historical zip codes from SEC filings for the 1,541 firms that our data checks indicate moved across state lines over our sample period. For the remaining 8,571 sample firms, we use Compustat's current zip codes. This will introduce noise to the extent that these firms moved counties within a state during our sample period. Given the large number of firm-years involved (82,219), hand-collecting historic zip codes for these firms is impracticable. However, our coefficients are quite precisely estimated, so noise does not appear to be a major concern.

unchanged: relative to firms in the same industry located just the other side of a state border, treated firms increase their leverage by 128 basis points on average following a tax rise (p=0.004).

Why is the estimated tax sensitivity roughly 30% greater than in our baseline specification, which does not condition on the location of control firms? Column 1 suggests that the increase is driven by a change in the behavior of the control firms: when taxes rise across the border, controls reduce their leverage (relative to their industry peers located elsewhere in the U.S.). This finding points to a local component of firms' debt policies. A plausible interpretation is that treated firms' leverage responses to tax rises "spill across borders," in the sense that their neighbors react (by cutting leverage) to the fact that leverage has increased among firms in their industry on the other side of the state border. Such behavior is consistent with product-market competition affecting debt policy and vice versa, as emphasized by Brander and Lewis (1985).

5.6.2 Potential Confounds: Union Power, Unemployment Risk, and Other Tax Changes

The tests reported in Table 6 can difference away any variable that satisfies two conditions: it correlates with tax changes and so could confound our tests; and its influence does not stop at the state border. Local economic shocks are one concrete example of a confound these tests can rule out, but the inference is more general: the fact that we continue to find a positive sensitivity to tax increases in Table 6 implies that our results are not driven by *any* source of unobserved variation that coincides with the tax increases and diffuses across state borders.

This leaves confounds whose influence stops at the state border and which therefore coincide exactly with the tax variation. Since the treatment varies within state across time, we cannot include state-year fixed effects to remove such confounds directly. This is a general feature of diff-in-diff tests using policy variation. The only way to address confounds of this kind is to be explicit about what they are and investigate them one by one.

In this section, we consider three potential confounds suggested by prior literature, starting with labor market conditions. Matsa (2010) finds that leverage increases in union power, 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 debt policy, what looks like a tax-induced

change in leverage may in fact be driven by unobserved variation in union power simultaneously causing tax rises and leverage increases.

To test this, we exploit variation in unionization rates across states and time. Columns 1 and 2 of Table 7 partition the sample into firms headquartered in states with either high or low union power. (See Appendix C for details.) Both sets of firms increase leverage significantly when taxes rise. Interestingly, the increase is considerably larger among firms located in *low*-union states, at 147 versus 83 basis points. This, together with the results in Table 1 showing that tax-increasing states are no more unionized than other states, casts doubt on the idea that firms borrow more not because of a tax rise but to counter union power.

Titman (1984) argues that firms choose their debt to insure workers against unemployment risk. To confound our results, unemployment risk would have to fall at the same time as states raise corporate taxes. In columns 3 and 4 of Table 7, we use state-level data on mass layoffs to measure unemployment risk, partitioning the sample into firms headquartered in states that suffer either unusually large or unusually small employment shocks at the time of a tax rise. (See Appendix C for definitions.) We observe a positive and significant tax sensitivity of nearly identical magnitude in both groups, averaging 102 and 104 basis points, respectively. Thus, firms increase their leverage in response to tax rises regardless of whether their state has suffered a large employment shock.

Finally, Table 1 shows a tendency for state-level corporate income tax changes to coincide with state-level changes in personal income or capital gains taxes. In columns 5 and 6 of Table 7, we omit as shocks any state corporate income tax change that coincides with a change in either state personal income taxes (column 5) or state capital gains taxes (column 6). We find that firms react to these corporate-only tax shocks just as strongly as to the full set of tax shocks used in Table 3, and the tax sensitivity of leverage remains asymmetric. This implies that leverage is sensitive to changes in corporate taxes but not to changes in personal taxes on income or capital gains.

We obtain similar results (not reported) if we remove the small number of corporate tax changes that coincide with changes in state credits for investment, R&D, or job creation (as listed in Table 1, Panel B), if we remove instances where a state changes both corporate and bank taxes, and if we

control for the political conditions in the state at the time of the tax change (see Table 1, Panel A).

5.7 Heterogeneous Treatment Effects

Interest tax shields depend on the interplay between personal taxes on income from interest (τ_i) and equity (τ_e) on the one hand and corporate taxes on profits (τ_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 $dD^*/d\tau_c = (1-\tau_e)/2c$. Thus, higher personal taxes on equity income dampen the impact of a corporate tax change on debt. Because τ_e likely varies in the cross-section, treatment effects should be heterogeneous.

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

This discussion suggests a useful validation test. If the observed tax sensitivity of debt is causal, we expect stronger effects among firms with small τ_e. To test this comparative static, Table 8 uses dividends to proxy for τ_e. Non-dividend payers have lower τ_e than dividend-payers because their investors derive their equity income solely in the form of (lower-taxed) capital gains. When we split the samples accordingly, we find results consistent with heterogeneous treatment effects. While non-dividend payers increase leverage by 143 basis points after a tax rise (p<0.001), dividend payers increase leverage by only 4 basis points (p=0.876); the difference between these point estimates is statistically significant (p=0.001).

A corollary of a causal interpretation is that the tax sensitivity of debt should vary with profits, as interest-bearing debt offers valuable tax shields only to profitable firms. Columns 3 and 4 partition sample firms according to whether or not they are profitable in the year of the tax rise. Consistent with firms borrowing to take advantage of tax shields, we find that only profitable firms borrow more: when faced with higher taxes in their home state, profitable firms increase leverage

by 109 basis points (p<0.001), nearly four times more than the estimated diff-in-diff increase of 29 basis points for loss-making firms (p=0.618); the difference is marginally significant (p=0.106).[29]

Another way to measure the potential tax benefit of increasing debt is to condition on firms' marginal tax rates. All else equal, firms with higher marginal tax rates have a greater incentive to take on more debt in response to a tax rise. To capture this, we use simulated firm-level marginal tax rates obtained from John Graham. (See Appendix C for details.) Preliminary inspection suggests that the relation is highly nonlinear. As a result, we use Graham's marginal tax rates to partition the sample as follows. Column 5 of Table 8 focuses on the 6,570 firm-years with a zero marginal tax rate. These firms experience a meaningful number of tax shocks (239 tax rises and 727 tax cuts) but do not respond to them significantly. Column 6 focuses on firms with positive marginal tax rates. Such firms are quite sensitive to tax rises, increasing their leverage by 107 basis points on average (p<0.001). Column 7 focuses on firms in the top decile of marginal tax rates (those paying at least 35% of their earnings in tax). The estimated sensitivity of firms with such high marginal tax rates is more than twice the average, at 252 basis points (p=0.021).

Trade-off theory suggests that the extent to which a firm *can* increase its leverage in response to a tax rise depends on its debt capacity and its likely costs of distress (as captured by c above). Effectively, its default risk acts as a constraint on its ability to take advantage of further tax shields of debt. To test this prediction, we partition firms into those rated investment-grade (column 8) and those rated junk by S&P, Moody's, or Fitch (column 9). Firms without a credit rating are omitted. We find that investment-grade firms increase their leverage by 89 basis points following a tax rise (p=0.041), whereas riskier borrowers do not increase their leverage at all (p=0.751).

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

6. Conclusions

The U.S. tax system subsidizes firms' use of debt: interest payments are tax deductible while dividends are not. Despite decades of scholarship, it is an open question whether taxes are a first-

[29] 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=0.024), which may be a better predictor of future profitability.

order determinant of capital structure. We overcome the identification challenges that have hampered previous work by using a natural experiment in the form of 121 staggered changes in corporate income tax rates across U.S. states. Our results show that firms react strongly to tax increases (implying relatively flat marginal cost curves and small adjustment costs on the margin) but are insensitive to tax cuts. These findings are robust to various potential confounds. We also find evidence of geographic clustering in corporate debt policies.

The asymmetry in tax sensitivity we observe in the data runs counter to static 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. This in turn is consistent with firms facing higher adjustment costs when seeking to cut leverage, an assumption commonly made in dynamic capital structure models.

We end with an important caveat about the external validity of our results. Our estimates of firms' leverage responses to tax changes are based on a relatively narrow range of variation in tax rates (up or down by no more than a few percentage points). As a result, we cannot predict how firms would react to tax changes that are much larger than those seen over our sample period. In particular, whether firms would respond to the drastic cuts in federal corporate income tax rates that some policy makers are currently debating is an open question.

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Appendix A. List of State Corporate Income Tax Increases.

This table lists all state corporate income tax increases over the tax years 1989-2012. 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), the *Book of the States*, a search of the "Current Corporate Income Tax Developments" feature published periodically in the *Journal of State Taxation*, and state codes accessed through Lexis-Nexis.

State	Year	Description	No. of affected sample firms
IL	1989	Increase in top corporate income tax rate from 4% to 4.8%	163
KY	1989	Increase in top corporate income tax rate from 7.25% to 8%	9
NJ	1989	Introduction of 0.375% tax surcharge	232
RI	1989	Increase in top corporate income tax rate from 8% to 9%	12
CT	1990	Introduction of 20% tax surcharge, increasing top marginal tax rate from 11.5% to 13.8%	106
MO	1990	Increase in top corporate income tax rate from 5% to 6.5%	54
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%	171
RI	1991	Introduction of 11% tax surcharge on tax liability	12
DC	1992	Introduction of 2.5% surcharge on tax liability	7
KS	1992	Increase in top corporate income tax rate (including surcharge) from 6.75% to 7.35%	21
KY	1992	Increase in top corporate income tax rate from 8% to 8.25%	14
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% and reduction in federal income tax deductibility from 100% to 50%	71
MT	1993	Increase in tax surcharge on tax liability from 2.3% to 4.7%	4
DC	1994	Introduction of additional 2.5% surcharge on tax liability	0
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%	21
AL	2001	Increase in top corporate income tax rate from 5% to 6.5%	21
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	140
KS	2002	Increase in tax surcharge on taxable income from 3.35% to 4.5%	19
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	173
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	86
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%	86
NJ	2006	Introduction of 4% tax surcharge on tax liability	153
MD	2008	Increase in top corporate income tax rate from 7% to 8.25%	59
MI	2008	Introduction of corporate income tax with a top rate of 4.95%; replaces a gross-receipts tax without interest deductibility	53
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	59
OR	2009	Increase in top corporate income tax rate from 6.6% to 7.9%	23
IL	2011	Increase in top corporate income tax rate from 4.8% to 7%	111
CT	2012	Unscheduled two-year extension of tax surcharge on tax liability and increase to 20%	1
MI	2012	Increase in top corporate income tax rate from 4.95% to 6%	5

Appendix B. List of State Corporate Income Tax Cuts.

This table lists all state corporate income tax cuts over the tax years 1989-2012. 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), the *Book of the States*, a search of the "Current Corporate Income Tax Developments" feature published periodically in the *Journal of State Taxation*, and state codes accessed through Lexis-Nexis.

State	Year	Description	No. of affected sample firms
CO	1988	Reduction in top corporate income tax rate from 6% to 5.5%	121
WV	1988	Reduction in top corporate income tax rate from 9.75% to 9.6%	8
CO	1989	Reduction in top corporate income tax rate from 5.5% to 5.4%	121
WV	1989	Reduction in top corporate income tax rate from 9.6% to 9.45%	7
AZ	1990	Reduction in top corporate income tax rate from 10.5% to 9.3%	44
CO	1990	Reduction in top corporate income tax rate from 5.4% to 5.3%	108
WV	1990	Reduction in top corporate income tax rate from 9.45% to 9.3%	6
CO	1991	Reduction in top corporate income tax rate from 5.3% to 5.2%	116
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
CO	1992	Reduction in top corporate income tax rate from 5.2% to 5.1%	124
CT	1992	Reduction in tax surcharge from 20% to 10%	112
MO	1992	Reduction in top corporate income tax rate from 6.5% to 5%	61
NC	1992	Reduction in tax surcharge from 4% to 3%	68
WV	1992	Reduction in top corporate income tax rate from 9.15% to 9%	6
CO	1993	Reduction in top corporate income tax rate from 5.1% to 5.0%	138
CT	1993	Repeal of 10% tax surcharge	118
ME	1993	Repeal of 10% tax surcharge	5
NC	1993	Reduction in tax surcharge from 3% to 2%	74
NE	1993	Repeal of 15% tax surcharge	12
NH	1993	Reduction in top corporate income tax rate from 8% to 7.5%	20
AZ	1994	Reduction in top corporate income tax rate from 9.3% to 9%	56
MT	1994	Repeal of 4.7% tax surcharge	3
NC	1994	Reduction in tax surcharge from 2% to 1%	77
NH	1994	Reduction in top corporate income tax rate from 7.5% to 7%	23
NJ	1994	Repeal of 0.375% tax surcharge	220
PA	1994	Reduction in top corporate income tax rate from 12.25% to 11.99%	200
RI	1994	Repeal of 11% tax surcharge	20
CT	1995	Reduction in top corporate income tax rate from 11.5% to 11.25%	124
DC	1995	Reduction in top corporate income tax rate from 10% to 9.5% (+2 tax surcharges at 2.5% each)	9
NC	1995	Repeal of 1% tax surcharge	76
PA	1995	Reduction in top corporate income tax rate from 11.99% to 9.99%	208
CT	1996	Reduction in top corporate income tax rate from 11.25% to 10.75%	134
CA	1997	Reduction in top corporate income tax rate from 9.3% to 8.84%	939
CT	1997	Reduction in top corporate income tax rate from 10.75% to 10.5%	137
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 9.5%	122
NC	1998	Reduction in top corporate income tax rate from 7.5% to 7.25%	83
CO	1999	Reduction in top corporate income tax rate from 5% to 4.75%	133
CT	1999	Reduction in top corporate income tax rate from 9.5% to 8.5%	110
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%	383
OH	1999	Reduction in top corporate income tax rate from 8.9% to 8.5%	146

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%	119
CT	2000	Reduction in top corporate income tax rate from 8.5% to 7.5%	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%	330
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%	294
KS	2003	Reduction in tax surcharge from 4.5% to 3.35%	20
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
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%	252
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 3.1%	17
KY	2008	Reduction in top corporate income tax rate from 7% to 6%	17
TX	2008	Abolition of income tax, replaced with gross receipts tax without interest deductibility	300
KS	2009	Reduction in tax surcharge from 3.1% 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%	11
MA	2011	Reduction in top corporate income tax rate from 8.75% to 8.25%	132
NC	2011	Repeal of 3% tax surcharge	47
ND	2011	Reduction in top corporate income tax rate from 6.4% to 5.4%	1
OR	2011	Reduction in top corporate income tax rate from 7.9% to 7.6%	18

Appendix C. Variable Definitions.

State-level variables (Table 1)

Democratic governor is an indicator set equal to one if the state is governed by a Democratic governor, and zero otherwise. Data come from the Congressional Quarterly (through 2008) and state election websites (after 2008).

State budget balance equals the difference between a state's *general revenues* and its *general expenditures* scaled by its *general expenditures*. The data come from the U.S. Census Bureau's State & Local Finances database, available at http://www.census.gov/govs/local.

State budget deficit equals *state budget balance* if the state runs a budget deficit, and zero otherwise.

State budget surplus equals *state budget balance* if the state runs a budget surplus, and zero otherwise.

State bond rating downgrade is an indicator set equal to one if the state's credit rating is downgraded by either S&P or Moody's.

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.

State union penetration 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, http://www.unionstats.com.

Tax competition is measured as the difference between a state's corporate income tax rate and the highest corporate income tax rate levied by any of the neighboring states.

State personal income taxes 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 capital gains tax rates 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.

State tax on banks captures changes in the rate at which a state taxes financial institutions with nexus to the state. (Both a physical presence in the state and out-of-state lending to borrowers located in the state constitute nexus.) The data come from the *Book of the States* and state codes accessed through Lexis-Nexis.

State investment tax credit rate is the rate at which a firm can deduct capital expenditures directly from its state corporate income tax liability (in addition to the usual depreciation deductions against taxable income). Data through 2006 come from Chirinko and Wilson (2008). Data for subsequent years come from tax forms available on state Department of Revenue websites.

State R&D credit rate is the percentage of a firm's R&D expenditures that it can deduct directly from its state corporate income tax liability (in addition to the usual deduction against taxable income). Data through 2006 come from Wilson (2007). Data for subsequent years come from tax forms available on state Department of Revenue websites.

State job creation credit is set equal to one if the state offers a tax credit in return for hiring new workers meeting certain requirements, and zero otherwise. The data come from Appendix A1 in Neumark and Grijalva (2013).

State job creation grants is set equal to one if the state offers grant payments in return for hiring new workers meeting certain requirements, and zero otherwise. The data come from Appendix A1 in Neumark and Grijalva (2013).

Firm-level dependent variables (Tables 2-9)

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

Log real 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.

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*).

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*).

Long-term 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*).

Prob(issue long-term debt) is an indicator set equal to one if the firm issues long-term debt (i.e., if the within-firm year-on-year change in Compustat item *dltt* is positive), and zero otherwise.

Prob(repurchase debt) is an indicator set equal to one if the firm repurchases long-term debt (i.e., if the within-firm year-on-year change in Compustat item *dltt* is negative), and zero otherwise.

Independent variables: Firm-level characteristics (Tables 2-9)

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

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

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

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

Independent variables: Credit market conditions (Tables 2-9)

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.

Independent variables: State-level characteristics (Tables 2-9)

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.

Conditioning variables (Tables 3 and 7-8)

Low 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 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 are below the 33rd percentile, and zero otherwise.

States with high (low) union power is an indicator set equal to one if the firm is headquartered in a state that ranks in the top (bottom) 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, http://www.unionstats.com.

States with high (low) mass layoffs is an indicator set equal to one if the firm is headquartered in a state that ranks in the top (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. 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.

Non-dividend payers are firms with zero dividends on common stock in year *t* (Compustat item *dvc*).

Dividend payers are firms with non-zero dividends on common stock in year *t* (Compustat item *dvc*).

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

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

Marginal tax rates (MTR) come from John Graham (http://faculty.fuqua.duke.edu/~jgraham/taxform.html); see Graham (1996a, 1996b). Following Graham, Lemmon, and Schallheim (1998), we use after-interest marginal tax rates (variable *mtrafter*). Missing values are filled in as recommended in Graham and Mills (2008).

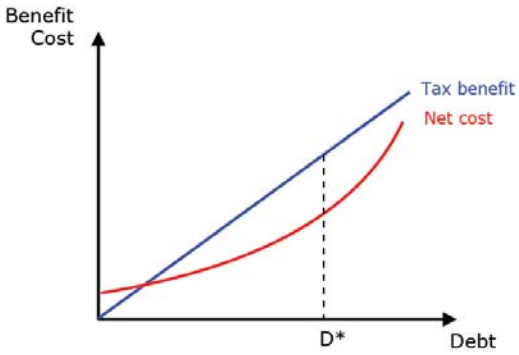
Investment grade is an indicator set equal to 1 if in year *t*, the firm has an investment-grade rating from S&P, Moody's, or Fitch, using data obtained from Compustat (variable *splticrm*) and Mergent FISD, and zero otherwise. It is missing for firms without a credit rating.

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 static 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 (τ_c), the personal tax rate on income from debt (τ_i), and the personal tax rate on income from equity (τ_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

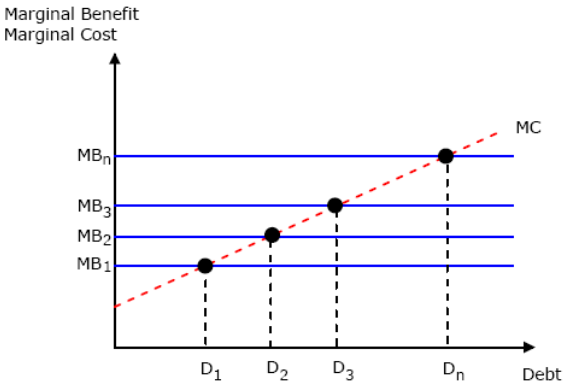


Figure 1c: Identifying assumption for observational data

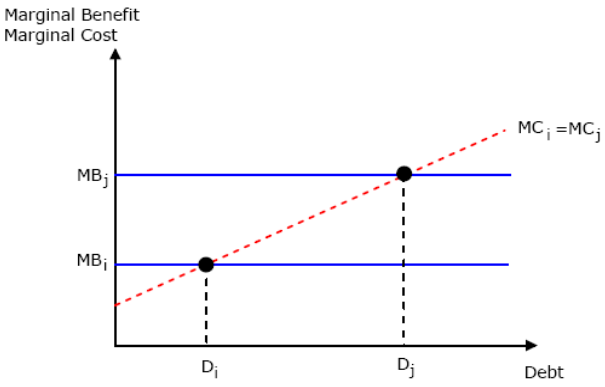


Figure 1d: Identification challenge

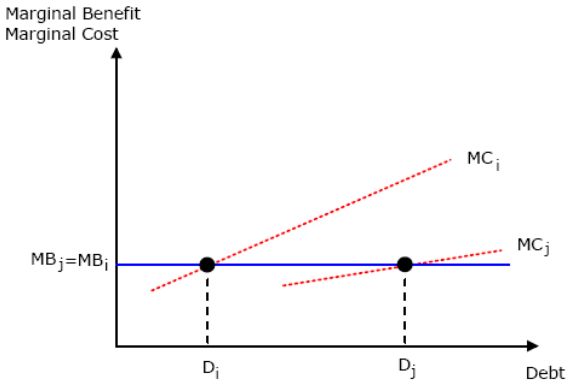


Figure 2. Geography of State Corporate Income Tax Changes, 1989-2012.

Tax increases, 1989-1993

Tax cuts, 1989-1993

Tax increases, 1994-1998

Tax cuts, 1994-1998

Tax increases, 1999-2003

Tax cuts, 1999-2003

Tax increases, 2004-2008

Tax cuts, 2004-2008

Tax increases, 2009-2012

Tax cuts, 2009-2012

Figure 3. 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 net of the contemporaneous leverage change in the firm's SIC4 industry (to remove the influence of time-varying changes in industry conditions or nation-wide variation in business conditions that affect all industries simultaneously). Leverage changes are plotted 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 (using standard errors clustered at the state level) 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.

Figure 3a. Tax Increases

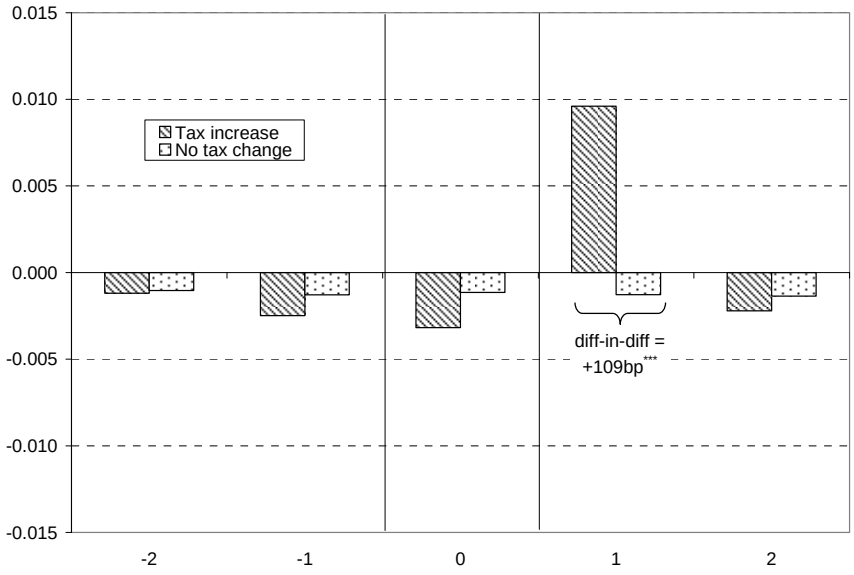


Figure 3b. Tax Cuts

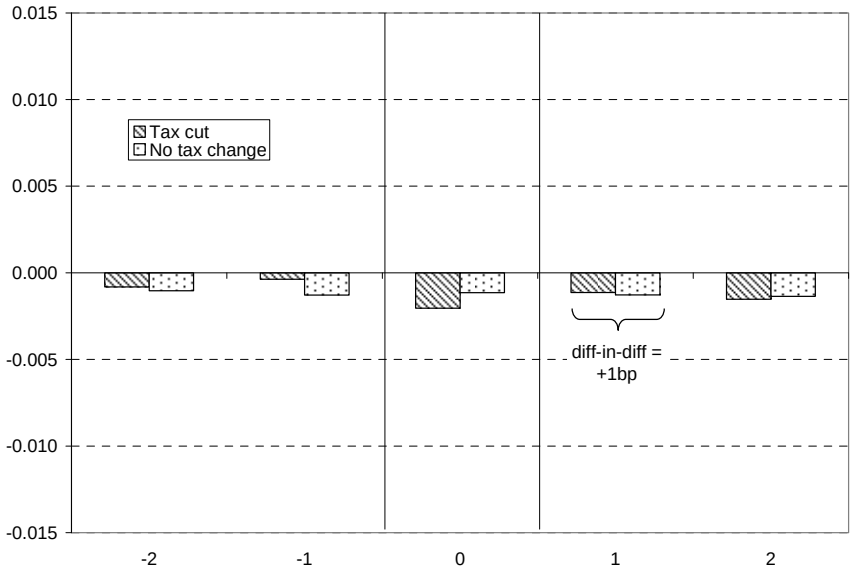


Figure 4. Asymmetric Tax Sensitivity, Leverage Hysteresis, and the Cost of Debt.

Figure 4a illustrates the implications of asymmetry in tax sensitivity for the static trade-off theory of capital structure shown in Figure 1a. The total net cost of debt is upward sloping and convex above the optimal level of debt D* but flat below it, exhibiting a kink at the firm's optimal level of debt. Figure 4b illustrates the implications of hysteresis based on the treatment-reversal estimates in Table 3, column 8. Before a 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 4a: Asymmetry

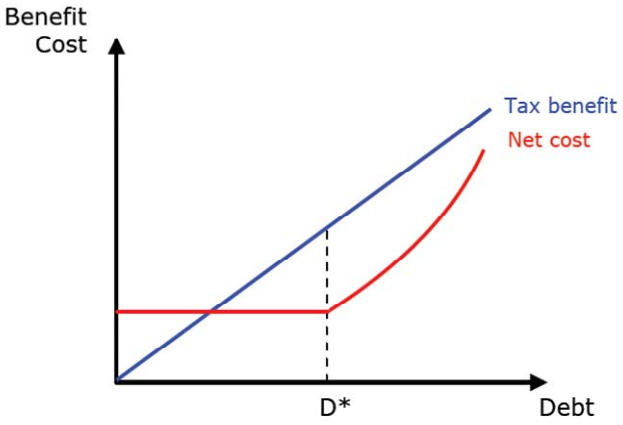


Figure 4b: Hysteresis

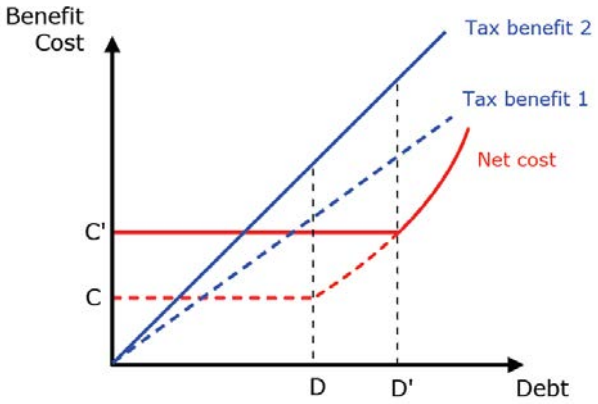


Table 1, Panel A. Determinants of State Corporate Income Tax Changes, 1990-2011.

The table models the determinants of the probability that a state changes its top marginal corporate income tax rate and the magnitude of any such change. Columns 1 to 3 report summary statistics of the explanatory variables, showing fractions or means (with standard deviations shown in italics underneath the means). Column 4 compares conditions in states that increase taxes to those in states that cut taxes. Columns 5 and 6 model the probability that a state raises or cuts corporate income taxes, using linear probability models. Columns 7 to 9 model the magnitude of the tax changes (measured in percentage points). For variable definitions and details of their construction, see Appendix C. All specifications are estimated using least squares with state and year fixed effects (not shown for brevity). The sample covers 50 states plus DC in 1990-2011, for a total of 1,122 observations. In columns 5-9, 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.

	Summary statistics			Difference	Probability of …		Magnitude of …		
	all obs.	tax increases	tax cuts	(tax inc. – tax cut)	tax increase	tax cut	tax change	tax increase	tax cut
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Political conditions									
=1 if Democratic governor in year *t*-1	0.472	0.622	0.392	0.230**	0.007	-0.037**	0.067**	0.034	-0.033**
					0.014	*0.017*	*0.026*	*0.021*	*0.014*
=1 if 1 year to next gubernatorial election					0.013	0.060**	0.024	0.028	0.005
					0.018	*0.030*	*0.036*	*0.017*	*0.032*
=1 if 2 years to next gubernatorial election					-0.003	0.026	0.029	0.027	-0.005
					0.016	*0.025*	*0.040*	*0.021*	*0.035*
=1 if 3 years to next gubernatorial election					0.053**	0.044	0.040	0.044**	0.006
					0.024	*0.029*	*0.035*	*0.020*	*0.028*
Economic conditions (in year *t*-1)									
state budget balance	0.020	0.008	0.034	-0.027**			-0.408**		
	0.069	*0.061*	*0.062*				*0.190*		
state budget deficit	-0.014	-0.020			-0.665**			-1.023**	
	0.026	*0.033*			*0.339*			*0.461*	
state budget surplus	0.034		0.043			0.296*			0.093
	0.056		*0.051*			*0.161*			*0.119*
=1 if state bond rating downgraded	0.044	0.135	0.000	0.135***	0.058	-0.059**	0.176	0.129	-0.042*
					0.044	*0.026*	*0.114*	*0.112*	*0.022*
GSP growth rate	0.027	0.018	0.030	-0.012**	0.248	0.336	-0.052	0.333	0.317
	0.028	*0.023*	*0.027*		*0.231*	*0.336*	*0.473*	*0.299*	*0.353*
state unemployment rate	0.055	0.053	0.056	-0.002	-0.251	1.000	-1.082	-0.249	0.752
	0.018	*0.017*	*0.019*		*0.601*	*1.106*	*1.080*	*0.849*	*0.568*
state union penetration	0.085	0.092	0.083	0.009	-0.368	-0.453	-0.349	-0.715	-0.229
	0.043	*0.032*	*0.044*		*0.473*	*0.703*	*0.879*	*0.741*	*0.489*
Tax competition (in year *t*-1)									
state's tax rate relative to highest tax rate	-0.019	-0.020	-0.005	-0.015***	-0.021***	0.036***	-0.059***	-0.023**	0.036***
among its neighboring states	*0.037*	*0.028*	*0.020*		*0.007*	*0.010*	*0.016*	*0.012*	*0.010*
Diagnostics									
R^2					12.4%	21.7%	10.7%	11.0%	11.9%

Table 1, Panel B: Coincident State-level Changes.

This table reports state-level changes in economic quantities that coincide with either increases or cuts in state corporate income taxes and that have a plausible basis in theory to potentially affect corporate leverage decisions. We focus on changes in state personal income tax rates, state capital gains tax rates, or state taxes on banks and changes in state investment incentive programs (i.e., tax credits for investment, R&D, and job creation, as well as job creation grant programs). For variable definitions and details of their construction, see Appendix C.

		Tax increases	Tax cuts
Number of tax changes		43	78
… of which coincide with	increase in state personal income tax rate	22	13
	cut in state personal income tax rate	2	15
	increase in state capital gains tax rate	21	12
	cut in state capital gains tax rate	2	16
	increase in state tax on banks	28	0
	cut in state tax on banks	0	56
	increase in state investment tax credit rate	1	6
	cut in state investment tax credit rate	0	0
	increase in state R&D credit rate	2	9
	cut in state R&D credit rate	1	2
	increase in state job creation credit	0	3
	cut in state job creation credit	0	1
	increase in state job creation grants	0	1
	cut in state job creation grants	0	0

Table 2. Firm-level Summary Statistics.

The sample consists of 91,487 firm-years for all non-financial and non-utility U.S. companies that are traded on the NYSE, Amex, or Nasdaq in fiscal years 1989 through 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 = 91,487)						One year before a tax increase (N = 1,735)		One year before a tax cut (N = 6,627)	
			percentile			first				
	mean	s.d.	25th	50th	75th	diff.	mean	s.d.	mean	s.d.
Firm leverage										
long-term book leverage	0.172	0.264	0.002	0.100	0.275	0.003	0.183	0.217	0.172	0.211
long-term book lev. (incl. current portion)	0.198	0.295	0.006	0.133	0.310	0.006	0.208	0.228	0.194	0.222
total book leverage	0.226	0.311	0.019	0.174	0.348	0.009	0.236	0.237	0.221	0.231
long-term market leverage	0.176	0.222	0.001	0.079	0.281	0.008	0.199	0.230	0.173	0.219
Real long-term debt ($m)	385.0	2,502.9	0.1	6.9	117.1	27.5	570.2	3,262.1	368.5	1,848.7
Firm characteristics										
ROA	0.034	0.273	0.009	0.104	0.166	-0.011	0.046	0.254	0.046	0.253
firm size (total assets, $m)	1,683.0	9,546.0	34.2	134.9	628.1	121.3	2,297.0	10,308.2	1,652.6	9,258.0
Tangibility	0.264	0.224	0.087	0.196	0.379	0.001	0.260	0.205	0.260	0.219
market/book	1.840	1.939	0.813	1.210	2.054	-0.076	1.796	1.977	1.856	1.982
Credit market conditions										
default spread (in %)	0.956	0.465	0.680	0.870	1.080	0.013	1.235	0.576	0.855	0.269
State characteristics										
GSP growth rate	0.029	0.026	0.012	0.029	0.046	-0.001	0.013	0.019	0.035	0.025
state unemployment rate	0.058	0.017	0.046	0.054	0.066	0.001	0.054	0.018	0.057	0.016

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. Except in columns 4 and 5, we capture tax changes using indicator variables for tax increases and tax cuts. In columns 4 and 5, we use changes in a state's top marginal corporate income tax rate. Note that two corporate income tax increases (CA 2002 and NJ 2002) and one tax cut (TX 2008) cannot be summarized in terms of changes in marginal tax rates; see Appendix A and B. In column 3, large, medium, and small tax changes are those in the top, middle, and bottom tercile. Column 5 restricts sample to firms those with low inter-state sales, based on whether sales in their three-digit NAICS industry are predominantly inter-state or intra-state. Column 6 restricts the tax-cut treatment to the first in a pre-announced sequence of annual tax cuts. (For example, NY cut the top corporate income tax rate from 9% to 7.5% in three annual half-percentage-point increments starting in 1999.) Column 8 restricts the sample of treated firms to those that suffer first a tax increase and then a subsequent tax cut ("reversals"). Column 9 models log real debt rather than a leverage ratio. The unit of analysis in each column is a firm-year. All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-year fixed effects to remove industry shocks. The specifications shown in columns 7 and 8 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 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.

	Dep. var.: Change in long-term book leverage								*Dep. var.:*
	Baseline (1)	Baseline w/ backfilled Compustat HQ data (2)	Large vs. small tax changes (3)	Baseline w/ changes in marginal rates (4)	Baseline, firms w/ low inter-state sales (5)	Baseline w/o late-in-sequence tax cuts (6)	Baseline w/ firm FE (7)	Reversals w/ firm FE (8)	Change in log real long-term debt (9)
Tax increase indicators (in %, exc. col. 8)									
=1 if tax rise at $t = -1$	1.042***	0.746***				1.039***	1.161***	1.064***	0.057**
	0.196	*0.204*				*0.194*	*0.215*	*0.307*	*0.025*
=1 if large tax rise at $t = -1$			1.310***						
			0.406						
=1 if medium tax rise at $t = -1$			0.907*						
			0.522						
=1 if small tax rise at $t = -1$			0.705						
			0.525						
Tax cut indicators (in %, except col. 8)									
=1 if tax cut at $t = -1$	-0.010	-0.045				-0.153	-0.097	-0.071	-0.003
	0.136	*0.167*				*0.131*	*0.200*	*0.487*	*0.867*
=1 if large tax cut at $t = -1$			0.009						
			0.239						
=1 if medium tax cut at $t = -1$			0.160						
			0.347						
=1 if small tax cut at $t = -1$			-0.112						
			0.246						

Table 3. Continued.

	Dep. var.: Change in long-term book leverage								*Dep. var.:*
	Baseline (1)	Baseline w/ backfilled Compustat HQ data (2)	Large vs. small tax changes (3)	Baseline w/ changes in marginal rates (4)	Firms w/ low inter-state sales (5)	Baseline w/o late-in-sequence tax cuts (6)	Baseline w/ firm FE (7)	Reversals w/ firm FE (8)	Change in log real long-term debt (9)
Continued:									
Continuous tax changes									
size of corporate tax rise at $t = -1$				0.387**	0.488**				
				0.168	*0.237*				
size of corporate tax cut at $t = -1$				0.048	0.094				
				0.128	*0.246*				
Lagged change in …									
ROA	-0.005	-0.005	-0.005	-0.004	0.004	-0.005	-0.001	0.001	-0.015
	0.004	*0.004*	*0.004*	*0.004*	*0.006*	*0.004*	*0.004*	*0.006*	*0.021*
firm size	0.007***	0.007***	0.007***	0.007***	0.008***	0.007***	0.003	0.002	0.149***
	0.002	*0.002*	*0.002*	*0.002*	*0.003*	*0.002*	*0.002*	*0.004*	*0.014*
tangibility	0.037***	0.037***	0.037***	0.037***	0.027	0.037***	0.026**	0.025	0.389***
	0.011	*0.011*	*0.009*	*0.011*	*0.028*	*0.011*	*0.011*	*0.018*	*0.071*
market/book	0.000	0.000	0.000	0.000	0.000	0.000	-0.001	-0.001	0.012***
	0.000	*0.000*	*0.000*	*0.000*	*0.001*	*0.000*	*0.000*	*0.001*	*0.003*
default spread	-0.512***	-0.512***	-0.513***	-0.536***	-0.582	-0.514***	-0.507***	-0.503**	-0.057***
	0.123	*0.124*	*0.168*	*0.127*	*0.355*	*0.123*	*0.123*	*0.216*	*0.012*
GSP growth rate	0.025	0.025	0.025	0.025	0.036	0.024	0.020	-0.010	0.189
	0.019	*0.019*	*0.023*	*0.019*	*0.046*	*0.019*	*0.020*	*0.042*	*0.190*
state unemployment rate	0.171**	0.176**	0.173*	0.168*	-0.342	0.162*	0.087	0.039	1.308**
	0.084	*0.086*	*0.096*	*0.084*	*0.234*	*0.085*	*0.104*	*0.157*	*0.603*
Diagnostics									
R^2	11.2%	11.2%	11.2%	11.2%	12.1%	11.2%	21.4%	31.8%	11.4%
Wald test: all coeff. = 0	13.4***	9.6***	14.8***	11.6***	5.3***	13.4***	n.a.	n.a.	28.7***
No. of firms	8,867	8,867	8,867	8,864	2,204	8,867	8,867	5,456	8,867
No. of observations	73,832	73,832	73,832	73,228	19,143	73,832	73,832	36,687	73,832

Table 4. Testing for Reversals and Pre-trends.

To investigate possible reversals and pre-trends, we include four lags and up to one lead in the baseline regression shown in Table 3, column 1. For variable definitions and details of their construction, see Appendix C. The unit of analysis is a firm-year. All specifications are estimated using OLS in first differences to remove firm fixed effects in the levels equations and include industry-year fixed effects to remove industry shocks. The fixed effects are not reported 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.

		Dep. var.: Change in long-term book leverage		
		(1)	(2)	(3)
Tax increase	at t = +1 (in %)	-0.121		
		0.302		
	at t = 0 (in %)	-0.234	-0.261	-0.369
		0.214	*0.232*	*0.241*
	at t = –1 (in %)	1.172***	1.229***	0.962***
		0.266	*0.209*	*0.195*
	at t = –2 (in %)	-0.328	-0.335	-0.392
		0.282	*0.285*	*0.263*
	at t = –3 (in %)	-0.126	0.000	0.088
		0.233	*0.232*	*0.221*
	at t = –4 (in %)	0.159	0.433	
		0.362	*0.315*	
Tax cut	at t = +1 (in %)	0.137		
		0.171		
	at t = 0 (in %)	0.142	-0.003	0.053
		0.172	*0.174*	*0.175*
	at t = –1 (in %)	-0.170	-0.019	-0.094
		0.188	*0.186*	*0.197*
	at t = –2 (in %)	-0.195	-0.028	0.003
		0.210	*0.170*	*0.165*
	at t = –3 (in %)	-0.032	-0.008	0.024
		0.146	*0.162*	*0.187*
	at t = –4 (in %)	0.184	0.116	
		0.125	*0.149*	
Lagged change in …				
ROA		-0.003	-0.002	-0.004
		0.006	*0.004*	*0.005*
firm size		0.008***	0.006***	0.007***
		0.002	*0.002*	*0.002*
tangibility		0.035***	0.036***	0.034***
		0.013	*0.013*	*0.011*
market/book		0.001*	0.001	0.000
		0.0005	*0.0005*	*0.0005*
default spread		-0.430***	-0.468***	-0.483***
		0.122	*0.107*	*0.104*
GSP growth rate		0.005	0.003	0.015
		0.025	*0.023*	*0.020*
state unemployment rate		0.142	0.110	0.133
		0.118	*0.104*	*0.107*
Diagnostics				
R^2		14.2%	13.2%	12.0%
Wald test: all coeff. = 0		7.3***	8.1***	9.3***
No. of firms		6,582	7,375	8,192
No. of observations		54,318	61,374	69,203

Table 5. 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 columns 7 and 8 model the probability that the firm issues or repurchases long-term debt, 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 to remove firm fixed effects in the levels equations and include industry-year fixed effects to remove industry shocks. (The specification shown in column 3 additionally includes state fixed effects and is estimated using Stata's *reg2hdfe* command for linear regressions with two high-dimensional fixed effects.) The fixed effects are not reported for brevity. Heteroskedasticity-consistent standard errors clustered at the 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.

	Dep. var.: Change in book leverage					*Dep. var.:*		
	long-term debt, 1990-2000 (1)	long-term debt, 2001-2011 (2)	long-term debt (w/ state FE) (3)	total (short- and long-term) debt (4)	long-term (incl. current portion) (5)	Change in long-term market leverage (6)	*Dep. var.:* Prob(issue long-term debt) (7)	*Dep. var.:* Prob(debt repur-chase) (8)
=1 if tax rise at $t = -1$ (in %, except cols. 7 & 8)	1.070***	1.025***	1.038***	0.830***	1.118***	0.660*	0.038***	-0.017*
	0.322	*0.236*	*0.225*	*0.225*	*0.191*	*0.342*	*0.010*	*0.010*
=1 if tax cut at $t = -1$ (in %, except cols. 7 & 8)	-0.103	0.158	-0.007	-0.032	-0.013	0.069	0.004	0.001
	0.188	*0.171*	*0.190*	*0.194*	*0.131*	*0.174*	*0.785*	*1.187*
Lagged change in …								
ROA	-0.001	-0.009**	-0.004	-0.010***	-0.012***	-0.017***	-0.031***	0.046***
	0.006	*0.003*	*0.004*	*0.004*	*0.004*	*0.003*	*0.009*	*0.010*
firm size	0.006**	0.007***	0.007***	0.011***	0.012***	0.027***	0.112***	-0.069***
	0.002	*0.003*	*0.002*	*0.002*	*0.002*	*0.003*	*0.008*	*0.007*
tangibility	0.038***	0.034**	0.037***	0.063***	0.057***	0.060***	0.258***	-0.164***
	0.013	*0.016*	*0.011*	*0.013*	*0.013*	*0.010*	*0.033*	*0.040*
market/book	0.000	-0.001***	0.000	-0.001**	-0.001	0.001***	0.007***	-0.005***
	0.001	*0.0004*	*0.000*	*0.0004*	*0.000*	*0.0004*	*0.002*	*0.001*
default spread	-0.188	-0.579***	-0.512***	-0.671***	-0.551***	-1.345***	-0.029***	0.038***
	0.502	*0.129*	*0.123*	*0.148*	*0.126*	*0.168*	*0.007*	*0.007*
GSP growth rate	0.039	0.014	0.024	0.033*	0.036**	0.017	0.032	-0.049
	0.027	*0.027*	*0.019*	*0.017*	*0.016*	*0.023*	*0.089*	*0.088*
state unemployment rate	0.170	0.186	0.138*	0.113	0.175**	0.166	-0.113	-0.022
	0.111	*0.128*	*0.084*	*0.090*	*0.085*	*0.107*	*0.334*	*0.606*
Diagnostics								
R^2	10.7%	12.0%	11.2%	11.3%	11.1%	20.3%	16.4%	14.0%
Wald test: all coeff. = 0	6.7***	13.2***	n.a.	52.0***	24.2***	49.1***	45.9***	38.6***
No. of firms	7,237	5,085	8,867	8,840	8,851	8,862	8,867	8,867
No. of observations	40,923	32,909	73,832	73,544	73,673	73,777	73,832	73,832

Table 6. 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 estimates a falsification test, asking if firms respond to tax changes that occur in a neighboring state when not themselves experiencing a tax change in their own home state. Columns 2 and 3 restrict the set of control firms to those located in a neighboring state, thus excluding far-away states. Column 2 focuses on firms in a tax-increase state and their untreated neighbors, while column 3 focuses on firms in a tax-cut state and their untreated neighbors; firms in states that neither experience a tax change nor border a state that does are excluded. Column 4 uses a restricted sample consisting of firms in contiguous 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 contiguous county do not. The effect of common local economic shocks are then removed by including county-pair/year fixed effects. Column 5 additionally requires that firms in contiguous county pairs operate in the same SIC4 industry in year *t*. The unit of analysis is a firm-year. All specifications except column 5 are estimated using OLS in first differences with industry-year fixed effects (not shown for brevity). Column 5 instead includes county-pair/industry/year fixed effects. For variable definitions and details of their construction, see Appendix C. Heteroskedasticity-consistent standard errors clustered at the 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.

	Dep. var.: Change in long-term book leverage				
		Firms located in		Firms in contiguous border counties	
	Full sample (1)	tax-increase states and their neighbors (2)	tax-cut states and their neighbors (3)	county-group/year FE & industry/year FE (4)	county-group/industry/year FE (5)
=1 if tax rise at $t = -1$ (in %)	0.931***	1.490***		1.305**	1.280***
	0.201	*0.321*		*0.666*	*0.382*
=1 if tax cut at $t = -1$ (in %)	0.011		-0.056	0.045	-0.109
	0.124		*0.185*	*0.582*	*0.597*
=1 if tax rise in a bordering state at $t = -1$ (in %)	-0.417***				
	0.142				
=1 if tax cut in a bordering state at $t = -1$ (in %)	0.067				
	0.081				
Lagged change in …					
ROA	-0.004	-0.019*	-0.005	0.003	0.008
	0.004	*0.010*	*0.005*	*0.012*	*0.022*
firm size	0.007***	0.010**	0.006	0.008	-0.004
	0.002	*0.004*	*0.004*	*0.006*	*0.009*
tangibility	0.037***	-0.001	0.054***	0.056	-0.071
	0.011	*0.030*	*0.015*	*0.035*	*0.043*
market/book	0.000	-0.002*	-0.001*	0.001	0.002
	0.000	*0.001*	*0.0006*	*0.001*	*0.001*
default spread	-0.524***	-0.755**	-0.780**	-1.419**	-0.447
	0.124	*0.293*	*0.312*	*0.673*	*1.102*
GSP growth rate	0.025	0.184**	0.021	0.127	-0.051
	0.019	*0.079*	*0.038*	*0.182*	*0.085*
state unemployment rate	0.198**	0.408**	0.367	-0.007	-0.014***
	0.077	*0.167*	*0.245*	*0.009*	*0.005*
Diagnostics					
R^2	11.2%	31.9%	22.9%	44.8%	31.2%
Wald test: all coeff. = 0	11.4***	8.2***	6.4***	n.a.	n.a.
No. of firms	8,867	4,750	6,440	2,278	532
No. of observations	73,832	10,180	25,071	12,822	1,520

Table 7. Potential Confounds: Union Power, Unemployment Risk, and Other Tax Changes.

This table investigates if our results are confounded by state-level variation in union power, unemployment risk, or non-corporate taxes. To address the first two confounds, we partition the sample into firms headquartered in states with high or low union power (columns 1 and 2) and those in states suffering large or no large employment shocks (columns 3 and 4). For the third confound, we omit as shocks any state corporate income tax change that coincides with a change in state personal income taxes (column 5) or state capital gains taxes (column 6). 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 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. (Reflecting the signed nature of the predictions, the test for equal tax sensitivity is one-sided.)

	Dep. var.: Change in long-term book leverage					
	States with …		States with …		Excluding coincident changes in state …	
	high union power	low union power	high mass layoffs	low mass layoffs	personal inc. taxes	capital gains taxes
	(1)	(2)	(3)	(4)	(5)	(6)
=1 if tax increase at $t = -1$	0.827***	1.467***	1.022***	1.042**	1.178***	1.139***
	0.227	*0.529*	*0.379*	*0.418*	*0.327*	*0.331*
=1 if tax cut at $t = -1$	0.044	0.189	0.210	0.357	0.014	0.018
	0.223	*0.316*	*0.255*	*0.253*	*0.172*	*0.172*
Lagged change in …						
ROA	-0.016***	0.003	-0.003	0.000	-0.004	-0.004
	0.004	*0.009*	*0.004*	*0.007*	*0.004*	*0.004*
firm size	0.006***	0.007	0.007***	0.010***	0.007***	0.007***
	0.002	*0.004*	*0.001*	*0.003*	*0.002*	*0.002*
tangibility	0.020	0.037*	0.053***	0.037	0.036***	0.036***
	0.013	*0.021*	*0.011*	*0.024*	*0.011*	*0.011*
market/book	0.000	-0.001	0.000	-0.001	0.000	0.000
	0.001	*0.001*	*0.000*	*0.001*	*0.000*	*0.000*
default spread	-0.353*	-0.087	-0.381**	-0.589***	-0.456***	-0.455***
	0.196	*0.476*	*0.161*	*0.184*	*0.116*	*0.116*
GSP growth rate	0.040	0.034	-0.057	0.014	0.022	0.021
	0.030	*0.048*	*0.041*	*0.029*	*0.019*	*0.019*
state unemployment rate	0.025	0.227	-0.153	0.185	0.157*	0.158*
	0.119	*0.188*	*0.194*	*0.143*	*0.086*	*0.086*
Diagnostics						
R^2	18.8%	28.4%	16.9%	20.3%	11.5%	11.5%
Wald test: all coeff. = 0	8.2***	8.7***	7.3***	7.2***	10.1***	10.1***
Equal tax sensitivity?	0.86		0.01		n.m.	
No. of firms	5,424	2,859	5,215	3,852	8,841	8,840
No. of observations	37,507	19,039	28,560	20,488	71,578	71,570

Table 8. Heterogeneous Treatment Effects.

Higher taxes on equity income (τ_e) dampen the impact of corporate tax changes on leverage. To test this, columns 1 and 2 split the sample according to a proxy for τ_e: non-dividend payers have lower τ_e than dividend-payers because their investors derive their equity income solely in the form of (lower-taxed) capital gains. A corollary of a causal interpretation of the observed tax sensitivity of debt is that it should vary with profits. Columns 3 and 4 partition sample firms according to whether they are profitable or loss-making in year 0. Columns 5-7 use John Graham's simulated marginal tax rates to partition sample firms into those with marginal tax rates that are zero, positive, or in the top decile, respectively. The extent to which a firm can borrow more when faced with a tax rise depends on its debt capacity. Columns 8 and 9 partition firms into those rated investment-grade or 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 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. (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 (1)	dividend payers (2)	profitable (3)	loss-making (4)	firms w/ zero marginal tax rates (5)	firms w/ positive marginal tax rates (6)	top decile of marginal tax rates (7)	invest-ment grade (8)	below invest-ment grade (9)
=1 if tax rise at $t = -1$ (in %)	1.433***	0.044	1.093***	0.288	0.688	1.067***	2.517**	0.887**	0.477
	0.276	*0.282*	*0.200*	*0.573*	*1.071*	*0.174*	*1.056*	*0.434*	*1.501*
=1 if tax cut at $t = -1$ (in %)	-0.028	0.102	-0.016	-0.124	-0.010	0.106	-0.226	0.101	-0.004
	0.180	*0.188*	*0.165*	*0.440*	*0.413*	*0.131*	*0.512*	*0.304*	*0.715*
Lagged change in …									
ROA	-0.007*	0.011	-0.008*	0.004	0.005	-0.006*	-0.073***	-0.018	0.019
	0.004	*0.012*	*0.004*	*0.006*	*0.013*	*0.004*	*0.022*	*0.026*	*0.046*
firm size	0.008***	-0.003	0.008***	0.009**	0.008*	0.007***	0.012*	-0.002	0.020**
	0.002	*0.004*	*0.002*	*0.003*	*0.004*	*0.002*	*0.007*	*0.007*	*0.009*
tangibility	0.036***	0.057**	0.025*	0.067***	0.044*	0.030***	0.094***	0.038*	0.063
	0.012	*0.024*	*0.013*	*0.018*	*0.024*	*0.011*	*0.031*	*0.023*	*0.045*
market/book	0.000	-0.004**	-0.001	0.000	0.001	-0.001**	0.002	-0.004**	-0.003
	0.000	*0.001*	*0.001*	*0.000*	*0.001*	*0.0004*	*0.001*	*0.002*	*0.004*
default spread	-0.494**	-0.658***	-0.625***	-0.312	-1.059	-0.496***	-1.654*	-0.080	-1.579**
	0.219	*0.245*	*0.094*	*0.401*	*2.488*	*0.117*	*0.897*	*0.309*	*0.745*
GSP growth rate	0.039	0.026	0.025	0.018	-0.045	-0.023	0.052	-0.005	0.025
	0.025	*0.037*	*0.021*	*0.048*	*0.118*	*0.018*	*0.084*	*0.039*	*0.096*
state unemployment rate	0.286**	-0.070	0.221***	-0.042	0.198	0.037	0.112	-0.062	0.179
	0.111	*0.169*	*0.081*	*0.207*	*0.406*	*0.100*	*0.495*	*0.206*	*0.469*
Diagnostics									
R^2	14.6%	32.3%	14.4%	26.8%	30.5%	12.2%	51.0%	11.3%	42.7%
Wald test: all coeff. = 0 (F)	14.1***	5.6***	15.2***	3.2***	3.0***	14.3***	6.4***	8.8***	1.7*
Equal tax sensitivity? (F)	10.45***		1.60		0.08		n.a.	0.21	
No. of firms	7,841	2,392	7,230	4,588	3,391	8,295	2,482	672	1,792
No. of observations	52,490	21,021	58,017	15,815	6,570	64,995	6,968	6,634	10,728

INTERNET APPENDIX

(NOT INTENDED FOR PUBLICATION)

Background on Tax Changes Affecting More Than 100 firms
(in reverse order by the number of treated firms)

1. Tax cuts

California 1997: tax cut from 9.3% to 8.84% (939 treated firms)

Party control: Governor: R, Lower House: D, Upper House: D

Political background: The theme of the 1997 budget was schools, law enforcement, and tax relief for businesses. The budget had all-party support from Republican Governor Wilson and the Democratic held legislature. Claiming it endangered school funding, Democrats blocked Governor Wilson's original, more ambitious $10 billion tax-reduction plan which included 5% cuts in personal, income and banking tax rates. The final budget did not change personal tax rates but included a $230 million cut in taxes on corporations and banks. The tax cut was seen as overdue; commentators pointed out that it moved California's corporate taxes from the 9th highest in the nation to the 15th highest.

Other notable changes: The budget's main focus was on $1,508 million in funding to school districts, primarily to reduce class sizes. It also provided an 8.4% increase in funding for prisons, a $100 million grant to local police forces, and a $50 million grant for local juvenile-justice programs; and a $287 million increase in the rainy-day fund. Separately, the state raised the R&D tax credit from 8% to 11%.

New York 1999: tax cut from 9% to 8.5% (383 treated firms)

Party control: Governor: R, Lower House: D, Upper House: R

Political background: In 1999, the New York legislature approved a $72 billion election-year budget that increased spending by 8.5%. Unusually, the budget process was largely out of Republican Governor Pataki's hands as the State Assembly held public, bi-partisan committee meetings to negotiate the budget. While the Democrats won generous spending increases, the Republicans won $740 million in tax cuts (mostly in the form of the acceleration of a previously scheduled property tax cut), to be rolled out over three years. Governor Pataki had not sought a corporate tax cut. The budget was hailed by politicians as good for "families and kids". Commentators called the budget fiscally irresponsible in the long-run. The state had experienced a fiscal surplus thanks to a booming Wall Street economy. It was felt that permanent tax cuts could lead to a budget crisis once the Wall Street boom ended.

Other notable changes: The budget included an additional $950 million in aid to schools, $500 million for school construction, and $200 million for child care.

New York 2000: tax cut from 8.5% to 8% (330 treated firms)

Party control: Governor: R, Lower House: D, Upper House: R

Political background: See New York 1999. The 2000 tax cut was part of the deal reached in 1999 whereby the corporate income tax rate would fall from 9% to 7.5% over three years.

Other notable changes: The $77 billion budget increased spending on education (now $13 billion) and doubled funding for a program to help the low-income elderly to buy prescription drugs. It increased the state's reserve fund to more than $3 billion and (for the first time in a number of years) did not cut Medicaid funding. New York also introduced borrowing limits, to be phased in gradually. A $3.8 billion bond issue to finance infrastructure was rejected in a referendum.

Texas 2008: tax cut (replacement of income tax with gross receipts tax, 300 treated firms)

Party control: Governor: R, Lower House: R, Upper House: R

Political background: Texas was forced to reform its tax system after the Texas Supreme Court declared the state's system for funding schools unconstitutional (Neeley v. West Orange-Cove I.S.D., Nov. 22, 2005). As part of the reform, Texas broadened the coverage of its corporation tax and expanded the taxable margin by taxing revenue rather than income. While overall a tax increase – corporate tax receipts increased from around $2.5 billion per year to around $4 billion – Texas's new "business franchise tax" effectively abolished the tax advantage of debt and so is coded as a tax cut for our purposes.

Other notable changes: Texas has a biennial budget cycle with the new budget coming into force on September 1st of every odd numbered year. Hence, there was no major budget change in 2008.

New York 2001: tax cut from 8% to 7.5% (294 treated firms)

Party control: Governor: R, Lower House: D, Upper House: R

Political background: See New York 1999. The 2001 tax cut was part of the deal reached in 1999 whereby the corporate income tax rate would fall from 9% to 7.5% over three years.

Other notable changes: The budget agreement between Republican Governor Pataki, the Republican Senate, and the Democratic Assembly provided for the largest increase in gambling since New York legalized gambling in 1966. Gambling was seen as a much-needed source of revenue (generating en estimated $1 billion per year) in the aftermath of the terrorist attacks on September 11th. Spending on state services such as legal advice for the poor was cut by $200 million. Spending on education (especially for teachers in urban districts) was increased by $200 million.

New York 2007: tax cut from 7.5% to 7.1% (252 treated firms)

Party control: Governor: D, Lower House: D, Upper House: R

Political background: Democratic Governor Spitzer, who in his election campaign had pledged not to raise taxes, instead attempted to close tax loopholes for businesses in his first budget. In the end, the $120 billion budget was one of the most generous in decades, owing to large projected budget surpluses from the Wall Street boom. In return for closing four loopholes, the corporate tax rate was cut to 7.1%.

Other notable changes: The budget extended health insurance coverage to 400,000 previously uninsured children and included a $1 billion cut in Medicaid, a $1.3 billion rebate on property taxes, a $600 million fund for stem-cell research, a doubling of the number of charter schools, and a $1.8 billion increase in spending on education.

New Jersey 1994: tax cut (repeal of 0.375% tax surcharge, 220 treated firms)

Party control: Governor: R, Lower House: R, Upper House: R

Political background: The 1989 tax surcharge was originally scheduled to end in mid-1994 but was repealed 6 months early as one of newly elected Republican Governor Whitman's first acts in office.

Other notable changes: The tax cut was funded by changing the actuarial assumptions underpinning the state's pension fund and thereby reducing the state's contribution to the fund by $1 billion a year. Separately, the state introduced a 2% investment tax credit to encourage investment in new fixed capital, an employment credit of $1,000 per new employee to encourage job creation, and a 10% R&D tax credit to encourage innovation.

Pennsylvania 1995: tax cut from 11.99% to 9.99% (208 treated firms)

Party control: Governor: R, Lower House: R, Upper House: R

Political background: In his first budget, Republican Governor Ridge, fulfilled his campaign pledge to cut business taxes in a bid to improve the state's business unfriendly image.

Other notable changes: The budget increased funding to education by $134 million, set aside an unprecedented $109 million for a rainy day fund, eliminated 15 state agencies, and abolished the unpopular "widow's tax".

Pennsylvania 1994: tax cut from 12.25% to 11.99% (200 treated firms)

Party control: Governor: D, Lower House: R, Upper House: R

Political background: Ever since the 1991 tax increase, Pennsylvania had been seen as a bad state to do business in. According to some estimates, the overall tax burden on business was the highest in the nation. The 1994 budget (Democratic Governor Casey's last due to term limits) cut corporate income taxes from 12.25% to 9.99% over a three-year period. The tax cut was

prompted by (and financed out of) a windfall (an unrelated favorable Supreme Court ruling) and a reduction in the Pennsylvania Industrial Development Authority's borrowing cost.

Other notable changes: The budget increased funding to poor school districts by $123 million, funding for children with disabilities by $33.9 million, Medicaid funding by $211 million, and the state's college scholarship grant program by 10%. New Jersey also reformed its welfare programs.

Massachusetts 2010: tax cut from 9.5% to 8.75% (160 treated firms)

Party control: Governor: D, Lower House: D, Upper House: D

Political background: As part of a corporate-tax reform that restricted out-of-state firms' ability to escape taxation in Massachusetts by requiring combined reporting, the "Act Relative to Tax Fairness and Business Competitiveness" cut Massachusetts' corporate tax rate from 9.5% to 8.75%. The extra revenue from closing the loophole was forecast to far outweigh the lost revenue from the rate reduction.

Other notable changes: The $27.6 billion budget was balanced with the help of $809 million in federal stimulus aid and by drawing $100 million from the state's reserve fund. It cut aid to cities by $900 million, laid off 1,500 state employees, and reduced spending on education by 4% and on higher education by 11.6%.

Minnesota 1991: tax cut (reduction in the legislated tax increase of 40 basis points, 146 treated firms)

Party control: Governor: R, Lower House: D, Upper House: D

Political background: Newly elected Republican Governor Carlson agreed to an increase in personal income taxes to close a projected budget deficit of $1.1 billion. Negotiations were difficult since the Democrats dominated both the Assembly and the Senate, both of which faced elections the following year. Corporate tax rates were originally scheduled to increase by 70 basis points in 1991, but this was cut to 30 basis points. To partly make up for the tax cut, the budget introduced a fixed fee for the privilege of doing business in the state.

Other notable changes: The budget included $200 million in spending cuts affecting financial assistance to cities, transportation infrastructure, and higher education, and a $600 million in tax increases on sales, personal income, and cigarettes. The property tax, a major political battleground, was held constant.

Ohio 1999: tax cut from 8.9% to 8.5% (146 treated firms)

Party control: Governor: R, Lower House: R, Upper House: R

Political background: The 1999 tax cut was part of Republican Governor Voinovich's biennial

$36 billion budget covering the period from mid-1997 to mid-1999. The 1997 budget was largely uncontroversial given higher than expected revenues and budget surpluses. The 1999 budget, which coincided with the scheduled 1999 corporate tax cut, focused on education. The trigger was the Ohio Supreme Court's order to reform the state's funding of public education.

Other notable changes: The 1999 budget also included a gradual phase-out of Ohio's tax on inventories held by businesses, to begin in 2002. The $17 billion education budget increased spending on basic and higher education by around 8%. The $23 billion general budget (two thirds of which is reserved for welfare and health) increased spending by around 6%.

Colorado 1993: tax cut from 5.1% to 5.0% (138 treated firms)

Party control: Governor: D, Lower House: R, Upper House: R

Political background: The corporate tax cut was scheduled by the Tax Reform Act of 1987.

Other notable changes: The constitutional amendment of 1992 severely constrained state finances by requiring voter approval of tax increases and limiting increases in state spending. To maintain the same level of services in Medicaid, schools, and higher education, the state needed an additional $500 million but the amendment allowed an increase in revenue of only $375million.

Connecticut 1997: tax cut from 10.75% to 10.5% (137 treated firms)

Party control: Governor: R, Lower House: D, Upper House: D

Political background: The 1997 tax cut was part of a 5-year step by step reduction of the corporate income tax from 11.5% to 7.5% enacted in 1995. It was a main plank of Republican Governor Rowland's ambition to shrink the state's government through lower taxes and lower government spending (helped by booming revenue from capital gains taxes in the wake of booming financial markets and six years of budget surpluses).

Other notable changes: The 1997 budget included nearly $400 million in tax cuts (on gasoline and personal income) and extensive property tax credits. (Perhaps not coincidentally, Governor Rowland announced he was to stand for re-election.) To cut government spending by $250 million, the state offered early retirement and contracted out all IT. The state ended the fiscal year with the largest surplus in a decade, totaling $260 million. The surplus was used to pay down debt, with the remainder transferred to the reserve fund. At the same as passing the budget, the legislature increased consumer protections from health maintenance organizations (HMOs).

Connecticut 1996: tax cut from 11.25% to 10.75% (134 treated firms)

Party control: Governor: R, Lower House: D, Upper House: R

Political background: The 1996 tax cut was part of a 5-year step by step reduction of the

corporate income tax from 11.5% to 7.5% enacted in 1995. It was a main plank of Republican Governor Rowland's ambition to shrink the state's government through lower taxes and lower government spending (helped by the fact that lawmakers faced elections later in the year).

Other notable changes: The 1995 biennial budget required adjustments in 1996 and after the sale of the state's lottery system was rejected by the legislature. In the end, the budget was easily balanced owing to a $250 million surplus (mostly from higher capital gains tax revenue). The state used $90 million to pay down debt and transferred $160 million to the reserve fund (only the second time ever it had made a contribution to the fund). Cuts to (and reform of) the state's welfare programs continued.

Colorado 1999: tax cut from 5% to 4.75% (133 treated firms)

Party control: Governor: R, Lower House: R, Upper House: R

Political background: A Republican controlled Assembly and Senate helped newly elected Republican Governor Owens to fulfill his campaign promises by enacting a series of tax cuts previously opposed by Democratic Governor Romer (in apparent violation of a 1992 amendment to the state constitution requiring revenue growth exceeding the combined annual increases in population and inflation to be returned to taxpayers).

Other notable changes: Both personal and corporate income taxes were cut from 5% to 4.75%. Several items exempted from taxation (long-term health care, coins, food sold by vending machines, etc). Taxes on telephone services were cut.

Massachusetts 2011: tax cut from 8.75% to 8.25% (132 treated firms)

Party control: Governor: D, Lower House: D, Upper House: D

Political background: As part of a corporate-tax reform that restricted out-of-state firms' ability to escape taxation in Massachusetts by requiring combined reporting, the "Act Relative to Tax Fairness and Business Competitiveness" cut Massachusetts' corporate tax rate from 8.75% to 8.25%.

Other notable changes: The budget had to close a $1.9 billion deficit (as Massachusetts lost $1.5 billion in federal funding). This was achieved through across-the-board spending cuts. Massachusetts legalized gambling in an effort to generate extra revenue of around $400 million.

Connecticut 1995: tax cut from 11.5% to 11.25% (124 treated firms)

Party control: Governor: R, Lower House: D, Upper House: R

Political background: Newly elected Governor Rowland – the first Republican to govern Connecticut in 20 years – campaigned on tax cuts, spending cuts, and being tough on crime. The corporate tax cut from was the first in a 5-year step by step reduction from 11.5% to 7.5%.

Other notable changes: The 1995 budget cut taxes (including corporate taxes) by $200 million and spending by $80 million to the lowest level since 1965. 2,200 state employees were to be laid off, welfare programs were to be cut by a quarter, and the number of state agencies was to be halved.

Colorado 1992: tax cut from 5.2% to 5.1% (124 treated firms)

Party control: Governor: D, Lower House: R, Upper House: R

Political background: The corporate tax cut was scheduled by the Tax Reform Act of 1987.

Other notable changes: Colorado adopted a Taxpayer Bill of Rights (TABOR) amending the state's constitution to give voters a veto over increases in state and local taxes and over spending increases. The TABOR also enshrines a flat income tax rate into the state's constitution.

Connecticut 1998: tax cut from 10.5% to 9.5% (122 treated firms)

Party control: Governor: R, Lower House: D, Upper House: D

Political background: The 1998 tax cut was part of a 5-year step by step reduction of the corporate income tax from 11.5% to 7.5% enacted in 1995. It was a main plank of Republican Governor Rowland's ambition to shrink the state's government through lower taxes and lower government spending. Re-election concerns (and a budget surplus) united the Republican Governor and Democratic lawmakers not to abandon the tax cuts.

Other notable changes: The highlight of the legislative session was campaign finance reform. A budget surplus projected at $460 million allowed the state to pay tax rebates to personal income taxpayers for the first time in its history. Gasoline tax was cut, aid to schools was increased, residents were given the right to choose their electricity provider, and tuition at state colleges was frozen. The budget also included an indirect cut in property taxes. Separately, the state introduced a 3% investment tax credit to encourage investment in new fixed capital.

Colorado 1988: tax cut from 6% to 5.5% (121 treated firms)

Party control: Governor: D, Lower House: R, Upper House: R

Political background: The tax cut was the first in a series of tax cuts scheduled over seven years by the Tax Equity Act of 1987. A state commission examined Colorado's state tax system after the federal tax reform of 1986 and came to the conclusion that a simplification of the state's overall tax system was needed. The Tax Equity Act provided for a single flat tax rate on personal and corporate income. The corporate tax rate change was phased in gradually in order to minimize its consequences for the state's revenue.

Other notable changes: [no info in Factiva]

Colorado 1989: tax cut from 5.5% to 5.4% (121 treated firms)

Party control: Governor: D, Lower House: R, Upper House: R

Political background: The corporate tax cut was scheduled by the Tax Reform Act of 1987.

Other notable changes: [no info on budget decisions]

Colorado 2000: tax cut from 4.75% to 4.63% (119 treated firms)

Party control: Governor: R, Lower House: R, Upper House: R

Political background: Republican Governor Owens, supported by the Republican-held Assembly and Senate, continued the series of tax cuts he initiated the year before, his first year in office.

Other notable changes: The main theme of the legislative session was education reform, after Colorado voters passed Amendment 23, instructing the state to spend $4.6 billion on education over the next 10 years. The sales tax was cut by 10 basis points and commercial vehicle registration fees by 25% (reducing revenue by $34 million). A committee was set up to make recommendations for reforming Colorado's tax system. New tax incentives were created to control pollution. Employers' contributions to unemployment insurance were cut by 20%. Private health insurance became tax deductible.

Connecticut 1993: tax cut (repeal of 10% tax surcharge, 118 treated firms)

Party control: Governor: Independent, Lower House: D, Upper House: D

Political background: The tax cut, which took place against a challenging economic and fiscal backdrop, was part of a two-year phase out of the surcharge introduced in 1991 when Connecticut's tax system underwent a complete overhaul.

Other notable changes: The budget made previous temporary welfare cuts permanent and cut $200 million over two years from the Medicaid budget by tightening eligibility. Taxes on cigarettes were increased together with a new tax on tires and an increase in the gasoline tax. The number of state government agencies was reduced. To boost economic development, the budget allocated $10 million of spending to the aerospace industry, $45 million of loan guarantees for bank lending to small and medium enterprises, $300 million for job training, and $60 million to regional economic development. Separately, the state introduced an R&D tax credit of between 1% and 6% and a job creation tax credit.

Colorado 1991: tax cut from 5.3% to 5.2% (116 treated firms)

Party control: Governor: D, Lower House: R, Upper House: R

Political background: The corporate tax cut was scheduled by the Tax Reform Act of 1987.

Other notable changes: Against the background of an improving local economy and a national recession, the budget included funding for major infrastructure projects, including a $4 billion new airport in Denver.

Connecticut 1992: tax cut (reduction in tax surcharge from 20% to 10%, 112 treated firms)

Party control: Governor: Independent, Lower House: D, Upper House: D

Political background: The corporate tax cut was part of a root-and-branch overhaul of Connecticut's tax system in 1991. Newly elected Governor Weicker had left the Republican party to run as an independent on a tax reform platform, including the introduction of a personal income tax with the aim to create a stable source of revenue to pay for health care, education, and prisons. Negotiations were difficult and the state government was at one point shut down. In the end, personal income tax was introduced at a rate of 4.5% and sales taxes were cut from 8% to 6%. The corporate tax rate cut was implemented as a two-year phase out of the 20% surcharge.

Other notable changes: The $8.2 billion budget closed a projected $3 billion two-year deficit through higher taxes and spending cuts. Spending cuts of $1.1 billion applied to all government agencies except those responsible for prisons and children/education. For example, welfare assistance was cut by $110 million, nursing-home services by $100 million, and aid to towns by $180 million. To boost the economy, the state's Works Jobs Funds received loan guarantees of $50 million.

Connecticut 1999: tax cut from 9.5% to 8.5% (110 treated firms)

Party control: Governor: R, Lower House: D, Upper House: D

Political background: Republican Governor Rowland, who had overseen significant tax and spending cuts in his first term in office, was re-elected with an overwhelming majority. It was the first time since 1944 that a Republican governor was re-elected in traditionally Democratic Connecticut. The 1999 tax cut was part of a 5-year step by step reduction of the corporate income tax from 11.5% to 7.5% enacted in 1995.

Other notable changes: The biennial budget increased spending by 4.8% in the first year and by 4.7% in the second year, the largest increases ever under Governor Rowland. The budget cut taxes by $270 million. A budget surplus of $550 million financed a $100 sales tax rebate for all residents and a $90 million increase in government employees' salaries. Separately, the state increased the investment tax credit rate from 3% to 4% of the cost of new fixed capital.

Colorado 1990: tax cut from 5.4% to 5.3% (108 treated firms)

Party control: Governor: D, Lower House: R, Upper House: R

Political background: Thanks to buoyant tourism, Colorado enjoyed a more upbeat economic outlook than the rest of the United States, which was heading into a recession. The state last experienced a recession in the late 1980s. The corporate tax cut was scheduled by the Tax Reform Act of 1987.

Other notable changes: [no info on budget decisions]

Connecticut 2000: tax cut from 8.5% to 7.5% (102 treated firms)

Party control: Governor: R, Lower House: D, Upper House: D

Political background: The 2000 tax cut was the last leg of a 5-year step by step reduction of the corporate income tax from 11.5% to 7.5% enacted in 1995. Facing elections and with the budget yet again in surplus, there was no pressure to abandon the tax-cutting program.

Other notable changes: The $12.3 billion budget reduced taxes on gasoline by 7 cents a gallon, eliminated sales tax on some items of clothing, and included tax breaks for hospitals worth $75 million. To shrink the size of the state government, spending was cut by $50 million (with $10 million coming from higher education) and hiring frozen at all state agencies. Separately, the state increased the investment tax credit rate from 4% to 5% of the cost of new fixed capital.

Ohio 2005: tax cut (replacing corporate income tax with gross receipts tax over a period of 5 years, 102 treated firms)

Party control: Governor: R, Lower House: R, Upper House: R

Political background: In the first major overhaul of the state's tax code in 70 years, Republican Governor Taft and the Republican House and Senate agreed to replace the corporate income tax with a gross receipts tax. The justification for the tax reform was to create a more equitable tax base, eliminating loopholes related to the calculation of taxable income for businesses. The reform was criticized for taxing companies regardless of whether they were profitable.

Other notable changes: The budget also phased out the tax on tangible property on machinery, equipment, and inventory (which provided annual revenue of $1.6 billion to local schools and government) and reduced personal income taxes by 21% over five years. It also repealed a half-cent state sales tax. On the spending side, the budget reduced financial aid to public schools and cut the costs of Medicaid by reducing spending on nursing homes.

2. Tax increases

New Jersey 1989: tax increase (introduction of 0.375% tax surcharge, 232 treated firms)

Party control: Governor: R, Lower House: R, Upper House: D

Political background: The main political battleground in New Jersey at the time of the 1989 budget was auto insurance reform: New Jersey's insurance rates were the highest in the nation. In response to a $400 million budget deficit resulting from lower than forecast revenues from sales and corporate taxes, outgoing Republican Governor Kean (who was term-limited) signed an austerity budget which included a surcharge on the corporate income tax.

Other notable changes: A $350 million bond issue was planned to finance open-space preservation and a $150 million bond issue was planned to finance the clean-up of New Jersey's coast line. Budget spending increased by only 0.8%.

New Jersey 2002: tax increase (introduction of Alternative Minimum Assessment tax, suspension of NOL deduction, 173 treated firms)

Party control: Governor: D, Lower House: D, Upper House: no overall control

Political background: Democratic Governor McGreevey carried out New Jersey's largest increase in business taxes since 1945 in the face of a budget deficit forecast at $3 billion in 2002. The Business Tax Reform Act was estimated to generate $800 million in additional revenue. It centered around the introduction of an alternative minimum tax and was presented as a move to close tax loopholes. The public backed the Governor's move to close the budget deficit by taxing businesses rather than workers or consumers.

Other notable changes: The budget increased the tax on cigarettes, allowed the state to borrow from the unemployment fund, and approved a $1.1 billion bond issue backed by the proceeds from settlements with the tobacco industry. Prior to the new Governor and Legislature taking office, the outgoing Assembly approved significant new spending ($680 million over 30 years on building landfills and trash incinerators, $120 million on pensions for veterans and disabled, $40 million on school aid) but also extended the energy tax to bring in an additional $800 million over four years.

Pennsylvania 1991: tax increase from 8.5% to 12.25% (171 treated firms)

Party control: Governor: D, Lower House: D, Upper House: R

Political background: The 1991 budget was negotiated in reaction to a severe budget crisis against the background of a nationwide economic downturn. The overall tax increase of $3.5 billion was the largest in Pennsylvania's history and involved increases in both personal income taxes (from 2.1% to 3.1%) and corporate income taxes (from 8.5% to 12.25%). The fact that the latter increased by substantially more was popular with the public; it explicitly shifted the burden to pay for services (and especially education) away from workers or consumers and towards

businesses.

Other notable changes: The budget expanded the sales tax to previously untaxed services such as personnel services, credit reporting, long-distance telephone calls, and computer and data processing services. It also allocated $280 million over four years to the Pennsylvania Industrial Development Authority and $30 million to Industrial Resource Centers which support small and medium sized manufacturers.

Illinois 1989: tax increase from 4% to 4.8% (163 treated firms)

Party control: Governor: R, Lower House: D, Upper House: D

Political background: The corporate tax increase was part of a surprising deal between the Democratic House and Republican Governor Thomson. The centerpiece of the budget deal was a 20% increase in personal and corporate income taxes. For the past three years, the governor had tried to increase income tax rates to fund increases in education and other state services but ran into opposition from the Democratic House Speaker. As a result, "lawmakers were frustrated with three years of belt-tightening budgets" (Chicago Tribune, 2 July 1989). The breakthrough came when some of the extra revenue from higher personal and corporate taxes was used to finance tax relief for homeowners.

Other notable changes: The budget also included a $96 million increase in taxes on cigarettes and a $300 million increase in gasoline taxes. Much of the overall $1.2 billion in tax increases was used to increase spending on education.

New Jersey 2006 tax increase (introduction of 4% tax surcharge on tax liability, 153 treated firms)

Party control: Governor: D, Lower House: D, Upper House: D

Political background: Democratic Governor Corzine came into office pledging to fix the state's fiscal situation resulting from a structural deficit of $4.5 billion despite the booming economy. The Governor's budget proposal included across-the-board-tax increases as well as $2 billion of spending cuts, but met with fierce resistance from lawmakers (despite Democratic control of both the Senate and Assembly) fearful of their re-election prospects in next year's election. The standoff led to a one-week government shutdown, New Jersey's first ever. The main battleground was the proposed one percentage point increase in the sales tax, the centerpiece of the budget. A compromise was reached when half of the sales tax increase was earmarked for a reduction in property taxes, which are among the highest in the nation.

Other notable changes: The budget included higher taxes on cigarettes and a surtax on the purchase of expensive cars. Much of the increase in tax revenue was applied to plugging structural budget deficits. On the spending side, funding for higher education was cut by $200 million.

California 2002: tax increase (suspension of net operating loss deduction, 140 treated firms)

Party control: Governor: D, Lower House: D, Upper House: D

Political background: California's 2002 budget crisis was to a large extent caused by the bursting of the tech bubble, which, through taxes on stock options, had for years underpinned strong revenue growth. Tackling the budget crisis proved politically contentious. A compromise was eventually reached after proposed tax increases on cigarettes and motoring were abandoned. Instead of anything that could be called a tax increase, the legislature agreed to what politicians called "revenue enhancements", including a suspension of companies' ability to carry forward net operating losses for a planned two years (though some lawmakers expressed their skepticism that the suspension would eventually be lifted as promised). The compromise ended a 60 day standoff and led to a budget with $9 billion in spending cuts and $2.4 billion in revenue increases (half of which coming from the NOL suspension). To pass the budget, Republicans voted with Democrats in the State Assembly. The $99 billion budget was the latest ever in recorded California history. It coincided with Democratic governor Gray Davis' campaign for re-election.

Other notable changes: Other "revenue enhancements" besides the NOL suspension included a suspension of a tax credit targeted at teachers and an increase in the withholding tax on stock options and bonus payments from 6% to 9.3%. The spending cuts were widespread, affecting all government operations and most spending programs, with particularly deep cuts in Medicaid.

Illinois 2011: tax increase from 4.8% to 7% (111 treated firms)

Party control: Governor: D, Lower House: D, Upper House: D

Political background: Illinois' fiscal position had been deteriorating for many years. The state had expanded spending during the 1990s and mid-2000s thanks to growing revenue, but did not raise taxes or accumulate any cash reserves. It had also accumulated large amounts of unfunded liabilities. Illinois' budget deficit in 2011 was projected at $13 billion (half the general-fund budget) and the state owed $3.9 billion in unpaid bills. It had long be clear that tax rates would have to increase to address the structural imbalance in the state's finances, but Democratic Governor Blagojevich had resisted tax increases. It was his successor, Democratic Governor Quinn, who eventually increased personal and corporate income taxes in the light of severely shrinking revenue estimates, negative trickle down effects of the state not paying its bills, and the potential for further deterioration in the state's bond rating.

Other notable changes: The budget included cuts in funding for public schools totaling $269 million.

Connecticut 1990: tax increase from 11.5% to 13.8% (due to introduction of 20% tax surcharge, 106 treated firms)

Party control: Governor: D, Lower House: D, Upper House: D

Political background: Connecticut's approach to taxing its residents had for a long time been to

define a narrow tax base and then to tax it at a high rate. For example, personal income was untaxed but corporations were taxed at the nationally high rate of 11.5%. Connecticut's main source of revenue was the sales tax, whose receipts were highly cyclical. Politicians had been debating tax reform for a number of years but yet again failed to tackle the structural problems in the 1990 budget. Instead, they temporarily increased corporate taxes by way of a tax surcharge and deferred the tough choices to after the gubernatorial election due later in 1990.

Other notable changes: The budget was characterized by many small fixes to the underlying budget problem including one-time revenue infusions, transfers among budget lines, and increased borrowing. For example, contributions to the teachers' retirement fund were reduced by $76 million, a $140 million was taken from the Property Tax Relief Fund, funding for state parks was reduced, and an across-the-board 2% cut in spending was imposed all government agencies.