NBER WORKING PAPER SERIES

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Working Paper 19551
http://www.nber.org/papers/w19551

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 2013

We are grateful to seminar audiences at HBS, NYU, NCCU, and the IDC Summer Finance Conference for their comments. Ljungqvist gratefully acknowledges the generous hospitality of Harvard Business School while working on this project. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 19551
October 2013
JEL No. G31,G32,G33

ABSTRACT

Financial constraints are not directly observable, so empirical research relies on indirect measures. We evaluate how well five popular measures (paying dividends, having a credit rating, and the Kaplan-Zingales, Whited-Wu, and Hadlock-Pierce indices) identify firms that are financially constrained, using three novel tests: an exogenous increase in a firm's demand for credit; exogenous variation in the supply of bank loans; and the tendency for firms to pay out the proceeds of equity issues to their shareholders ("equity recycling"). We find that none of the five measures identifies firms that behave as if they were constrained: public firms classified as constrained have no trouble raising debt when their demand for debt increases, are unaffected by changes in the supply of bank loans, and engage in equity recycling. The point estimates are little different for supposedly constrained and unconstrained firms, even though we find important differences in their characteristics and sources of financing. On the other hand, privately held firms (particularly small ones) and public firms with below investment-grade ratings appear to be financially constrained.

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How financial constraints affect firm behavior is a core question in corporate finance.[1] Answering it requires a way to identify constrained firms with reasonable accuracy. Since the financial constraints a firm faces are not directly observable, the empirical literature finds itself having to rely on indirect proxies (such as having a credit rating or paying dividends) or on one of three popular indices based on linear combinations of observable firm characteristics such as size, age, or leverage (the Kaplan-Zingales, Whited-Wu, and Hadlock-Pierce indices).

In this paper, we ask a simple question: How well do these measures of financial constraints identify firms that are plausibly financially constrained? The short answer is: not well at all.

Our empirical strategy is based on the premise that firms that are financially constrained effectively face an inelastic supply of external capital: raising external capital quickly becomes ever more expensive (reflecting a steep supply curve) and in the limit the firm is shut out of the capital markets (a vertical supply curve).[2] In contrast, firms that can raise a large amount of external capital without much of an increase in the cost of capital are plausibly unconstrained.

We propose three tests to identify the shape of a firm's supply of capital curve. The traditional way to estimate a supply curve is to exploit exogenous variation in demand. This is precisely what our first test does. Specifically, we use a natural experiment first analyzed by Heider and Ljungqvist (2013), consisting of 121 staggered changes in corporate income taxes levied by individual U.S. states. Debt confers a tax benefit on firms because the IRS allows firms to deduct interest payments from taxable income. All else equal, therefore, an increase in tax rates raises the value of debt tax shields and thereby increases firms' demand for debt. The observed sensitivity of a firm's borrowing to tax increases is then a natural measure of the local elasticity of the credit supply curve it faces.

[1] See, for example, Fazzari, Hubbard, and Petersen (1988), Kaplan and Zingales (1997), Almeida, Campello, and Weisbach (2004, 2011), Whited and Wu (2006), Rauh (2006), Leary (2009), Livdan, Sapriza, and Zhang (2009), Duchin, Ozbas, and Sensoy (2010), Almeida and Campello (2010), Brockman, Martin, and Unlu (2010), Denis and Sibilkov (2010), Giroud and Mueller (2013), among many others.
[2] As Almeida and Campello (2001) put it, "constrained firms are at the point where the supply of capital becomes inelastic." We formally define financial constraints in Section 3.1.

Our second test uses plausibly exogenous variation in the supply of a specific form of capital – bank loans made in a firm's home state. The intuition for this test is that an unconstrained firm should not be sensitive to small supply shocks to a specific form of capital (as long as its investment opportunity set remains unchanged): if bank loans become, say, less plentiful, it can easily substitute toward the next best source of funding. A financially constrained firm, on the other hand, will find it harder to substitute across sources of capital; in other words, its (overall) supply curve is less elastic than that of a firm with many choices of funding sources.

The source of variation we exploit for Test 2 is due to changes in state taxes on *banks*. Banks have a unique status for state tax purposes (Koch (2005)). They are taxed on a different schedule from corporations and so are subject to their own tax changes, which tend not to coincide with changes in the state corporate income taxes we use in Test 1. When a state increases its bank tax, it reduces the after-tax profit on every loan made to borrowers located in the state, regardless of the lender's own location. Variation in a state's bank taxes can thus induce variation in the supply of loans available to firms located in the state.

Our third test focuses on the supply of equity. It exploits the recently documented tendency of firms to pay out the proceeds of equity issues to their shareholders, a phenomenon Farre-Mensa, Michaely, and Schmalz (2013) call "equity recycling." A firm that pays out much of the proceeds obtained from issuing equity is unlikely to be financially constrained.

Section 3 discusses the identifying assumptions and limitations of each test at length. The key identification concern for Tests 1 and 2 is that state-level changes in tax rates coincide with unobserved economic shocks that might themselves affect the local demand and supply of credit. We address this concern by means of a difference-in-differences approach, using as controls only firms that are headquartered in states that border a tax-change state. This helps hold local economic conditions constant, isolating the effect of tax changes on firms' demand for debt.

To validate that each test has power to identify financially constrained firms, we use a set of firms that are plausibly constrained: private (i.e., non-stock market listed) firms. As expected, we find that private firms (especially small ones) do not increase their borrowing when their tax rates go up, suggesting they face an inelastic supply of credit (Test 1); are highly sensitive to variation in the local supply of bank loans, suggesting that they lack easy access to alternative funding sources (Test 2); and do not engage in equity recycling (Test 3). Our three tests thus appear to have enough power to identify financial constraints.

Yet when applied to the five most popular measures of financial constraints, all three tests paint a strikingly consistent picture: public firms that the literature classifies as 'constrained' do not behave in ways that suggest they face inelastic capital supply curves. Specifically, for each of the five constraints measures, we find that the average 'constrained' firm is able to:

- borrow more when it makes sense to do so (i.e., in response to an increase in state corporate income tax rates);
- maintain borrowing levels when banks lending in its home state are hit with a tax shock that shifts the local supply of bank loans; and
- raise equity and at the same time increase its payouts to shareholders.

These patterns are hard to reconcile with the notion that these firms are truly financially constrained. What is more, we find little difference in the magnitudes of the responses of 'constrained' and 'unconstrained' firms when hit with shocks to their credit demand or local bank loan supply. Nor do firms differ systematically in the extent of their equity recycling.[3]

As a final validation of our methodological approach, we apply our tests to a subsample of public firms that are a priori likely to face relatively inelastic capital supply curves and so are plausibly financially constrained: junk bond issuers. We show that these firms behave in a way consistent with being financially constrained: they do not borrow more when taxes increase; they

[3] We do observe systematic differences in the way 'constrained' and 'unconstrained' firms finance themselves and in key accounting and demographic variables (such as size or age). But these differences do not seem to correlate with behavior that suggests financial constraints.

are sensitive to changes in the supply of bank loans; and they do not engage in equity recycling. In other words, junk bond issuers behave much like privately held firms and very differently from supposedly ‘constrained’ public firms as identified by the five measures.

The paper proceeds as follows. Section 1 provides an overview of existing measures of financial constraints. Section 2 describes the sample and data. Section 3 outlines our three empirical tests and reports our main findings, showing that existing measures of financial constraints do not identify firms that behave as if they are indeed constrained. Section 4 discusses what kinds of firms these measures *actually* identify. Section 5 concludes.

1. Measures of Financial Constraints

Existing proxies aim to infer financial constraints from firms’ statements about their funding situation or changes in investment plans, their actions (such as not paying a dividend), or their characteristics (such as being young, or small, or having low leverage, or not having a credit rating). The literature is divided on which of these best captures financial constraints and as a result, empirical studies tend to employ a range of measures for robustness.

Judged by Google Scholar citations, the KZ index is the most popular measure of financial constraints. It has its origins in an influential debate between Fazarri, Hubbard, and Petersen (FHP, 1988) and Kaplan and Zingales (1997). Augmenting Hayashi’s (1982) *Q*-investment model, FHP find a significant sensitivity of investment to cash flow in a sample of 422 firms over the period 1970 to 1984. Based on the finding that cash flow sensitivities are especially large among the 49 sample firms that pay no or low dividends, FHP conclude that significant cash flow sensitivities reflect empirically important financial constraints. Implicit in their argument is the assumption that low dividends are a useful indicator of financial constraints.[4]

Using a text-based approach that has proved popular, Kaplan and Zingales (1997) challenge FHP’s conclusions. They assess whether a firm is financially constrained by reading the 10-Ks

[4] If this were literally true, the number of financially constrained firms in the U.S. could potentially be vast: over the 1989-2011 period that our tests focus on, nearly 70% of firms traded on U.S. exchanges *never* paid a dividend.

(annual reports) of the 49 supposedly constrained low-dividend firms in the FHP sample. Based on their reading, only 15% of firm-years show evidence of firms being unable to fund their desired investments. Moreover, estimated cash flow sensitivities are greatest not among these arguably constrained firms but among the firms that, based on their 10-Ks, are the *least* constrained. The implications are that neither absence of dividends nor significant cash flow sensitivities are useful indicators of financial constraints.

The actual KZ index is due to Lamont, Polk, and Saa-Requejo (2001). These authors estimate an ordered logit model relating the degree of financial constraints according to Kaplan and Zingales' (1997) classification to five readily available accounting variables: cash flow, market value, debt, dividends, and cash holdings, each scaled by total assets. The model is estimated on the 49 firms in FHP's sample and the estimated regression coefficients are used to construct an index. The resulting KZ index loads positively on market to book and leverage and negatively on cash flow, dividends, and cash. A higher index value suggests a firm that is more constrained. Subsequent authors have used Lamont, Polk, and Saa-Requejo's coefficient estimates to create an index for samples other than FHP's 49 firms, assuming implicitly that the coefficient estimates are stable across samples and over time. The convention in the literature is to classify, each year, the top tercile of firms as constrained and the bottom tercile as unconstrained. (Implicit in this approach is the assumption that the prevalence of financial constraints varies neither over time nor over the business cycle.)

Hadlock and Pierce (2010) update Kaplan and Zingales' (1997) text-based approach by combing the 10-Ks of 356 randomly selected firms over the period 1995-2004 for evidence of firms identifying themselves as financially constrained.[5] They use this classification to create their own index of financial constraints, based on size (with a negative loading), size-squared

[5] Ball, Hoberg, and Maksimovic (2012) take the text-based approach to its logical conclusion by machine-reading the 10-Ks of essentially all publicly traded firms in 1997-2009, identifying financially constrained firms as those that mention having recently delayed investment projects.

(positive), and age (negative). As with the KZ index, subsequent users of the HP index proceed by applying Hadlock and Pierce's coefficients to their own samples.

Another popular measure of financial constraints is to treat firms without a credit rating as constrained.[6] The empirical literature offers two main motivations for this. First, unrated firms are assumed to have no access to the public debt markets (Faulkender and Petersen (2006)) and so must borrow on less competitive terms from intermediaries such as banks. Second, the rating process may reduce information asymmetries between the firm and investors, which implies that unrated firms are more opaque than rated firms and so more likely to be rationed by lenders (see, for example, Whited (1992)).

Whited and Wu (2006) follow a different approach. Their index is based on the coefficients obtained from a structural model. The index is effectively measured as the projection of the shadow price of raising equity capital onto the following variables: cash flow to assets (with a negative loading); a dummy capturing whether the firm pays a dividend (negative); long-term debt to total assets (positive); size (negative); sales growth (negative); and industry sales growth (positive). Rather than re-estimating the structural model on their own samples, users of the WW index then extrapolate out of sample using Whited and Wu's reported coefficient estimates.[7]

2. Sample and Data

Our sample of public firms consists of all U.S. firms traded on the NYSE, Amex, or Nasdaq in fiscal years 1989 through 2011. Applying the same filters as in Heider and Ljungqvist (2013) gives a sample of 91,487 firm-years for 10,112 firms (though the need to lag certain variables as well as gaps in some firms' panel structure reduce the sample size used in our regressions).[8]

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[6] See, for example, Kashyap, Lamont, and Stein (1994), Almeida, Campello, and Weisbach (2004), or Adam (2009).

[7] As Whited and Wu note when discussing the literature's use of the KZ index, one concern with the practice of out-of-sample extrapolation of index coefficients is "parameter stability both across firms and over time." Despite this warning, the practice has continued and is now also common with the WW index.

[8] Starting with the merged CRSP-Compustat Fundamentals Annual database, Heider and Ljungqvist filter out financial firms (SIC=6), utilities (SIC=49), public-sector entities (SIC=9), non-U.S. firms, and firms traded OTC or in the Pink Sheets; firm-years with negative or missing total assets or missing return on assets; and firms with a single panel year or a CRSP share code >11 (REITS etc.).

We also use a sample of private U.S. firms, which we obtain from Asker, Farre-Mensa, and Ljungqvist (2013). The underlying data come from Sageworks, a database containing accounting data for hundreds of thousands of private U.S. firms for fiscal years 2001-2011. After filtering out non-U.S. firms, financial firms, regulated utilities, and firms with data quality problems, we have a panel consisting of 536,694 firm-years for 160,920 firms (though again the use of lags and gaps in the panel structure will reduce the sample size used in our regressions).

2.1 Summary Statistics

Table 1 shows summary statistics for public firms classified as 'constrained' or 'unconstrained' according to the five measures of financial constraints outlined in Section 1. The first classifies firms as constrained or unconstrained based on whether they do or do not lack a history of paying a dividend.[9] The second classifies firms based on whether they have or have had a credit rating from S&P, Moody's, Fitch, or Duff & Phelps. The final three measures classify as constrained firms in the top tercile according to the Kaplan-Zingales (KZ) index developed by Lamont, Polk, and Saa-Requejo (2001), the Hadlock-Pierce (HP, 2010) index, and the Whited-Wu (WW, 2006) index, respectively. Firms in the bottom tercile are classified as unconstrained.[10] (For variable definitions and details of their construction, see Appendix A.)

With the exception of the KZ index, which we will discuss later, the measures in Table 1 identify similar kinds of firms as constrained. Firms classified as constrained using the dividend, ratings, HP, or WW measures are younger and smaller compared to 'unconstrained' firms, carry more cash on their balance sheets, have fewer tangible assets, lower return on assets, and are more likely to be unprofitable. Similarly, 'constrained' firms are less leveraged, rely more on short-term debt, and more often have no long-term debt at all. (Each of these differences between 'constrained' and 'unconstrained' firms is statistically significant at the 1% level or better.)

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[9] To establish the necessary history, we look back as far as 1970.

[10] Accordingly, as is customary in the literature, we exclude from our analysis firms in the middle tercile. The use of terciles is necessarily arbitrary – as the indices are silent on appropriate breakpoints – but follows convention.

These characteristics are certainly intuitive markers for financial constraints, but whether they truly identify constrained firms remains to be seen.

Interestingly, 'constrained' firms have substantially higher market-to-book ratios, indicating that investors expect them to grow faster than 'unconstrained' firms. And indeed, 'constrained' firms experience significantly faster growth in both sales and employment. For example, unrated firms grow sales and employment by 27.7% and 13.5% a year on average, compared to less than half that (11.7% and 5.2%) among rated firms. Thus, being younger, smaller, less profitable, and less leveraged does not appear to be an impediment to fast growth.

We next examine differences in firms' investment in fixed assets and R&D. The evidence on fixed investment is mixed. On average, non-dividend payers invest significantly more than dividend payers (7% versus 5.3% of assets), while unrated firms invest nearly as much as rated firms (6.2% versus 6.3%). Similarly, constrained firms according to the HP index invest significantly more (5.5% versus 4.8%), but the opposite is the case according to the WW index (4.2% versus 6.0%).[11] For R&D, the evidence is unambiguous: in each case, 'constrained' firms invest significantly more than 'unconstrained' firms. The differences are quite substantial. For example, unrated firms spend an average of 7.7% of total assets on R&D a year, compared to 2.3% for rated firms. The differences are even larger for the other three measures.

These patterns suggest that being younger, smaller, less profitable, and less leveraged – i.e., being 'constrained' according to the dividends, ratings, HP index, and WW index measures – does not appear to be an impediment to investment or R&D.

2.2 Lamont, Polk, and Saa-Requejo's KZ Index

Lamont, Polk, and Saa-Requejo's (2001) version of the KZ index identifies a markedly different set of firms as constrained, on almost every dimension considered in Table 1. 'Constrained' firms according to the KZ index are only marginally younger and no smaller than

[11] The latter is a rare instance of the HP and WW indices producing different results.

‘unconstrained’ firms. They have *less* cash on their balance sheets, have *more* tangible assets, and are *less* often loss-making (though their ROA is marginally lower). They also have substantially *higher* leverage than do ‘unconstrained’ firms: 27.7% versus 8.5% long-term debt to book assets, on average.[12] Their market-to-book ratios are lower, as is their growth in sales or employment, and while they invest more in fixed assets, they spend considerably less on R&D.

2.3 Cross-tabulations

Panel B reports cross-tabulations of the five measures. For each measure, the first five rows show the fraction of firms classified as constrained according to the measure that would also be classified as constrained under each of the other four measures. This illustrates the extent to which the five measures produce similar classifications. Consistent with the evidence shown in Panel A, the KZ index correlates the least with the other four measures, which in turn correlate highly with each other. Generally, the greatest agreement is between the HP and WW indices. To illustrate, column 4 shows that, among firms classified as constrained according to the HP index, 84% do not pay dividends, 98.8% are unrated, and 97.5% are also constrained according to the WW index. But only 44.5% of them are constrained according to the KZ index.

The last five rows of Panel B report the fraction of firms classified as *unconstrained* that would be classified as constrained under the other measures. Except for the HP and WW indices, there is much less agreement. For example, 58.1% of dividend payers lack a credit rating while 29.8% of rated firms do not pay dividends. The KZ index again stands out. For example, lack of a credit rating is *more* common among firms the KZ index classifies as unconstrained than among constrained firms.

3. Do Measures of Financial Constraints Measure Financial Constraints?

The summary statistics and cross-tabulations reported in Table 1 indicate that there are important commonalities among firms classified as ‘constrained’ by the dividends, ratings, HP,

[12] This is not surprising, given that leverage and cash enter into the index with financial constraints increasing in leverage and decreasing in cash.

and WW measures (the KZ index appears to be more of an outlier). In this section, we investigate whether these commonalities are driven by financial constraints, as the literature assumes, or whether they reflect some other differences (say, a firm's lifecycle stage).

We begin by formally defining financial constraints. We then present three tests that evaluate the five financial constraints measures outlined in Section 1. Overall, the evidence from these tests suggests that the behavior of firms classified as financially constrained is not obviously consistent with them in fact being financially constrained.

3.1 Defining financial constraints

As Tirole (2006) explains, financial constraints arise due to frictions in the supply of capital, the chief source of friction being information asymmetries between investors and the firm. Supply frictions decrease the elasticity of the supply of external capital curve, driving a wedge between the internal and the external cost of capital.[13] Almeida and Campello (2001), for example, observe that "constrained firms are at the point where the supply of capital becomes inelastic." In the limit, a perfectly inelastic (i.e., vertical) supply curve implies that the firm "has been cut out of its usual source of credit" (Kaplan and Zingales (1997)).

To formalize this, denote a firm's capital supply curve by *p(k)*, a function capturing the price at which a firm with *k* units of financial capital can raise an incremental unit of capital in the capital markets. We can then characterize the extent of financial constraints a firm faces in terms of the elasticity of *p(k)*: the steeper (i.e., more inelastic) the supply curve, the more financially constrained the firm. Thus, a firm is financially constrained if it faces a highly inelastic supply curve: $(\partial p(k)/\partial k)(k/p(k)) >> 0$. This captures Kaplan and Zingales' (1997) notion that the firm has been "cut out" of the capital markets. Figure 1 illustrates the definition graphically:

[13] Fazzari, Hubbard, and Petersen (1988) note that when the supply curve becomes inelastic, "the cost of new debt and equity may differ substantially from the opportunity cost of internal finance generated through cash flow and retained earnings."

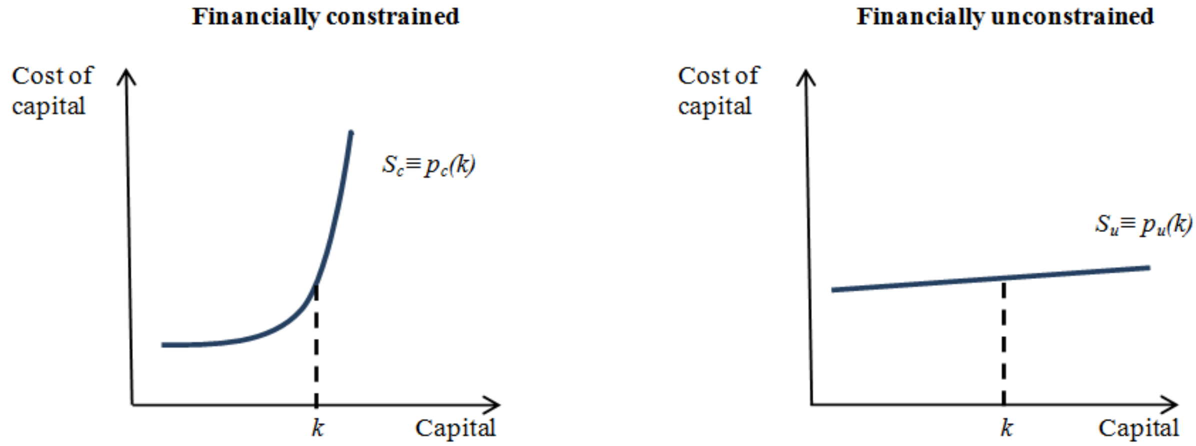

Figure 1. The figure shows the supply of capital curves faced by two hypothetical firms, both currently holding *k* units of capital. The firm on the left is financially constrained. The firm on the right is financially unconstrained.

As a firm's capital supply curve is not readily observable to the econometrician, measuring financial constraints is empirically challenging. This is why the literature instead attempts to infer the elasticity of the capital supply curve indirectly, by looking either at what managers say in their 10-Ks (e.g., KZ index, HP index) or at a particular action they take (e.g., pay a dividend, obtain a credit rating). The identifying assumption behind measures based on these approaches is that managers' words or actions reflect the shape of the supply curve as they perceive it. If this assumption is correct, we should observe that firms classified by these measures as financially constrained behave as if their supply of capital curve were indeed highly inelastic.

This is precisely the motivation of our first test. Specifically, we exploit exogenous variation in the demand for debt to estimate the average elasticity of the supply of debt curve faced by firms the literature classifies as constrained. This follows the usual identification strategy used to estimate the shape of a supply curve (see, for example, Hayashi's (2000) textbook, p. 189).

3.2 Test 1: Exploiting tax increases as shocks to the demand for debt

Debt confers a tax benefit in the U.S. given that interest payments are tax-deductible. The standard trade-off theory of capital structure hence predicts that the demand for debt of a firm expecting to be profitable should increase in its marginal tax rate. In recent work, Heider and

Ljungqvist (2013) provide evidence consistent with this prediction. Their identification strategy exploits 43 staggered increases in state corporate income taxes in 24 U.S. states and 78 staggered tax cuts in 27 states between 1989 and 2011. They find that, on average, public firms increase their long-term leverage by 104 basis points in response to a tax increase measuring on average 131 basis points. Importantly, this is a pure capital-structure change: firms do not increase their asset base overall, suggesting that investment opportunities remain unchanged and that the tax shock increases their relative demand for debt but not their overall demand for capital. Heider and Ljungqvist also show that firms do not reduce their leverage in response to tax cuts, suggesting that the tax sensitivity of leverage is asymmetric.

Motivated by this evidence, we exploit increases (but not cuts) in state corporate income tax rates as plausibly exogenous shocks to the demand for debt.[14] This allows us to estimate the shape of the debt supply curve faced by firms classified as either constrained or unconstrained. Figure 2 illustrates the identification strategy for Test 1.

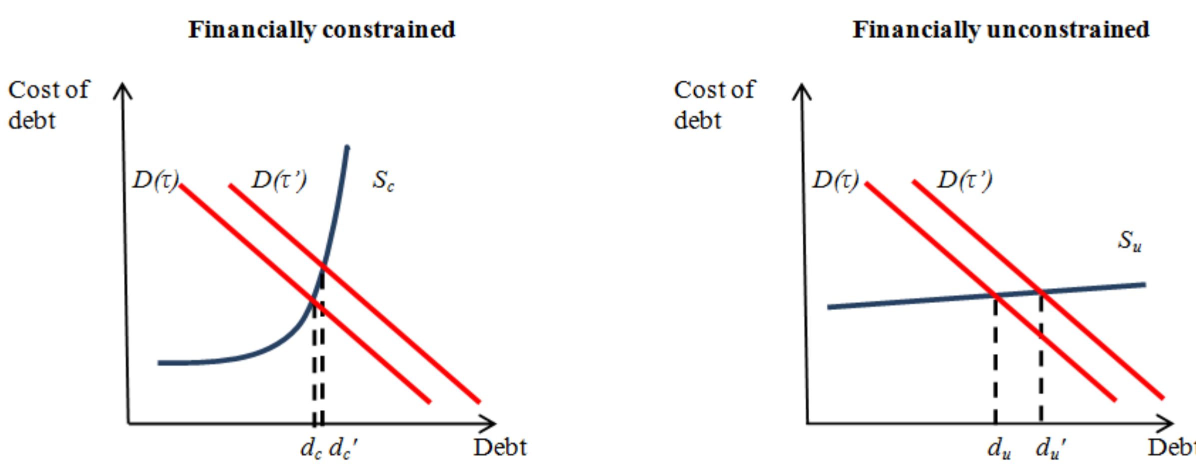

Figure 2. This figure illustrates the identification strategy for Test 1 by showing how a corporate tax increase from τ to τ' shifts the demand curve for debt, *D*, and the effect that this shift has on the debt holdings of a financially constrained firm (from d_c to d_c') and of a financially unconstrained firm (from d_u to d_u').

A firm that is financially constrained (i.e., one that faces an inelastic supply of debt curve) should not (meaningfully) increase its debt holdings in response to an increase in state taxes. An

[14] See Heider and Ljungqvist's (2013) Appendix A for a list of the relevant tax increases.

unconstrained firm, on the other hand, should make full use of the additional tax shields by issuing debt. Empirically, therefore, we can judge how well a financial constraints measure actually captures constraints by testing for a higher average sensitivity of debt to tax increases among firms classified as constrained than among firms classified as unconstrained.

In what follows, we discuss the identifying assumptions and limitations of Test 1, lay out the empirical specifications we estimate, show that the test has power to identify financially constrained firms, and test whether firms classified as financially constrained in the literature respond less strongly to corporate tax increases than do firms classified as unconstrained.

3.2.1 Identifying assumptions and limitations

A key requirement for our identification strategy to be valid is that we can capture changes in debt holdings that are the direct result of the tax increases and so are not confounded by other shifts in the capital demand or supply curves. An important concern is that unobserved business cycle factors cause both states to raise tax rates (shifting the demand for debt due to the increased value of interest payments as tax shields) and, at the same time, firms to invest less (decreasing their demand for capital) or banks to cut lending (shifting the supply of debt). For example, it is possible that due to balanced-budget rules, states raise taxes when the local economy is weak (and tax revenues fall) at the same time as banks suffer an increase in defaults and so cut lending.

Heider and Ljungqvist (2013) find no evidence that state tax changes correlate with observed local business cycle effects, which is reassuring. To mitigate the potential influence of unobserved business cycle effects, we follow their approach and exploit the local and staggered nature of state tax increases. First, we estimate difference-in-differences tests, using as controls firms that have not been affected by a tax increase. This establishes a counterfactual for the observed change in debt holdings. Second, we restrict the control group to firms located in a state adjacent to the tax-increase state. This differences away changes in debt holdings that are the result of changes in local economic conditions and so allows us to identify the effect on debt holdings of exogenous shifts in the debt demand curve induced by tax increases.[15]

Test 1 has two main limitations. First, given that tax shields are only of value to firms that have (or expect soon to have) profits to shield from tax, the test cannot identify financial constraints among chronically loss-making firms. To (conservatively) account for this, Test 1 excludes firm-years with losses. Second, Test 1 focuses on changes in the demand for debt, not for equity. Thus, it can only identify whether firms classified as financially constrained are unable to raise debt; it is silent regarding their ability to raise equity. Given that having restricted access to the debt market is a necessary but not sufficient condition for a firm to be financially constrained, the test allows us to effectively identify firms that are *not* financially constrained, but it cannot unambiguously identify firms that *are* constrained.

3.2.2 Empirical specification

Equation (1) implements our empirical strategy for Test 1 as follows:

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

where *i* indexes firms, *j* industries, *s* headquarter states, and *t* fiscal years. The dependent variable *D* is either long-term book leverage, as is common in the capital structure literature, or log long-term debt. The latter allows us to show that our results are not driven by firms increasing leverage through asset sales, without actually raising any debt. The main variable of interest, T^{+}, is a dummy variable equal to 1 if the state a firm is headquartered in increased its top corporate income tax rate, and 0 otherwise.[16,17] To ensure our results are not driven by changes in debt holdings that are unrelated to the tax changes, the vector *X* includes controls for ROA,

[15] Some corporate tax rises coincide with bank tax rises. This shifts the demand for debt out and the supply of debt in (see Test 2), biasing us against finding a significant response to corporate tax rises. Our estimates are thus conservative. More potentially problematic are corporate tax rises that coincide with bank tax *cuts* (as the increase in credit supply could allow even financially constrained firms to borrow more, undermining the identification strategy). Fortunately, bank tax cuts never coincide with corporate tax increases in our sample period.

[16] Following Heider and Ljungqvist (2013), we lag this variable to ensure that firms have enough time to adjust their debt holdings in response to a corporate income tax increase.

[17] Note that firms are taxed not where they are incorporated but where they operate (which Heider and Ljungqvist (2013) approximate using a firm's headquarter state). We use Heider and Ljungqvist's hand-collected HQ data, rather than Compustat's (which suffers from backfill bias).

tangibility, firm size, and a proxy for investment opportunities. (For variable definitions and details of their construction, see Appendix A.)

We estimate equation (1) using OLS in first-differences to remove time-invariant unobserved firm heterogeneity and include industry-year fixed effects to remove the effects of unobserved time-varying industry shocks. Following Heider and Ljungqvist (2013), the sample is restricted to treated firms (those in a state experiencing a tax increase) using their immediate neighbors (in all adjacent states that do not change tax rates) as controls. Constraining treated and control firms to be neighbors minimizes the impact of unobserved differences in economic conditions between treated and control firms.

3.2.3 Power

To see if Test 1 has enough power to identify constraints, we compare the average tax sensitivity of debt among public firms in the U.S. to that of private U.S. firms contained in the Sageworks database (see Asker, Farre-Mensa, and Ljungqvist (2013)).[18] The assumption motivating this power test is that private firms are more likely to be financially constrained than public firms (see, for example, Saunders and Steffen (2011) for evidence consistent with this assumption). Thus, if Test 1 has power to identify financial constraints, we should find that private firms respond less strongly to tax increases than do public firms.

The results, reported in Table 2, support this prediction. To provide a baseline, column 1 shows that the average public firm increases its leverage by 1.1 percentage points in response to a tax rise ($p<0.001$), compared to other firms operating in its industry located elsewhere in the U.S. This is identical to the corresponding point estimate in Heider and Ljungqvist's (2013) Table 9. Column 2 restricts the control group to firm-years neighboring a tax-increase state. As in Heider and Ljungqvist, requiring treated firms and their controls to be geographically proximate increases the estimated tax sensitivity, to 1.3 percentage points ($p<0.001$). Columns 5

[18] We restrict the sample of private firms to include only C Corps. Firms that are S Corps or are unincorporated pay personal rather than corporate income taxes and so are not affected by our corporate income tax shocks.

and 6 show that these results also hold when we use the change in log long-term debt as the dependent variable: public firms significantly increase the amount of debt in their capital structure, by between 8.6% and 10.1% on average. This alleviates concerns that our findings might be driven by changes in firms' total assets rather than in their debt holdings.

Overall, these results show that the average public firm is able to borrow more when its demand for debt increases exogenously. This is not the case for private firms. Relative to private control firms in neighboring states, private firms in treated states do not increase their leverage or log debt outstanding significantly in response to a tax increase. In column 3, for example, the point estimate is 0.4 percentage points with a *p*-value of 0.269 – less than a third of the average public firm's tax sensitivity. This is consistent with the assumption that private firms are more likely to be financially constrained than public firms and so suggests that Test 1 has power to identify financial constraints.

Columns 4 and 8 corroborate this conclusion by allowing private firms' tax sensitivity to differ depending on their size. While private firms in the three bottom size quartiles do not increase their debt holdings significantly in response to a tax increase, the very largest private firms do. Leverage, for example, increases by 1 percentage points among the largest firms (p=0.021), not far off the point estimate for the average public firm. These patterns are consistent with small private firms being more financially constrained than the very largest private firms, which in turn behave more like public firms on average.

3.2.4 Are 'constrained' firms financially constrained?

Table 3 compares the tax sensitivity of debt holdings across firms classified as financially constrained or unconstrained by the dividend and credit-rating measures, as well as by the KZ, HP, and WW indices. Panels A and B show how long-term book leverage and log long-term debt, respectively, respond to corporate tax increases.

The table reveals two noteworthy results. First, firms classified as constrained according to each of the five measures are in fact able to increase their leverage significantly when their demand for debt increases exogenously. So unlike (small) private firms, supposedly constrained public firms can and do respond to tax increases. Second, there is no evidence that firms classified as constrained react any less strongly than firms classified as unconstrained, whether we focus on changes in leverage or in log debt.[19]

The results for Test 1 cast doubt on the ability of standard measures of financial constraints to identify firms that are constrained in their ability to raise debt capital: none of the five measures of financial constraints identifies firms with unusually inelastic supply of debt curves. In fact, the estimated supply curves seem remarkably flat: the coefficients among 'constrained' firms range from 1.2 to 2.1 percentage-point increases in leverage. Economically, this implies that the average 'constrained' firm raises new debt equivalent to between 19% and 38% of its annual CAPEX spending (see Table 1) – a sizeable amount. The two caveats, as mentioned before, are that Test 1 is silent on how constrained loss-making firms might be and that it cannot speak to firms' ability to raise equity when unable to raise debt. To address these caveats, we introduce two alternative identification strategies.

3.3 Test 2: Exploiting bank tax changes as shocks to the supply of debt

Test 2 is based on the following premise. The behavior of an unconstrained firm should not be affected by small shocks to capital supply that do not alter its investment opportunity set: if one source of capital becomes, say, less plentiful, an unconstrained firm can simply substitute towards another. A financially constrained firm, on the other hand, faces an inelastic capital supply curve and thus should find itself less able to substitute across sources of capital.[20] Its

[19] Heider and Ljungqvist (2013) show that the leverage response to tax increases is smaller the more geographically dispersed a firm's operations. The reason is that firms are taxed in each state in which they operate, so exposure to the tax treatment varies with the concentration of their operations in their HQ state. Table 1 shows that with the exception of the KZ index, 'constrained' firms have less dispersed operations, so we expect a larger tax sensitivity, all else equal. The data confirm this: in 7 of the 10 cases in Table 3, the estimated sensitivity is somewhat larger for 'constrained' firms, significantly so for the dividend measure (*p*-value for the difference in coefficients = 0.068).

[20] This is similar in spirit to Lemmon and Roberts (2010), Chava and Purnanandam (2011), and Lin and Paravisini (2011), who analyze the effects of credit supply shocks on firm investment, performance, and risk, respectively.

reliance on debt should thus be more sensitive to shocks to the supply of debt from a particular source. These predictions hold whether or not the firm is profitable, allowing us to address the first limitation of Test 1.

To operationalize Test 2, we exploit 88 state-level changes in *bank* taxes between 1989 and 2011, listed in Appendix B. Importantly, for state tax purposes, states apportion a bank's income from lending to their state based on the location of the borrower, rather than the lender. Changes in state bank taxes, by affecting the after-tax profitability of lending, thus directly affect the supply of bank loans available to firms located in the state (though the economic magnitude of the effect is an empirical question, which we address below). As a result, we expect banks to expand lending in states with falling taxes and reduce it in states with rising taxes. Figure 3 illustrates our identification strategy, using a bank tax increase as an example.

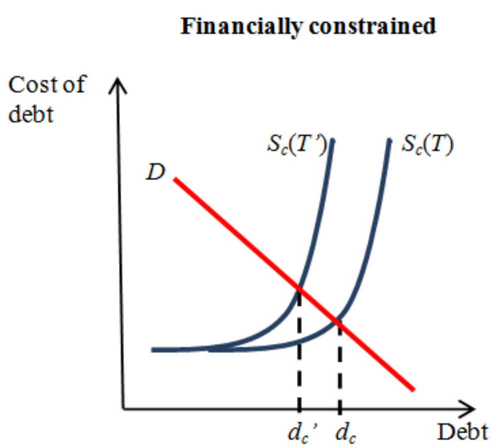

Figure 3. This figure illustrates the identification strategy for Test 2. It shows how a bank tax increase from *T* to *T'* shifts the supply curve of debt, *S*, to the left and traces out the effect of this shift on the debt holdings of a financially constrained firm (from d_c to d_c') and of a financially unconstrained firm (from d_u to d_u').

For a financially constrained firm, depicted on the left, rising bank taxes shift its (inelastic) credit supply curve to the left. Absent suitable alternatives, the firm will have to reduce its reliance on debt by a large amount, from d_c to d_c'. Unconstrained firms, depicted on the right, face a flatter (more elastic) credit supply curve, meaning they have access to alternative sources of debt. As a result, a reduction in local bank lending has little impact on their debt holdings.

As in the previous section, we first discuss the identifying assumptions and limitations of the test. We then show that bank tax changes affect bank lending, as required for identification, lay out our empirical specifications, use the private-firm sample to show that the test has power to identify financially constrained firms, and finally test if firms classified as constrained in the literature are indeed sensitive to tax-induced variation in the local supply of bank loans.

3.3.1 Identifying assumptions and limitations

The key identifying assumption of Test 2 is that the tax-induced supply shocks are not confounded by changes in firms' demand for debt, allowing us to isolate changes in debt holdings that are the direct result of changes in supply. This assumption faces two main potential challenges: states don't change bank taxes in a vacuum and bank tax changes could coincide with the corporate tax changes we analyzed in Test 1. We address each concern in turn.

We follow a two-pronged approach to address the non-random nature of bank tax changes. We first ask if observed variation in local conditions causes both states to change bank taxes and firms to change their demand for debt. Table IA.1 in the Internet Appendix relates the probability of a bank tax change to political and economic conditions, focusing on the governor's political affiliation, the state's budget balance, bond rating changes, growth, unemployment, unionization, and tax competition with neighboring states. This reveals that states are more likely to cut bank taxes the larger their budget surplus, the higher their taxes relative to their neighbors, and if governed by a Republican; and more likely to increase bank taxes the larger the budget deficit and the lower their taxes relative to their neighbors. None of these factors has any obvious direct link to firms' demand for debt, mitigating omitted variable concerns. Second, as in Test 1, we remove *unobserved* changes in local conditions by means of a diff-in-diff approach, using as controls only firms headquartered in states that border a tax-change state.

When bank tax increases coincide with corporate tax increases, two things happen: credit supply contracts and demand for credit increases.[21] For a constrained firm, the demand increase could partially offset the supply shock, leaving its debt holdings little changed. This would make it look as if the firm were unaffected by the supply shock and hence unconstrained.[22] Of the 88 bank tax changes in our sample, 24 are increases that coincided with a corporate tax rise. We show in Table IA.2 in the Internet Appendix that our Test 2 results are robust to excluding these.

A more philosophical challenge when exploiting supply shocks to identify financial constraints is how large the shock should ideally be. Too small and the test will have no power. Too large and it will fundamentally change the shape of the capital supply curve for virtually *all* firms. For instance, if the global financial system were to collapse due to another financial crisis, a large number of previously unconstrained firms would presumably find themselves facing an inelastic supply curve.[23] But the aftermath of such a shock would not be particularly informative of the financial constraints faced by such firms in more ordinary times. In other words, a large shock to the financial system would have poor external validity.

In practice, changes in state bank taxes tend to be relatively small: over our sample period, bank tax increases and cuts average 71 and –52 basis points, respectively. Tax shocks of these magnitudes are unlikely to fundamentally alter the capital supply curves firms face. The greater concern is instead whether the shocks are large enough to induce a significant change in lending behavior.

The first three columns in Table 4 aim to answer this question. We use Call Report data from the Federal Reserve to analyze the sensitivity of bank lending to changes in state bank taxes. Specifically, we analyze how changes in a state's bank tax rate affect (the logarithm of) the total dollar amount of commercial and industrial (C&I) loans made by banks headquartered in that

[21] We need not worry about the opposite case of bank tax cuts coinciding with corporate tax cuts, as Heider and Ljungqvist (2013) show that corporate tax cuts have no effect on firms' demand for debt.
[22] Contrast this with Test 1, where overlapping tax increases merely diminish the power of the test.
[23] Graphically, a shock of such magnitude would not only shift the capital supply curve inward, as in Figure 3, but would make the capital supply curve of most previously unconstrained firms as inelastic as the S_c curve in Figure 1.

state,[24] following a differences-in-differences approach similar to that used in Test 1. We find that a one-percentage-point increase (cut) in state bank taxes is associated with a highly significant 1.5 to 1.8 percent decrease (increase) in C&I loans made by banks headquartered in the state, relative to untreated control banks in the adjacent states. This suggests that changes in state bank taxes induce a significant change in bank lending behavior, a necessary requirement for our identification strategy to be valid.

3.3.2 Empirical specification

Equation (2) captures our empirical strategy for Test 2, which as in Test 1 is estimated by OLS in first-differences with the control group restricted to firms located in neighboring states to those affected by a bank tax change:

$$\Delta D_{ijst} = \beta BT_{st} + \delta \Delta X_{it-1} + \alpha_{jt} + \varepsilon_{ijst} \tag{2}$$

As before, i indexes firms, j industries, s firms' headquarter states, and t fiscal years. In this case, our main regressor of interest, BT, is a variable capturing the tax rate change (in percentage points) affecting banks that lend in the state the firm is headquartered in (regardless of the banks' own geographic location). As in Test 1, we model changes in both long-term book leverage and log long-term debt, and we include the same control vector X as in equation (1). Unlike in the case of Test 1, the identification strategy behind Test 2 does not require firms to be profitable and so we include all firm-years in our analysis.

3.3.3 Power

To establish whether Test 2 has enough power to identify financial constraints, we compare public and private firms' sensitivity of debt holdings to changes in bank taxes, as an instrument for changes in bank credit supply.[25] To the extent that private firms, particularly small ones, are more likely to face an inelastic debt supply curve, we expect their debt holdings to be more

[24] We focus on banks with total assets between $500 million and $10 billion in 2005 dollars, thus excluding banks that are too small to provide any meaningful lending to public firms as well as the largest systemic banks that are too big to be affected by changes in individual states' taxes. Excluding the largest banks has no impact on our results.

[25] Unlike in Test 1, this test does not require the private-firm sample to be restricted to C Corps.

sensitive to shifts in the debt supply curve than those of public firms.

The results, shown in Table 4, are consistent with this prediction. In column 4, changes in state bank taxes have no effect – either economically or statistically – on public firms' leverage. Private firms, on the other hand, respond significantly to shocks to bank credit supply. A one-percentage-point increase (cut) in bank taxes in column 5 is associated with a 0.4 percentage point reduction (increase) in leverage (p=0.004). Column 6 shows that this effect is driven by smaller private firms, particularly those in the two smallest size quartiles; larger private firms behave more like the average public firm in column 4. Columns 7 through 9 model log long-term debt instead, with qualitatively similar, albeit somewhat noisier, results.

The results in Table 4 suggest that changes in a state's bank taxes induce significant shifts in the debt supply curve firms in that state face and that these changes can be exploited to identify variation in financial constraints across firms.

3.3.4 Comparing the behavior of 'constrained' and 'unconstrained' firms

Table 5 reports the results of using Test 2 to examine if public firms classified as constrained or unconstrained by the five financial constraints measures behave in a way that is consistent with their classification. Specifically, we expect a negative effect of bank tax changes on the debt holdings of financially constrained firms and no effect among financially unconstrained firms. Panels A and B model long-term book leverage and log long-term debt, respectively.

Two results stand out. First, we find no evidence that supposedly constrained public firms borrow less when bank taxes go up or borrow more when bank taxes fall. Thus, unlike the private firms analyzed in Table 4 to establish the power of Test 2, the average public firm classified as financially constrained does not behave in a way that suggests it actually is financially constrained. Second, if anything, we find some evidence that supposedly *un*constrained firms behave in a constrained fashion: the debt holdings of 'unconstrained' dividend payers are at least marginally statistically more sensitive to bank tax changes than the debt holdings of non-dividends payers; and the HP Index similarly appears to misclassify firms.

Overall, we find no evidence suggesting that the firms the literature would classify as financially constrained react to shifts in their local debt supply curves. Taken together with our findings in Test 1, these results cast doubt on the notion that these firms are in fact financially constrained, in the sense of facing an inelastic supply of debt curve.

3.4 Test 3: Equity Recycling

Neither Test 1 nor Test 2 can identify firms that are constrained in the equity markets. Of course, a firm that has access to the debt markets cannot meaningfully be called financially constrained, whether or not it has easy access to the equity markets. In that sense, Tests 1 and 2 already suffice to show that existing measures of financial constraints do not correctly identify firms that are financially constrained, as we have found their debt supply curves to be no steeper than those of supposedly unconstrained firms.

Nevertheless, it would be useful to have a test that can identify equity constraints, so that future researchers can evaluate other candidate financial constraints measures comprehensively: to be classified as financially constrained, a firm would have to act as if it faced both an inelastic debt supply curve and an inelastic equity supply curve. To this end, we propose a third test using an alternative identification strategy to indirectly estimate the shape of the equity supply curve.

Test 3 is motivated by the findings of Farre-Mensa, Michaely, and Schmalz (2013), who show that over the 1989-2012 period, 48.4% of the proceeds of public U.S. firms' equity issues were paid out again (via dividends or share repurchases) *during the same year*, a practice they call "equity recycling." The identifying assumption behind Test 3 is that we should not observe equity recycling among firms facing an inelastic supply of equity curve:[26] equity recycling, by revealed preference, is an indicator that a firm is not concerned about its ability to raise equity

[26] Farre-Mensa et al. explore reasons why firms may find it optimal to recycle equity. Whatever the reason, the only assumption for Test 3 to be valid is that equity recycling is suboptimal for a financially constrained firm.

and so is plausibly unconstrained.[27]

3.4.1 Empirical specification

Our analysis of what firms do with the proceeds of their equity issues adapts the framework proposed by Kim and Weisbach (2008) to the empirical strategy of Tests 1 and 2. Specifically, we use OLS in first-differences to estimate the following equation:

$$\Delta P_{ijt} = \beta \Delta Equity\ Issue_{ijt} + \delta\ \Delta Other\ Sources\ of\ Funds_{ijt} + \gamma \Delta Size_{ijt} + \alpha_{jt} + \varepsilon_{ijt} \qquad (3)$$

where *i* indexes firms, *j* industries, and *t* fiscal years. The dependent variable, *P* for payout, can be measured either as total payouts, adding up dividends and repurchases, or as dividends only. The variable of interest, *Equity Issue*, captures a firm's proceeds from SEOs, private stock placements, stock option exercises, and employee stock ownership plans. *Other Sources of Funds* captures operating cash flows, debt issues net of debt repurchases, and the proceeds from asset sales. We also control for firm size and include industry-year fixed effects. All variables (except for size) are scaled by beginning-of-year total assets.

3.4.2 Power

To establish power, we turn once more to our comparison of the behavior of public and private firms. To the extent that private firms are more likely to face constraints in their ability to raise equity, we expect them not to engage in equity recycling.

Table 6 confirms this prediction. Columns 1 and 2 show that for public firms, increases in equity issuance proceeds are associated with highly significant increases in total payouts (dividends plus repurchases, column 1) and in dividends (column 2). In other words, the average public firm appears to "recycle equity," suggesting it is not financially constrained. Column 3, on the other hand, shows that private firms tend to *cut* their dividends at the same time they issue equity.[28] A plausible interpretation is that private firms use a combination of equity issues and

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[27] See Babenko, Lemmon, and Tserlukevich (2011) for a similar argument in the context of cash inflows from employee stock option exercises.
[28] Sageworks does not report data on share repurchases, so we cannot analyze total payouts for private firms.

reductions in payouts to fund increases in their investment or operating needs. The point estimate suggests that the average private firm reduces its dividend by 0.31 percentage points of total assets for every 10 percentage-point increase in its equity-issuance-to-assets ratio. Column 4 shows that this effect is largest (in absolute value) for the smallest private firms and becomes monotonically smaller (though remaining significant) for larger firms.

Absence of equity recycling is a necessary, but not a sufficient, condition for financial constraints. Thus, the fact that private firms do not recycle equity does not imply that they are necessarily financially constrained. The presence of equity recycling among public firms, on the other hand, violates the necessary condition and so implies that public firms are not constrained on average.

3.4.3 Comparing the behavior of 'constrained' and 'unconstrained' firms

Table 7 reports the results of examining whether or not firms the literature classifies as financially constrained engage in equity recycling. For brevity, we focus on total payouts; results are economically unchanged if we model dividends only instead.

Two results stand out. First, across all five constraints measures, we consistently find that supposedly constrained firms do recycle their equity proceeds. Second, for only two of the five measures do 'constrained' firms recycle less than 'unconstrained' firms: the dividend measure and the KZ index. Unsurprisingly, dividend payers tend to distribute a significantly larger share of their equity proceeds than non-dividend payers. Overall, firms classified as constrained by each of the five measures engage in "equity recycling" behavior, which is hard to reconcile with the notion that they are constrained in their ability to raise equity.

3.5 Validating The Methodological Approach: The Case of Junk Bond Issuers

The results of our three tests paint a consistent picture: the behavior of firms the literature classifies as financially constrained does not appear to differ systematically from the behavior of firms typically classified as unconstrained. In particular, the average 'constrained' firm (just like the average ‘unconstrained’ one) is able to

- borrow more when its demand for debt increases exogenously (Test 1);
- maintain borrowing levels when banks lending in its home state are hit with a tax shock that demonstrably affects their loan supply (Test 2); and
- use a significant part of the proceeds of their equity issues to increase their payouts to shareholders (Test 3).

In sharp contrast, none of our tests can rule out the hypothesis that private firms (particularly the smaller ones) are indeed financially constrained. Taken together, these findings run counter to the notion that firms commonly classified in the literature as financially constrained face an inelastic supply of capital curve and so are indeed constrained.

As a final validation of our methodological approach, we apply our tests to a subsample of public firms that are a priori likely to face inelastic capital supply curves and so are plausibly financially constrained: junk bond issuers. Table 8 reports the results.

Columns 1 and 2 show that junk bond issuers fail Test 1: the tax sensitivity of debt among junk bond issuers is not statistically different from zero, for either leverage or log debt. This is consistent with junk bond issuers facing an inelastic debt supply curve on the margin. Reinforcing this conclusion, columns 3 and 4 show that junk bond issuers reduce their debt holdings significantly when bank lending in their home state is hit with a tax increase and vice versa. This finding is consistent with the premise of Test 2 that the debt holdings of constrained firms should be sensitive to shifts in their debt supply curve. Finally, unlike public firms in general and those the literature traditionally classifies as constrained, junk bond issuers do *not* engage in equity recycling. This is consistent with the premise of Test 3, as we should observe no equity recycling among constrained firms.

The results in Table 8 help alleviate the concern that we fail to find support for traditional measures of financial constraints simply because our tests lack the power to identify financial constraints among *any* subsample of (public) firms. Indeed, the results suggest that public firms with a below-investment grade credit rating face, on average, an inelastic supply of capital curve and thus are financially constrained.

4. What do Traditional Measures of Financial Constraints Actually Measure?

The evidence presented in Section 3 suggests that firms classified as ‘constrained’ or ‘unconstrained’ by the five measures we examine do not actually differ, on average, in their ability to raise debt or equity capital. Does this mean there are no meaningful differences between these groups of firms? The fact that the empirical literature documents plenty of differences in behavior suggests that tests based on these measures do pick up important differences in firm types – just not, according to our tests, in financial constraints.[29]

The summary statistics and cross-tabulations in Table 1 suggest that public firms classified as ‘constrained’ by the dividends, ratings, HP, and WW measures look very different from ‘unconstrained’ firms (the KZ index is more of an outlier): ‘Constrained’ firms tend to be younger, smaller, less profitable, and less leveraged than ‘unconstrained’ firms, but they also grow faster and invest more, particularly in R&D. In this section, we investigate differences in funding sources between ‘constrained’ and ‘unconstrained’ firms. As we will see, the five measures produce sample splits that differ markedly in terms of funding patterns.

Table 9 shows the frequency with which ‘constrained’ and ‘unconstrained’ public firms issue equity, sell bonds, or take out a loan. This reveals three important differences. First, ‘constrained’ firms are substantially more likely to fund themselves by issuing equity than are ‘unconstrained’ firms. For instance, 9.3% of non-dividend-payers raise equity from outside investors in a given year, while only 4.3% of dividend-paying firms do so (a difference that is significant at the 1% level). Firms classified as constrained by the KZ, HP, and WW indices similarly raise equity more frequently than ‘unconstrained’ firms. (The only exceptions are non-

[29] For instance, Giroud and Mueller (2013) show that ‘constrained’ firms (but not ‘unconstrained’ ones) reallocate capital and labor from less to more productive divisions.

rated and rated firms, which both raise equity with almost exactly the same frequency.)

Second, 'constrained' firms rely much less heavily on bond issues than 'unconstrained' firms (except for the KZ index). For example, 21.3% of 'unconstrained' firms according to the HP index issue bonds in a given year, whereas 'constrained' firms very rarely do so (1.3%).[30] This difference is mostly driven by issues of *public* bonds, which are rare among 'constrained' firms, but it persists if we focus on bonds issued under rule 144A or placed privately.[31]

Third, while 'constrained' firms do not use the bond markets much, they do regularly access the syndicated loan market: the fraction of 'constrained' firms obtaining a loan in a given year ranges from 9.8% for the HP index to 27.6% for the dividends measure. This is consistent with our finding in Test 1 that 'constrained' firms are able to borrow more when their demand for debt increases due to exogenous increases in state corporate income taxes. Apparently, much of this extra borrowing comes from loans rather than bonds.[32]

Taken together, the results in Tables 1 and 9 suggest that the five financial constraints measures we examine do *not* generate a random partition of the universe of public firms. Rather (with the exception of the KZ index), they tend to identify as 'constrained' firms that are younger, smaller, and faster growing than 'unconstrained' firms. However, our three tests do not support the hypothesis that these firms face inelastic supply curves of debt or equity capital, and Table 9 confirms that these firms regularly access the public equity and bank-loan markets (though not the bond market). A plausible reading of the evidence is that the dividends, ratings, HP, and WW measures identify as 'constrained' public firms that find themselves in the growth phase of their lifecycle. However, while these firms differ markedly from the more mature 'unconstrained' firms, they do not appear to be restricted in their ability to finance this growth, at

[30] As in much of Table 1, the KZ index departs from this pattern: here, 'constrained' firms are *more* likely to issue bonds than 'unconstrained' firms (12.1% vs. 7.2%).
[31] In particular, consistent with Faulkender and Petersen's (2006) assumption that firms without a credit rating have no access to the public debt market, we find that a negligible 0.2% of unrated firms issue public debt in a given year.
[32] This rules out the possibility that the lack of response to shocks to the local credit supply among 'constrained' firms in Test 2 reflects such firms not having access to bank loans in the first place.

least not during the average year in our sample period.

5. Conclusions

Much empirical research in corporate finance proxies for financial constraints using measures that capture what firms say, do, or look like. We evaluate how well such measures identify firms that are financially constrained, using three novel tests that help identify the elasticity of a firm's supply of capital curve: an exogenous increase in a firm's demand for credit; exogenous variation in the supply of bank loans; and the tendency for firms to pay out the proceeds of equity issues to their shareholders ("equity recycling").

We find that none of the five measures we evaluate is able to identify firms that behave as if they were in fact constrained. Specifically, public firms classified as constrained for not paying dividends or not having debt or according to the KZ, HP, or WW indices appear to have no trouble raising debt when their demand for debt increases exogenously; are unaffected by changes in the supply of bank loans; and engage in equity recycling. Furthermore, they differ little in these respects from supposedly unconstrained firms, even though they are much smaller and younger, grow considerably faster, and rarely access either the public or the private bond market. But they appear to have ready access to both the equity markets and bank lending, which appear to supply capital to them when they need it.

Our results imply that popular measures of financial constraints identify as constrained subsets of firms that differ from the general firm population on a number of dimensions, but not in their ability to raise external funding. This suggests that extant findings that have been attributed to financial constraints are more likely to be caused by some other difference in firm characteristics, such as size, age, growth rates, or preferred funding source.

While we have no reason to doubt that the firms Kaplan and Zingales (1997), Hadlock and Pierce (2010), and Whited and Wu (2006) originally identify as constrained *in their respective samples* truly were financially constrained, our results make us skeptical of the popular practice to use the coefficients from these three studies to extrapolate to other samples and time periods in an effort to identify potentially constrained firms. As regards the other two measures, we note that paying a dividend or obtaining a credit rating are choices firms make endogenously and so may be more reflective of the firm's lifecycle than its ability to raise external funding.[33]

So which firms *are* financially constrained? Unfortunately, our methodological approach can only be used to test whether a particular measure identifies firms that are plausibly financially constrained – not to construct an alternative measure of financial constraints. The reason is that our tests identify behavior that is necessary but not sufficient for a firm to be classified as constrained. As a result, we cannot use the tests to unequivocally identify which firms are financially constrained and which are not. Having said that, when applied to two groups of firms that are plausibly financially constrained – small privately held firms and public firms with below investment-grade ratings – our tests are able to identify behavior that is consistent with our prior that these firms are indeed financially constrained.

[33] For example, it is hard to believe that Microsoft was financially constrained before paying its first dividend in 2003 or that Apple was constrained before obtaining its first bond rating since 2004 in connection with its $17 billion bond issue in 2013, the largest corporate-bond deal in history.

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Appendix A. Variable Definitions.

Company-level variables

Age is years since founding. We hand-collect founding dates from regulatory filings, business directories, and a comprehensive search of online and offline sources.

Total real assets is defined as the book value of assets (Compustat item *at*) in year 2005 real dollars.

Cash /assets is defined as Compustat items *che/at*.

Tangibility is defined as net property, plant, and equipment (Compustat item *ppent*, or Sageworks *NetFixedAssets*), over total assets.

ROA (return on assets) is defined as operating income before depreciation (Compustat item *oibdp* or its Sageworks equivalent, *Sales – CostOfSales – Payroll – Rent – Advertising – Overhead + OtherOperatingIncome – OtherOperatingExpenses*) over total assets.

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

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

Book long-term leverage is defined as long-term debt (Compustat item *dltt*) over total assets (Compustat item *at*). For private firms, it is defined as Sageworks *SeniorDebt + SubordinatedDebt* over *TotalAssets*.

% short-term debt (1 year) is defined as Compustat items *dlc /* (*dlc + dltt*).

Any long-term debt? is an indicator set equal to one if Compustat item *dltt* is strictly positive, and zero otherwise.

Investment opportunities is measured, for public firms, using a firm's market to book ratio, 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*]. For private firms, we use the industry market to book ratio, estimated separately for each four-digit NAICS industry and each year.

Sales growth is the annual percentage increase in sales: $Sales_{it}/Sales_{it-1} - 1$ (using Compustat item *sale*).

Employment growth is the annual percentage increase in employment: $Employees_{it}/Employees_{it-1} - 1$ (using Compustat item *emp*).

Gross investment is the annual increase in gross fixed assets (Compustat data item *ppegt*) scaled by beginning-of-year total assets.

R&D is defined as Compustat item *xrd* over beginning-of-year total assets.

Geographic concentration measures the geographic concentration in a firm's operations and hence in its tax base across states. To capture where a firm operates, Garcia and Norli (2012) count the number of times a firm mentions each state it operates in in the Management Discussion and Analysis section of its 10-K filing. They then scale by the total number of mentions to obtain a proxy for the fraction of a firm's operations that are associated with each state. We measure geographic concentration as the sum of the squared state-by-state fractions within firm and fiscal year (following the way the Herfindahl index is used to measure industry concentration). For example, a "single-state" firm has concentration of one, as all its operations are in a single state, while a firm whose operations are evenly distributed across all 50 states has concentration of 1/50.

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

Fim size is defined as the natural logarithm of the book value of assets (Compustat item *at*, or Sageworks *TotalAssets*) in year 2005 real dollars.

Dividends is defined as Compustat item *dv* for public firms and Sageworks item *Dividends* for private firms, scaled by beginning of year total assets.

Dividends and repurchases is defined as the sum of Compustat items *dv* + *prstkc*, scaled by beginning of year total assets.

Equity issuance proceeds is measured as Compustat item *sstk* for public firms and the change in Sageworks items *TotalEquity – RetainedEarnings* (set to 0 if negative) for private firms.

Other sources of funds is defined, for public firms, as the sum of Compustat items *dltis + ibc* + *dpc* + *sppe* + *siv*, scaled by beginning of year total assets. For private firms, it is the sum of the change in long-term debt (Sageworks *SeniorDebt* + *SubordinatedDebt*, set to 0 if negative) + *NetIncome + Depreciation* + *Amortization* – change in *GrossFixedAssets* (set to 0 if negative), also scaled by beginning of year total assets.

Log total assets is defined as the natural logarithm of beginning-of-year total assets (Compustat item *at*, or Sageworks *TotalAssets*) in year 2005 real dollars.

Junk bond issuers are firms which in year *t* have a below investment-grade rating from S&P, Moody's, or Fitch, using data obtained from Compustat (variable *splticrm*) and Mergent FISD.

Financial constraints measures

Non-dividend payers are firms with a history of zero dividends on common stock (Compustat item *dvc*), going as far back as 1970.

Dividend payers are firms with a history of non-zero dividends on either common stock (Compustat item *dvc*), going as far back as 1970.

Non-rated firms are those that do not have a credit rating from S&P, Moody's, Fitch, or Duff & Phelps, using data obtained from Compustat (variable *splticrm*) and Mergent FISD.

Rated firms are those that have a credit rating from S&P, Moody's, Fitch, or Duff & Phelps, using data obtained from Compustat (variable *splticrm*) and Mergent FISD.

KZ Index is constructed following Lamont, Polk, and Saa-Requejo (2001) as –1.001909[(*ib* + *dp*)/lagged *ppent*] + 0.2826389[(*at* + *prcc_f*×*csho* - *ceq* - *txdb*)/*at*] + 3.139193[(*dltt* + *dlc*)/(*dltt* + *dlc* + *seq*)] – 39.3678[(*dvc* + *dvp*)/lagged *ppent*] – 1.314759[*che*/lagged *ppent*], where all variables in italics are Compustat data items. Following convention, firms are sorted into terciles based on their index values in the previous year. Firms in the top tercile are coded as constrained and those in bottom tercile are coded as unconstrained.

WW Index is constructed following Whited and Wu (2006) and Hennessy and Whited (2007) as –0.091 [(*ib* + *dp*)/*at*] – 0.062[indicator set to one if *dvc* + *dvp* is positive, and zero otherwise] + 0.021[*dltt/at*] – 0.044[log(*at*)] + 0.102[average industry sales growth, estimated separately for each three-digit SIC industry and each year, with sales growth defined as above] – 0.035[sales growth], where all variables in italics are Compustat data items. Following convention, firms are sorted into terciles based on their index values in the previous year. Firms in the top tercile are coded as constrained and those in bottom tercile are coded as unconstrained.

HP Index is constructed following Hadlock and Pierce (2010) as –0.737Size + 0.043Size^2 – 0.040Age, where Size equals the log of inflation-adjusted Compustat item *at* (in 2004 dollars), and Age is the number of years the firm is listed with a non-missing stock price on Compustat. In calculating the index, we follow Hadlock and Pierce and cap Size at (the log of) $4.5 billion and Age at 37 years. Following convention, firms are sorted into terciles based on their index values in the previous year. Firms in the top tercile are coded as constrained and those in bottom tercile are coded as unconstrained.

Bank-level variables

Log commercial and industrial loans is defined as the natural logarithm of one plus *rcon1766* from the Federal Reserve's bank call reports, deflated to 2005 dollars using the GDP deflator. Call report data for 1976 to 2010 are available at http://www.chicagofed.org/webpages/banking/financial_institution_reports/commercial_bank_data.cfm. Data for 2011 and beyond are available at https://cdr.ffiec.gov/public/. We filter out banks involved in a merger and foreign filers. (We thank Kristy Agostino from the Chicago Fed for providing us with the merger data.) To annualize the quarterly Call report data, we focus on observations from the fourth quarter.

Log deposits is defined as the natural logarithm of one plus *rcon2200* from the Federal Reserve's bank call reports, deflated to 2005 dollars using the GDP deflator.

Log total assets is defined as the natural logarithm of one plus *rcon2170* from the Federal Reserve's bank call reports, deflated to 2005 dollars using the GDP deflator.

State-level variables

Corporate income tax increase is an indicator set equal to one if the state increased its corporate income tax rate, and zero otherwise. In states with more than one tax bracket, we focus on changes to the top bracket. See Appendix A in Heider and Ljungqvist (2013) for a complete list corporate income tax increases.

Bank tax change equals the change (in percentage points) in the state income tax rate affecting banks and other financial institutions operating in the state. See Appendix B for a complete list of state bank tax increases and cuts, respectively.

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.

Appendix B. List of Changes in State Bank Taxes.

This table lists all changes in state income tax rates affecting banks and other financial institutions over the tax years 1989-2011. In states with more than one tax bracket, we report the change in 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
CO	1989	Cut in top rate from 5.5% to 5.4%
IL	1989	Increase in top rate from 4% to 4.8%
NJ	1989	Introduction of a 0.375% tax rate surcharge
WV	1989	Cut in top rate from 9.75% to 9.6%
AZ	1990	Cut in top rate from 10.5% to 9.3%
CO	1990	Cut in top rate from 5.4% to 5.3%
CT	1990	Introduction of a 20% tax surcharge
MN	1990	Increase in top rate from 9.5% to 9.8%
MT	1990	Introduction of a 5% surcharge
NE	1990	Increase in top rate from 3.25% to 3.53%
NY	1990	Introduction of a 15% surcharge
OK	1990	Increase in top rate from 5% to 6%
WV	1990	Cut in top rate from 9.45% to 9.3%
AR	1991	Increase in top rate from 6% to 6.5%
CO	1991	Cut in top rate from 5.3% to 5.2%
MT	1991	Repeal of 5% surcharge
NE	1991	Increase in top rate from 3.53% to 3.81%
WV	1991	Cut in top rate from 9.3% to 9.15%
CO	1992	Cut in top rate from 5.2% to 5.1%
CT	1992	Cut in tax surcharge from 20% to 10%
DC	1992	Introduction of a 2.5% surcharge on tax liability
MT	1992	Re-introduction of tax surcharge on tax liability at 2.3% rate
NY	1992	Cut in tax surcharge from 15% 10%
WV	1992	Cut in top rate from 9.15% to 9%
CO	1993	Cut in top rate from 5.1% to 5.0%
CT	1993	Repeal of 10% tax surcharge
MT	1993	Increase in tax surcharge on tax liability from 2.3% to 4.7%
NH	1993	Cut in top rate from 8% to 7.5%
NY	1993	Repeal of 10% tax surcharge
AZ	1994	Cut in top rate from 9.3% to 9%
DC	1994	Introduction of additional 2.5% tax surcharge on tax liability
HI	1994	Cut in top rate from 11.7% to 7.92%
MT	1994	Repeal of 4.7% tax surcharge
NH	1994	Cut in top rate from 7.5% to 7%
NJ	1994	Repeal of 0.375% tax surcharge
CA	1995	Cut in top rate from 11.47% to 11.3%
CT	1995	Cut in top rate from 11.5% to 11.25%
DC	1995	Cut in top rate from 10% to 9.5% (+2 tax surcharges at 2.5% each)
MA	1995	Cut in top rate from 12.54% to 12.13%
CT	1996	Cut in top rate from 11.25% to 10.75%
MA	1996	Cut in top rate from 12.13% to 11.72%
RI	1996	Increase in top rate from 8% to 9%
CA	1997	Cut in top rate from 11.3% to 10.84%
CT	1997	Cut in top rate from 10.75% to 10.5%
MA	1997	Cut in top rate from 11.72% to 11.32%
NC	1997	Cut in top rate from 7.75% to 7.5%

AZ	1998	Cut in top rate from 9% to 8%
CT	1998	Cut in top rate from 10.5% to 9.5%
MA	1998	Cut in top rate from 11.32% to 10.91%
NC	1998	Cut in top rate from 7.5% to 7.25%
CO	1999	Cut in top rate from 5% to 4.75%
CT	1999	Cut in top rate from 9.5% to 8.5%
KS	1999	Cut in top rate from 4.25% to 2.25%
MA	1999	Cut in top rate from 10.91% to 10.5%
NC	1999	Cut in top rate from 7.25% to 7%
NH	1999	Increase in top rate from 7% to 8%
AZ	2000	Cut in top rate from 8% to 7.968%
CO	2000	Cut in top rate from 4.75% to 4.63%
CT	2000	Cut in top rate from 8.5% to 7.5%
NC	2000	Cut in top rate from 7% to 6.9%
AL	2001	Increase in top rate from 6% to 6.5%
AZ	2001	Cut in top rate from 7.968% to 6.968%
ID	2001	Cut in top rate from 8% to 7.6%
NH	2001	Increase in top rate from 8% to 8.5%
NY	2001	Cut in top rate from 9% to 8.5%
NY	2002	Cut in top rate from 8.5% to 8%
TN	2002	Increase in top rate from 6% to 6.5%
AR	2003	Introduction of 3% tax surcharge on tax liability
CT	2003	Introduction of 20% tax surcharge on tax liability
NY	2003	Cut in top rate from 8% to 7.5%
CT	2004	Increase in surcharge to 25%
AR	2005	Repeal of 3% tax surcharge on tax liability
CT	2006	Cut in tax surcharge from 25% to 20%
NJ	2006	Introduction of 4% tax surcharge on tax liability
NY	2007	Cut in top rate from 7.5% to 7.1%
WV	2007	Cut in top rate from 9% to 8.75%
CT	2008	Repeal of 20% tax surcharge
MD	2008	Increase in top rate from 7% to 8.25%
NC	2009	Introduction of 3% tax surcharge on tax liability
OR	2009	Increase in top rate from 6.6% to 7.9%
WV	2009	Cut in top rate from 8.75% to 8.5%
MA	2010	Cut in top rate from 10.5% to 10%
NJ	2010	Repeal of 4% tax surcharge
IL	2011	Increase in top rate from 7.3% to 9.5%
MA	2011	Cut in top rate from 10% to 9.5%
NC	2011	Repeal of 3% tax surcharge
ND	2011	Cut in top rate from 7% to 6.5%
OR	2011	Cut in top rate from 7.9% to 7.6%

Table 1, Panel A.
Summary Statistics of 'Constrained' and 'Unconstrained' Firms.

The table reports summary statistics for 91,487 non-financial and non-utility public U.S. companies between 1989 and 2011 that are classified as 'constrained' and 'unconstrained' by five popular measures of financial constraints. For variable definitions and details of their construction, see Appendix A. All pairwise differences in means or fractions are significant at the 1% level except those shown in square brackets.

		Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
		non-div. payer	dividend payer	non-rated	rated	con-strained	uncon-strained	con-strained	uncon-strained	con-strained	uncon-strained
Age (since founding)	mean	21.0	57.2	29.4	58.1	[35.7	35.9]	19.3	64.5	22.6	60.0
	st.dev.	*19.2*	*40.1*	*27.9*	*43.7*	*32.6*	*35.2*	*16.6*	*40.0*	*18.6*	*42.8*
Total real assets, $m	mean	502.9	3,694.7	271.9	6,499.3	[1,732.3	1,665.8]	53.6	5,318.3	71.8	5,679.0
	st.dev.	*2,692.1*	*15,172.9*	*856.6*	*19,479.3*	*10,846.5*	*8,143.3*	*90.9*	*18,029.3*	*476.8*	*18,111.1*
Cash/assets	mean	0.263	0.115	0.232	0.102	0.082	0.336	0.289	0.106	0.262	0.110
	st.dev.	*0.264*	*0.146*	*0.249*	*0.134*	*0.135*	*0.262*	*0.272*	*0.133*	*0.260*	*0.146*
Tangibility	mean	0.235	0.309	0.245	0.331	0.381	0.140	0.207	0.320	0.208	0.327
	st.dev.	*0.222*	*0.220*	*0.216*	*0.232*	*0.254*	*0.128*	*0.211*	*0.220*	*0.206*	*0.222*
ROA	mean	-0.030	0.127	0.009	0.125	0.015	0.033	-0.096	0.134	-0.085	0.136
	st.dev.	*0.323*	*0.121*	*0.299*	*0.100*	*0.290*	*0.275*	*0.369*	*0.090*	*0.338*	*0.119*
Profitable?	fraction	0.650	0.927	0.711	0.941	0.762	0.735	0.537	0.953	0.541	0.954
Total book leverage	mean	0.199	0.237	0.175	0.329	0.353	0.115	0.155	0.253	0.173	0.255
	st.dev.	*0.216*	*0.189*	*0.192*	*0.198*	*0.209*	*0.162*	*0.190*	*0.183*	*0.199*	*0.185*
Book long-term leverage	mean	0.145	0.193	0.121	0.292	0.277	0.085	0.094	0.213	0.108	0.219
	st.dev.	*0.193*	*0.175*	*0.162*	*0.196*	*0.211*	*0.142*	*0.151*	*0.174*	*0.164*	*0.176*
% short-term debt (1 year)	mean	0.367	0.240	0.378	0.145	0.263	0.376	0.468	0.200	0.453	0.191
	st.dev.	*0.355*	*0.291*	*0.350*	*0.215*	*0.319*	*0.357*	*0.358*	*0.261*	*0.359*	*0.255*
Any long-term debt?	fraction	0.729	0.852	0.720	0.960	0.913	0.603	0.641	0.905	0.671	0.906
Investment opportunities	mean	2.167	1.399	1.988	1.379	1.581	2.264	2.366	1.382	2.071	1.559
	st.dev.	*2.313*	*1.180*	*2.112*	*1.128*	*1.975*	*2.061*	*2.514*	*1.106*	*2.277*	*1.383*
Sales growth	mean	0.325	0.108	0.261	0.140	0.182	0.236	0.304	0.092	0.251	0.133
	st.dev.	*1.071*	*0.420*	*0.948*	*0.496*	*0.797*	*0.876*	*1.136*	*0.325*	*1.066*	*0.453*
Employment growth	mean	0.158	0.061	0.129	0.075	0.057	0.138	0.133	0.044	0.094	0.071
	st.dev.	*0.569*	*0.343*	*0.520*	*0.367*	*0.460*	*0.489*	*0.590*	*0.279*	*0.555*	*0.323*
Gross investment	mean	0.070	0.053	[0.062	0.063]	0.060	0.044	0.055	0.048	0.042	0.060
	st.dev.	*0.181*	*0.142*	*0.166*	*0.163*	*0.195*	*0.110*	*0.182*	*0.128*	*0.175*	*0.136*
R&D	mean	0.096	0.021	0.077	0.023	0.047	0.093	0.120	0.024	0.110	0.025
	st.dev.	*0.156*	*0.050*	*0.141*	*0.054*	*0.133*	*0.138*	*0.182*	*0.049*	*0.169*	*0.062*
Geographic concentration	mean	0.430	0.362	0.433	0.320	0.374	0.439	0.485	0.335	0.486	0.327
	st.dev.	*0.236*	*0.235*	*0.240*	*0.214*	*0.228*	*0.241*	*0.240*	*0.222*	*0.240*	*0.220*
No. of firm-years		46,970	36,935	62,092	22,174	21,895	23,926	22,938	25,475	22,514	25,231

Table 1, Panel B.
Cross-tabulations of Financial Constraints Measures.

The table reports cross-tabulations of the five financial constraints measures to illustrate the extent to which the measures produce overlapping classifications. The first five rows show the fraction of firms classified as constrained by each measure that would also be classified as constrained under each of the other four measures. The last five rows report the fraction of firms classified as unconstrained by each measure that would be classified as constrained under the other measures.

		Financial constraints measure				
		Dividends (1)	Credit ratings (2)	Kaplan-Zingales (3)	Hadlock-Pierce (4)	Whited-Wu (5)
Constrained firms	fraction no dividend	1.000	0.653	0.610	0.840	0.815
	fraction unrated	0.860	1.000	0.663	0.988	0.970
	fraction constrained KZ	0.509	0.428	1.000	0.445	0.525
	fraction constrained HP	0.802	0.685	0.521	1.000	0.956
	fraction constrained WW	0.780	0.689	0.576	0.975	1.000
Unconstrained firms	fraction no dividend	0.000	0.298	0.537	0.186	0.205
	fraction unrated	0.581	0.000	0.810	0.410	0.391
	fraction constrained KZ	0.435	0.619	0.000	0.472	0.426
	fraction constrained HP	0.150	0.017	0.549	0.000	0.023
	fraction constrained WW	0.172	0.043	0.478	0.040	0.000

Table 2.
Power of Test 1: Tax Sensitivity of Debt Holdings for Public and Private Firms.

We estimate standard capital structure regressions to test whether public and private firms increase their debt holdings in response to increases in state corporate income taxes in their headquarter state. The dependent variable is the change in long-term book leverage in columns 1 through 4 and the change in the logarithm of real long-term debt in columns 5 through 8. We follow a difference-in-differences approach, using all firm-years not affected by a tax increase as control group in columns 1 and 5, and restricting the control group to firm-years neighboring a state affected by a tax rise in all other columns. Since only firms that are (or expect to be) profitable benefit from tax shields and so have an incentive to increase debt as taxes increase, we screen out unprofitable firms. We also screen out private firms that are not C Corps and thus pay personal rather than corporate income taxes. For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS in first differences with Fama-French 48 industry-year fixed effects. 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				Change in log long-term debt			
	Public firms		Private firms		Public firms		Private firms	
	all	neighbors			all	neighbors		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
=1 if corporate tax increase at $t = -1$	0.011***	0.013***	0.004		0.086***	0.101***	0.062	
	0.002	*0.002*	*0.004*		*0.030*	*0.030*	*0.100*	
x quartile 1 (smallest)				-0.017				-0.042
				0.023				*0.366*
x quartile 2				0.006				-0.116
				0.006				*0.231*
x quartile 3				0.007				0.011
				0.006				*0.165*
x quartile 4 (largest)				0.010**				0.278*
				0.004				*0.160*
Lagged change in …								
ROA	-0.008**	-0.014	-0.001	-0.001	0.013	-0.024	0.081	0.082
	0.004	*0.017*	*0.003*	*0.003*	*0.031*	*0.092*	*0.084*	*0.084*
tangibility	0.030**	0.009	-0.015	-0.016	0.462***	0.397*	1.380***	1.379***
	0.012	*0.027*	*0.026*	*0.026*	*0.090*	*0.198*	*0.412*	*0.412*
firm size	0.009***	0.006	0.002	0.001	0.194***	0.151***	-0.025	-0.030
	0.002	*0.004*	*0.008*	*0.008*	*0.020*	*0.043*	*0.181*	*0.182*
investment opportunities	-0.001	-0.004***	0.009	0.009	0.007	-0.009	0.196	0.200
	0.001	*0.001*	*0.005*	*0.005*	*0.005*	*0.010*	*0.129*	*0.131*
Diagnostics								
R^2	3.1%	8.1%	5.2%	5.3%	2.8%	7.6%	6.8%	6.8%
No. of firms	7,230	3,979	4,400	4,400	7,230	3,979	4,400	4,400
No. of observations	58,017	8,670	4,713	4,713	58,017	8,670	4,713	4,713
No. of treated observations	1,522	1,522	940	940	1,522	1,522	940	940

Table 3, Panel A.
Test 1: Tax Sensitivity of Leverage By Financial Constraints Measure.

We estimate standard capital structure regressions to test whether ‘constrained’ firms increase their debt holdings in response to increases in state corporate income taxes in their headquarter state, and if so, whether the tax sensitivity of ‘constrained’ firms’ is lower than that of ‘unconstrained’ firms. Firms are categorized as ‘constrained’ or ‘unconstrained’ according to the five measures of financial constraints introduced in Table 1. We follow a difference-in-differences approach, restricting the control group to firm-years neighboring a state affected by a tax rise. Since only firms that are (or expect to be) profitable benefit from tax shields and so have an incentive to increase debt as taxes increase, we screen out unprofitable firms. The dependent variable in Panel A is the change in long-term book leverage. The dependent variable in Panel B is the change in the logarithm of real long-term debt. For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS in first differences with Fama-French 48 industry-year fixed effects. 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									
	Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
	non-dividend payer	dividend payer	unrated	rated	con-strained	uncon-strained	con-strained	uncon-strained	con-strained	uncon-strained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
=1 if tax increase at $t = -1$	0.017***	0.008**	0.012***	0.014***	0.021***	0.015***	0.021**	0.009*	0.012*	0.007**
	0.004	*0.003*	*0.003*	*0.005*	*0.007*	*0.005*	*0.008*	*0.005*	*0.007*	*0.003*
Lagged change in …										
ROA	-0.014	-0.011	-0.011	-0.090	-0.041	-0.003	-0.027	0.054	-0.020	-0.008
	0.021	*0.023*	*0.018*	*0.062*	*0.044*	*0.027*	*0.022*	*0.062*	*0.027*	*0.030*
tangibility	0.006	0.004	-0.007	0.087*	-0.026	0.019	-0.020	-0.014	-0.049	0.050*
	0.041	*0.026*	*0.034*	*0.051*	*0.045*	*0.040*	*0.040*	*0.024*	*0.030*	*0.029*
firm size	0.007	0.003	0.007	0.003	0.012	0.000	0.017	0.001	0.003	0.010
	0.006	*0.011*	*0.004*	*0.013*	*0.013*	*0.008*	*0.012*	*0.010*	*0.011*	*0.009*
investment opportunities	-0.003***	-0.005*	-0.004***	0.001	-0.011***	-0.001	-0.001	-0.009***	-0.003**	0.001
	0.001	*0.003*	*0.001*	*0.006*	*0.004*	*0.001*	*0.001*	*0.003*	*0.001*	*0.002*
Diagnostics										
R^2	15.2%	12.1%	9.3%	20.0%	23.7%	16.9%	21.6%	17.6%	22.6%	17.5%
Wald test: equal tax effect	3.49*		0.14		0.49		1.15		0.46	
No. of firms	2,076	2,050	2,861	1,327	1,525	1,538	1,125	1,471	1,153	1,620
No. of observations	3,656	4,998	5,692	2,978	2,391	2,771	1,695	3,745	1,756	3,692
No. of treated obs.	585	933	991	531	396	507	273	692	282	684

Table 3, Panel B.
Test 1: Tax Sensitivity of Log Debt By Financial Constraints Measure.

	Dep. var.: Change in log long-term debt									
	Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
	non-dividend payer	dividend payer	unrated	rated	con-strained	uncon-strained	con-strained	uncon-strained	con-strained	uncon-strained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
=1 if tax increase at $t = -1$	0.144**	0.074**	0.080**	0.146***	0.125***	0.094	0.097	0.074**	0.033	0.083
	0.062	*0.028*	*0.035*	*0.045*	*0.036*	*0.060*	*0.068*	*0.032*	*0.036*	*0.052*
Lagged change in …										
ROA	-0.008	-0.012	0.007	-0.595	0.012	0.024	-0.118	0.200	-0.069	0.252
	0.101	*0.197*	*0.092*	*0.514*	*0.249*	*0.120*	*0.095*	*0.364*	*0.113*	*0.200*
tangibility	0.580**	0.195	0.402*	0.759*	-0.030	0.848*	0.398	-0.029	0.033	0.719*
	0.283	*0.215*	*0.232*	*0.443*	*0.265*	*0.492*	*0.307*	*0.223*	*0.236*	*0.369*
firm size	0.148**	0.148	0.132***	0.252**	0.186***	0.161*	0.138**	-0.084	0.064	0.210**
	0.065	*0.088*	*0.043*	*0.123*	*0.059*	*0.096*	*0.062*	*0.106*	*0.076*	*0.097*
investment opportunities	-0.012	-0.012	-0.013	0.081	-0.005	0.009	0.003	-0.038	-0.010	0.011
	0.013	*0.021*	*0.008*	*0.074*	*0.028*	*0.021*	*0.012*	*0.036*	*0.011*	*0.046*
Diagnostics										
R^2	13.5%	12.0%	11.6%	16.1%	21.3%	16.4%	22.0%	16.3%	21.3%	14.2%
Wald test: equal tax effect	1.28		1.71		0.17		0.09		0.86	
No. of firms	2,076	2,050	2,861	1,327	1,525	1,538	1,125	1,471	1,153	1,620
No. of observations	3,656	4,998	5,692	2,978	2,391	2,771	1,695	3,745	1,756	3,692
No. of treated obs.	585	933	991	531	396	507	273	692	282	684

Table 4.
Power of Test 2: Tax Sensitivity of Banks' Loan Supply and of Public and Private Firms' Debt Holdings.

The first three columns use Call Report data from the Federal Reserve to test whether increases (cuts) in state bank taxes induce a decrease (increase) in commercial and industrial lending by banks headquartered in the affected state. Columns 4 through 9 report standard capital structure regressions to test whether public and private firms decrease (increase) their debt holdings in response to an inward (outward) shift in the supply of credit induced by an increase (cut) in state bank taxes in their headquarter state. In all columns, we follow a difference-in-differences approach, restricting the control group to observations neighboring a state affected by a tax change. For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS in first differences with year fixed effects in columns 1-3 and Fama-French 48 industry-year fixed effects in columns 4-9. 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.

Dep. var.:	Change in log commercial and industrial loans			Change in long-term book leverage			Change in log long-term debt		
				Public	Private firms		Public	Private firms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bank tax change (in %) at t = 0	-0.018***	-0.018***	-0.015***	-0.001	-0.004***		-0.020	-0.003	
	0.006	*0.005*	*0.005*	*0.001*	*0.001*		*0.013*	*0.004*	
x quartile 1 (smallest)						-0.005*			-0.006*
						0.002			*0.004*
x quartile 2						-0.006***			-0.009***
						0.002			*0.003*
x quartile 3						-0.004			-0.008
						0.003			*0.007*
x quartile 4 (largest)						-0.002			0.011
						0.002			*0.015*
Lagged change in …									
ROA				-0.006	0.001	0.001	-0.019	0.001	0.001
				0.274	*0.001*	*0.001*	*0.550*	*0.007*	*0.007*
tangibility				0.038**	-0.027	-0.027	0.351***	-0.004	-0.004
				0.027	*0.017*	*0.017*	*0.005*	*0.033*	*0.033*
firm size				0.007***	0.004	0.004	0.160***	0.022*	0.022*
				0.002	*0.004*	*0.004*	*0.000*	*0.012*	*0.012*
investment opportunities				0.000	-0.001	-0.001	0.013***	-0.010	-0.010
				0.413	*0.002*	*0.002*	*0.000*	*0.010*	*0.010*
log deposits		0.315***							
		0.058							
log total assets			0.575***						
			0.045						
Diagnostics									
R^2	7.3%	9.7%	12.1%	4.1%	2.3%	2.3%	4.1%	2.4%	2.4%
F test: bank tax change = 0	10.0***	14.5***	7.8***	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
No. of banks/firms	1,612	1,612	1,612	6,824	13,355	13,355	6,824	13,355	13,355
No. of observations	5,100	5,100	5,100	27,743	19,654	19,654	27,743	19,654	19,654
No. of treated obs.	1,289	1,289	1,289	8,339	3,591	3,591	8,339	3,591	3,591

Table 5, Panel A.
Test 2: Bank-tax Sensitivity of Leverage By Financial Constraints Measure.

We estimate standard capital structure regressions to test whether 'constrained' firms decrease (increase) their debt holdings in response to an inward (outward) shift in the supply of credit induced by an increase (cut) in state bank taxes in their headquarter state, and if so, whether this change in debt holdings is larger than that of 'unconstrained' firms. Firms are categorized as 'constrained' or 'unconstrained' according to the five measures of financial constraints introduced in Table 1. We follow a difference-in-differences approach, restricting the control group to firm-years neighboring a state affected by a tax change. The dependent variable in Panel A is the change in long-term book leverage. The dependent variable in Panel B is the change in the logarithm of real long-term debt. For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS in first differences with Fama-French 48 industry-year fixed effects. 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									
	Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
	non-dividend payer	dividend payer	unrated	rated	con-strained	uncon-strained	con-strained	uncon-strained	con-strained	uncon-strained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bank tax change (in %) at t =0	0.002	-0.003*	0.001	-0.005	0.005*	-0.001	0.000	-0.006***	-0.002	-0.004*
	0.002	*0.002*	*0.001*	*0.004*	*0.003*	*0.002*	*0.003*	*0.001*	*0.003*	*0.002*
Lagged change in …										
ROA	-0.008	-0.003	-0.003	-0.013	-0.018**	-0.003	-0.002	-0.009	-0.008	-0.009
	0.005	*0.022*	*0.005*	*0.050*	*0.009*	*0.007*	*0.006*	*0.030*	*0.006*	*0.013*
tangibility	0.047**	0.001	0.044**	0.029	0.081***	0.011	0.045**	0.022	0.022	0.079***
	0.019	*0.036*	*0.018*	*0.049*	*0.022*	*0.025*	*0.019*	*0.030*	*0.020*	*0.019*
firm size	0.010***	-0.004	0.004	0.020**	-0.001	0.004	0.005	0.006	0.005	0.003
	0.002	*0.005*	*0.002*	*0.008*	*0.005*	*0.003*	*0.003*	*0.005*	*0.004*	*0.004*
investment opportunities	0.000	-0.001	0.000	-0.003	-0.001	-0.001	0.000	-0.002	0.000	-0.003**
	0.001	*0.001*	*0.000*	*0.002*	*0.001*	*0.001*	*0.001*	*0.003*	*0.001*	*0.001*
Diagnostics										
R^2	6.2%	8.6%	4.8%	12.0%	11.6%	8.1%	10.2%	10.6%	10.6%	11.6%
Wald test: equal tax effect	2.81*		1.73		4.69**		2.74*		0.16	
No. of firms	4,536	2,572	5,615	1,675	3,327	3,325	3,307	1,860	3,305	2,367
No. of observations	15,644	11,946	20,542	7,201	7,655	9,681	9,080	9,037	9,042	8,754
No. of treated obs.	4,924	3,381	6,282	2,057	2,248	2,986	2,886	2,587	2,928	2,436

Table 5, Panel B.
Test 2: Bank-tax Sensitivity of Log Debt By Financial Constraints Measure.

	Dep. var.: Change in log long-term debt									
	Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
	non-dividend payer (1)	dividend payer (2)	unrated (3)	rated (4)	con-strained (5)	uncon-strained (6)	con-strained (7)	uncon-strained (8)	con-strained (9)	uncon-strained (10)
Bank tax change (in %) at *t* =0	0.001	-0.036**	-0.013	-0.046	0.024	-0.020	-0.014	-0.070***	-0.023	-0.030
	0.015	*0.015*	*0.015*	*0.040*	*0.021*	*0.028*	*0.021*	*0.022*	*0.022*	*0.023*
Lagged change in …										
ROA	-0.041	0.167	-0.019	0.462	-0.110**	-0.005	-0.024	0.731***	-0.048	0.164
	0.038	*0.109*	*0.028*	*0.280*	*0.051*	*0.038*	*0.024*	*0.225*	*0.035*	*0.141*
tangibility	0.374***	0.272	0.362***	0.502	0.506***	0.253	0.209***	0.571*	0.114	0.715**
	0.123	*0.238*	*0.095*	*0.480*	*0.136*	*0.197*	*0.067*	*0.288*	*0.084*	*0.272*
firm size	0.174***	0.107***	0.124***	0.315***	0.129***	0.100***	0.090***	0.150***	0.101***	0.104***
	0.021	*0.037*	*0.023*	*0.062*	*0.043*	*0.025*	*0.024*	*0.036*	*0.025*	*0.029*
investment opportunities	0.014***	-0.002	0.008***	0.044***	0.007	0.009**	0.012***	-0.038*	0.010**	-0.011
	0.003	*0.011*	*0.003*	*0.013*	*0.006*	*0.004*	*0.004*	*0.019*	*0.004*	*0.009*
Diagnostics										
R^2	6.1%	8.1%	5.6%	10.5%	13.2%	7.6%	11.5%	9.4%	10.1%	9.7%
Wald test: equal tax effect	3.97*		0.73		2.52		3.05*		0.04	
No. of firms	4,536	2,572	5,615	1,675	3,327	3,325	3,307	1,860	3,305	2,367
No. of observations	15,644	11,946	20,542	7,201	7,655	9,681	9,080	9,037	9,042	8,754
No. of treated obs.	4,924	3,381	6,282	2,057	2,248	2,986	2,886	2,587	2,928	2,436

Table 6.
Power of Test 3: Equity Recycling Among Public and Private Firms.

In columns 1 and 2, we test whether public firms use the proceeds of equity issues to increase their payouts, a practice we call "equity recycling." Columns 3 and 4 report similar analysis for private firms. In column 1, payouts are defined as the sum of dividends and share repurchases, while in columns 2 through 4 we focus on dividends only. (Share repurchase data are not available for private firms.) Our choice of control variables follows the analysis by Kim and Weisbach (2008). For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS in first differences with Fama-French 48 industry-year fixed effects. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dep. var.: Change in…	dividends & repurchases	dividends		
	Public firms		Private firms	
	(1)	(2)	(3)	(4)
Change in …				
equity issuance proceeds	0.009***	0.001***	-0.031***	
	0.001	*0.0002*	*0.004*	
x quartile 1 (smallest)				-0.042***
				0.006
x quartile 2				-0.022***
				0.005
x quartile 3				-0.017***
				0.003
x quartile 4 (largest)				-0.014***
				0.003
other sources of funds	0.011***	0.002***	0.042***	0.043***
	0.001	*0.0002*	*0.004*	*0.004*
log total assets	0.008***	-0.001**	0.001	0.001
	0.001	*0.0002*	*0.003*	*0.003*
Diagnostics				
R^2	3.7%	2.4%	5.7%	5.8%
No. of firms	8,807	8,838	98,562	98,566
No. of observations	71,283	72,380	207,592	207,589

Table 7.
Test 3: Equity Recycling By Financial Constraints Measure.

We compare the extent to which ‘constrained’ and ‘unconstrained’ firms use the proceeds of equity issues to increase their payouts, measured as the sum of dividends and share repurchases. Firms are categorized as ‘constrained’ or ‘unconstrained’ according to the five measures of financial constraints introduced in Table 1. Our choice of control variables follows the analysis by Kim and Weisbach (2008). For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS in first differences with Fama-French 48 industry-year fixed effects. Heteroskedasticity-consistent standard errors clustered at the firm level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Dep. var.: Change in dividends & repurchases									
	Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
	non-dividend payer	dividend payer	unrated	rated	con-strained	uncon-strained	con-strained	uncon-strained	con-strained	uncon-strained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Change in …										
equity issuance proceeds	0.008***	0.019***	0.009***	0.012***	0.008***	0.014***	0.009***	0.010**	0.010***	0.014***
	0.001	*0.004*	*0.001*	*0.004*	*0.001*	*0.002*	*0.001*	*0.005*	*0.001*	*0.003*
other sources of funds	0.008***	0.020***	0.009***	0.015***	0.007***	0.012***	0.007***	0.017***	0.006***	0.016***
	0.001	*0.002*	*0.001*	*0.002*	*0.001*	*0.002*	*0.001*	*0.002*	*0.001*	*0.002*
log total assets	0.006***	0.016***	0.008***	0.008***	0.003***	0.016***	0.007***	0.009***	0.008***	0.006***
	0.001	*0.002*	*0.001*	*0.002*	*0.001*	*0.002*	*0.001*	*0.002*	*0.001*	*0.002*
Diagnostics										
R^2	3.7%	6.3%	3.6%	8.7%	6.1%	7.0%	5.2%	7.5%	5.0%	8.4%
Wald test:										
equal equity issuance effect	6.50**		0.61		9.55***		0.09		2.37	
No. of firms	6,108	3,264	7,389	2,237	5,342	4,876	4,752	2,340	4,909	3,460
No. of observations	38,375	32,640	51,113	20,170	20,917	22,918	22,014	24,264	21,600	24,086

Table 8.
Applying Tests 1, 2, and 3 to Junk Bond Issuers.

The table shows the results of applying our three tests to a subsample of public firms that are a priori likely to face inelastic capital supply curves and so are plausibly financially constrained: junk bond issuers. For details on each of the tests, see Tables 2, 4, and 6. Heteroskedasticity-consistent standard errors clustered at the state level (columns 1 through 4) and at the firm level (column 5) are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Test 1		Test 2		Test 3
	Change in long-term book leverage (1)	Change in log long-term debt (2)	Change in long-term book leverage (3)	Change in log long-term debt (4)	Change in dividends & repurchases (5)
=1 if tax increase at t = –1	0.008	0.111			
	0.022	*0.240*			
Bank tax change (in %) at t =0			-0.008**	-0.237**	
			0.004	*0.091*	
Change in …					
equity issuance proceeds					0.007
					0.005
other sources of funds					0.014***
					0.003
log total assets					0.010***
					0.004
Lagged change in …					
ROA	-0.061	0.422	0.045	0.626	
	0.207	*1.413*	*0.051*	*0.671*	
tangibility	0.016	0.321	0.017	1.654**	
	0.196	*1.199*	*0.078*	*0.793*	
firm size	-0.001	0.067	0.024*	0.508**	
	0.033	*0.367*	*0.014*	*0.210*	
investment opportunities	-0.005	-0.039	-0.003	0.223***	
	0.023	*0.246*	*0.004*	*0.047*	
Diagnostics					
R^2	6.1%	2.5%	20.4%	27.8%	4.8%
No. of firms	842	842	1,182	1,182	1,779
No. of observations	1,465	1,465	3,432	3,432	10,165
No. of treated obs.	239	239	885	885	n.a.

Table 9.
Sources of Financing of ‘Constrained’ and ‘Unconstrained’ Firms.

The table shows the annual frequency of a given ‘constrained’ or ‘unconstrained’ firm issuing equity, selling bonds, or taking out a syndicated loan. Firms are categorized as ‘constrained’ or ‘unconstrained’ according to the five measures of financial constraints introduced in Table 1. The table combines data from SDC and Mergent FISD (which we match to Compustat by CUSIPs) and Dealscan (which we match to Compustat using Chava and Roberts’s (2008) Compustat-Dealscan link). All pairwise differences in frequencies are significant at the 5% level or better except those shown in square brackets.

	Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
	non-div. payer	dividend payer	non-rated	rated	con-strained	uncon-strained	con-strained	uncon-strained	con-strained	uncon-strained
Equity										
Common stock issues (primary)	0.093	0.043	[0.074	0.075]	0.094	0.064	0.091	0.049	0.083	0.062
excluding IPOs	0.093	0.042	[0.074	0.074]	0.094	0.064	0.091	0.048	0.083	0.061
Preferred stock issues	0.002	0.016	0.006	0.026	0.016	0.006	0.009	0.013	0.009	0.015
Bonds										
All bond issues	0.047	0.222	0.020	0.416	0.121	0.072	0.013	0.213	0.021	0.231
public bond issues	0.015	0.158	0.002	0.307	0.059	0.042	0.002	0.136	0.006	0.143
Rule 144A bonds	0.025	0.042	0.005	0.112	0.053	0.022	0.003	0.063	0.007	0.073
private placements	0.012	0.055	0.014	0.064	0.026	0.015	0.008	0.043	0.008	0.047
Loan issues										
All loans	0.276	0.161	0.151	0.475	0.255	0.139	0.098	0.358	0.106	0.378
term loans	[0.104	0.035]	0.047	0.117	0.080	0.029	0.036	0.083	0.038	0.091
revolvers / lines of credit	0.246	0.148	0.136	0.439	0.232	0.128	0.086	0.333	0.093	0.350
No. of firm-years	33,767	15,177	53,763	9,112	20,270	22,196	21,157	23,685	21,103	23,518

INTERNET APPENDIX

(NOT INTENDED FOR PUBLICATION)

Table IA.1.
Determinants of State Bank Tax Changes, 1990-2011.

We estimate the effect of political and economic conditions and of tax competition among states on the probability that a state changes the rate at which it taxes banks operating within its borders. Columns 1 to 3 report summary statistics of the explanatory variables, showing fractions or means (with standard deviations shown in italics underneath the means). Columns 4 and 5 model the probability that a state raises or cuts bank taxes, using linear probability models. For variable definitions and details of their construction, see Appendix A. The regression specifications are estimated using least squares with state and year fixed effects (not shown for brevity). The unit of observation in all columns is a state-year. The sample covers 50 states plus the District of Columbia over the period 1990-2011, for a total of 1,122 observations. In columns 4 and 5, 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			Probability of …	
	all tax changes (1)	tax increases (2)	tax cuts (3)	tax increase (4)	tax cut (5)
Political conditions (in year *t*-1)					
=1 if Democratic governor	0.472	0.571	0.392	0.003	-0.034**
				0.012	*0.015*
Economic conditions (in year *t*-1)					
state budget balance	0.020	0.010	0.034		
	0.069	*0.067*	*0.054*		
state budget deficit	-0.014	-0.022		-0.692**	
	0.026	*0.036*		*0.298*	
state budget surplus	0.034		0.040		0.274*
	0.056		*0.045*		*0.147*
=1 if state bond rating downgraded	0.044	0.107	0.025	0.027	-0.040
				0.034	*0.039*
GSP growth rate	0.027	0.020	0.030	0.138	0.113
	0.028	*0.025*	*0.029*	*0.229*	*0.360*
state unemployment rate	0.055	0.052	0.056	-0.222	-0.620
	0.018	*0.018*	*0.020*	*0.529*	*1.179*
state union penetration	0.085	0.086	0.096	-0.004	-0.011
	0.043	*0.035*	*0.049*	*0.004*	*0.007*
Tax competition (in year *t*-1)					
state's corporate tax rate relative to highest	-0.019	-0.018	-0.014	-0.012**	0.024***
corporate tax rate among neighboring states	*0.037*	*0.024*	*0.034*	*0.005*	*0.009*
Diagnostics					
R^2				11.0%	24.6%

Table IA.2, Panel A.
Test 2: Bank-tax Sensitivity of Leverage By Financial Constraints Measure.

We estimate standard capital structure regressions to test whether 'constrained' firms decrease (increase) their debt holdings in response to an inward (outward) shift in the supply of credit induced by an increase (cut) in state bank taxes in their headquarter state, and if so, whether this change in debt holdings is larger than that of 'unconstrained' firms. Firms are categorized as 'constrained' or 'unconstrained' according to the five measures of financial constraints introduced in Table 1. We follow a difference-in-differences approach, restricting the control group to firm-years neighboring a state affected by a tax change. Unlike in Table 5 in the paper, we restrict the set of bank tax increases to those that do not coincide with corporate income tax increases in the same state. The dependent variable in Panel A is the change in long-term book leverage. The dependent variable in Panel B is the change in the logarithm of real long-term debt. For variable definitions and details of their construction, see Appendix A. All specifications are estimated using OLS in first differences with Fama-French 48 industry-year fixed effects. 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									
	Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
	non-dividend payer (1)	dividend payer (2)	unrated (3)	rated (4)	con-strained (5)	uncon-strained (6)	con-strained (7)	uncon-strained (8)	con-strained (9)	uncon-strained (10)
Bank tax change (in %) at t =0	0.003**	-0.003	0.003	-0.010***	0.010**	-0.003*	0.004	-0.007***	0.000	-0.002
	0.002	*0.003*	*0.002*	*0.003*	*0.004*	*0.002*	*0.003*	*0.002*	*0.004*	*0.002*
Lagged change in …										
ROA	-0.008*	-0.009	-0.005	0.009	-0.014	-0.007	-0.005	0.011	-0.008	-0.002
	0.005	*0.021*	*0.004*	*0.057*	*0.009*	*0.005*	*0.006*	*0.025*	*0.006*	*0.016*
tangibility	0.051**	0.007	0.044**	0.061	0.084***	0.014	0.048**	0.045	0.020	0.090***
	0.020	*0.043*	*0.021*	*0.051*	*0.025*	*0.027*	*0.020*	*0.035*	*0.024*	*0.021*
firm size	0.011***	-0.005	0.004	0.023**	-0.001	0.005	0.005	0.006	0.004	0.005
	0.003	*0.005*	*0.003*	*0.010*	*0.005*	*0.004*	*0.003*	*0.006*	*0.004*	*0.004*
investment opportunities	0.000	-0.002	0.000	-0.003	-0.001	-0.001	0.000	-0.003	0.000	-0.004**
	0.001	*0.001*	*0.001*	*0.002*	*0.001*	*0.001*	*0.001*	*0.003*	*0.001*	*0.002*
Diagnostics										
R^2	6.7%	9.1%	5.3%	12.4%	12.5%	8.5%	10.9%	11.0%	11.5%	12.3%
Wald test: equal tax effect	3.53*		10.05***		6.76**		8.64***		0.07	
No. of firms	4,054	2,230	4,995	1,465	2,896	3,008	2,957	1,605	2,956	2,087
No. of observations	13,616	9,980	17,837	5,887	6,399	8,548	7,953	7,589	7,928	7,312
No. of treated obs.	4,522	2,955	5,742	1,756	1,989	2,764	2,655	2,262	2,685	2,121

Table IA.2, Panel B.
Test 2: Bank-tax Sensitivity of Log Debt By Financial Constraints Measure.

	Dep. var.: Change in log long-term debt									
	Dividends		Credit ratings		Kaplan-Zingales		Hadlock-Pierce		Whited-Wu	
	non-dividend payer	dividend payer	unrated	rated	con-strained	uncon-strained	con-strained	uncon-strained	con-strained	uncon-strained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Bank tax change (in %) at *t* =0	0.007	-0.028	0.004	-0.071	0.049	-0.035	-0.004	-0.071**	-0.032	0.004
	0.015	*0.026*	*0.022*	*0.049*	*0.038*	*0.033*	*0.017*	*0.029*	*0.026*	*0.023*
Lagged change in …										
ROA	-0.064	0.151*	-0.046	0.633**	-0.101*	-0.046	-0.056***	0.820***	-0.070*	0.173
	0.040	*0.083*	*0.029*	*0.308*	*0.059*	*0.039*	*0.018*	*0.208*	*0.037*	*0.158*
tangibility	0.344**	0.284	0.356***	0.571	0.451***	0.211	0.197***	0.682*	0.084	0.741**
	0.146	*0.275*	*0.116*	*0.550*	*0.147*	*0.223*	*0.073*	*0.369*	*0.090*	*0.286*
firm size	0.181***	0.091**	0.121***	0.357***	0.113**	0.102***	0.091***	0.167***	0.094***	0.118***
	0.027	*0.039*	*0.027*	*0.069*	*0.043*	*0.023*	*0.024*	*0.043*	*0.021*	*0.035*
investment opportunities	0.014***	-0.012	0.007**	0.040**	0.004	0.008*	0.011***	-0.039*	0.008**	-0.014
	0.003	*0.011*	*0.003*	*0.015*	*0.007*	*0.005*	*0.004*	*0.022*	*0.004*	*0.009*
Diagnostics										
R^2	6.6%	9.0%	6.2%	11.8%	13.8%	8.5%	11.7%	10.3%	10.8%	10.9%
Wald test: equal tax effect	3.28*		3.10*		3.53*		3.31*		1.77	
No. of firms	4,054	2,230	4,995	1,465	2,896	3,008	2,957	1,605	2,956	2,087
No. of observations	13,616	9,980	17,837	5,887	6,399	8,548	7,953	7,589	7,928	7,312
No. of treated obs.	4,522	2,955	5,742	1,756	1,989	2,764	2,655	2,262	2,685	2,121