Do Measures of Financial Constraints Measure Financial Constraints? [*] [†]

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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.

Key words: Financial constraints.

JEL classification: G32, G31, G33.

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:[2] 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). 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 the shape of 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) who use staggered changes in state corporate income tax rates across U.S. states and time to test how taxes affect capital structure. 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 supply curve of credit 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), Livdan, Sapriza, and Zhang (2009), Duchin, Ozbas, and Sensoy (2010), Denis and Sibilkov (2010), Giroud and Mueller (2013), among many others.

[2] As Kaplan and Zingales (1997) put it, constrained firms face a "wedge" between their internal and external cost of funds (due to information frictions, moral hazard, or imperfections in the capital markets). This is equivalent to saying that the supply curve for external capital is steeper than the supply curve for internal funds.

Our second test uses exogenous variation in the supply of a specific form of capital – bank loans in a firm's home state – induced by state-level changes in taxes on banks. The intuition for this test is that an unconstrained firm should not be sensitive to supply shocks to a specific form of capital: if bank loans become 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 obtain reasonably priced capital from other sources; in other words, its (overall) supply curve is less elastic than that of a firm with many choices of funding sources.

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. Perhaps the most obvious identification concern for Tests 1 and 2 is that states do not change tax rates in a vacuum. This raises the concern that the changes might coincide with local 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' debt holdings.

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. In stark contrast to public firms, 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).

When applied to the five most popular measures of financial constraints, our three tests paint

a similar picture: firms the literature would classify as ‘constrained’ do not behave in ways that suggest they face inelastic capital supply curves. In particular, 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 local banks are hit with a tax shock that shifts the supply of bank loans in its home state; and
- raise equity while 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.

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.

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 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’ 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 these measures for robustness.

Augmenting Hayashi's (1982) *Q*-investment model, Fazarri, Hubbard, and Petersen (1988) find a significant sensitivity of investment to cash flow in a sample of 422 manufacturing 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.[3]

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 (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 potentially constrained firms but among the *least* constrained firms in the FHP sample. The implications are that neither absence of dividends nor significant cash flow sensitivities are useful indicators of financial constraints.

Lamont, Polk, and Saa-Requejo (2001) use Kaplan and Zingales' classification of the 49

[3] 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, fully 73.9% of non-financial non-utility firms traded on U.S. exchanges *never* paid a dividend.

firms in FHP's sample according to their degree of financial constraints to estimate an ordered logit model that relates this classification to several readily available accounting variables. The estimated regression coefficients are used to construct an (out-of-sample) index consisting of a linear combination of these variables. The index is higher for firms that are more financially constrained, and is based on five variables: cash flow to total capital (negative effect on the index), the market to book ratio (positive), debt to total capital (positive), dividends to total capital (negative), and cash holdings to capital (negative).

Hadlock and Pierce (2010) update Kaplan and Zingales' (1997) text-based approach by combing the 10-Ks of 356 pseudo-randomly selected firms over the period 1995-2004 for evidence of firms identifying themselves as financially constrained. Then, they use this classification to create their own index of financial constraints, which in their case is based on size (negative effect on the index), size-squared (positive), and age (negative). Ball, Hoberg, and Maksimovic (2011) 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.

These text-based approaches are not universally popular. In a response to Kaplan and Zingales (1997), Fazzari, Hubbard, and Petersen (2000) question whether managers will necessarily reveal the presence of financial constraints in their 10-Ks. They cite correspondence with a senior SEC official who explained that SEC regulations require "the firm to reveal the inability to invest due to financing constraints only when the firm fails to act on a *previously announced* investment commitment" (emphasis in the original). This implies that text-based approaches need not reveal the full extent of financial constraints. For example, a firm that is chronically constrained would presumably never announce investment commitments in the first place and so would be under no obligation to comment on its constraints in its 10-K.

Another popular measure of financial constraints is to classify as constrained all firms

lacking a credit rating. Faulkender and Petersen (2006) argue that having a credit rating identifies firms that have access to the public bonds markets, while "firms that do not have access to the public debt markets are constrained by lenders as to the amount of debt capital they may raise."

Whited and Wu (2006) follow a different approach to capture financial constraints. Their index is based on the estimation of 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 (negative effect on the index); a dummy capturing whether the firm pays a dividend (negative); long-term debt to total capital (positive); size (negative); sales growth (negative); and industry sales growth (positive).

2. Sample and Data

Our sample of public firms consists of all U.S. companies 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 final sample of 91,485 firm-years for 10,112 firms (though the need to lag certain variables as well as gaps in the panel structure of some firms will reduce the sample size used in our regressions).[4]

We also use a sample of private U.S. companies, 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 the period 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

[4] 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.).

'unconstrained' according to five popular measures of financial constraints. The first measure classifies firms as constrained or unconstrained based on whether they do or do not lack a history of paying a dividend.[5] 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.[6] (For variable definitions and details of their construction, see Appendix A.)

All measures except the KZ index, which we will discuss later, 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 less tangible assets, lower return on assets, and are more likely to be unprofitable. These characteristics are intuitive markers for financial constraints. Perhaps not coincidentally, '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.)

Interestingly, however, 'constrained' firms also have substantially higher market-to-book ratios, indicating that investors expect them to grow faster than 'unconstrained' firms. Consistent with such expectations, 'constrained' firms experience significantly faster growth in both sales and employment. The average unrated firm, for example, grows sales and employment by 27.7% and 13.5% a year, respectively, compared to less than half that (11.7% and 5.2%) for the average rated firm. Thus, being younger, smaller, less profitable, and less leveraged does not appear to be an impediment to fast growth.

[5] To establish the necessary history, we look back as far as 1970.
[6] 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.

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%).[7] 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 innovation.

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 'unconstrained' firms. They have *less* cash on their balance sheets, have *more* tangible assets, and are *less* often unprofitable (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.[8] 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 considerable less on R&D.

2.3 Cross-tabulations

Panel B reports cross-tabulations of the five financial constraints measures. For each

[7] The latter is a rare instance of the HP and WW indices producing different results.
[8] This is not surprising, given that leverage and cash are two of the components of the index, with leverage being positively associated with financial constraints and cash negatively associated.

measure, the first five rows show the fraction of firms classified as constrained according to the measure that would be classified as constrained under each of the other four measures. This illustrates the extent to which the 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 do not have a credit rating while 29.8% of rated firms do not pay dividends. The KZ index again stands out. For example, unrated firms are *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, 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 such as firms' lifecycle stage.

We begin by formally defining financial constraints. We then present three distinct tests that we use to analyze whether firms classified as financially constrained according to the previous five measures of financial constraints are indeed financially constrained. Overall, the evidence from these tests suggests that the behavior of firms classified as financially constrained by these measures is not consistent with them in fact being financially constrained.

3.1 Defining financial constraints

Kaplan and Zingales (1997) classify as "likely to be financially constrained" firms that in their 10-Ks "mention having difficulties in obtaining financing." These "include firm-years in which firms postpone an equity or convertible debt offering due to adverse market conditions, or claim they need equity capital but are waiting for improved market conditions." They go on to classify as "undoubtedly financially constrained" firm-years that "are in violation of debt covenants, have been cut out of their usual source of credit, are renegotiating debt payments, or declared that they are forced to reduce investments because of liquidity problems."

This classification suggests that, to identify whether a firm is financially constrained at a given point in time, we would ideally like to observe the supply curve of external financial capital it faces (henceforth, the supply curve of capital).[9] To formalize this, denote the supply of capital curve by *p(k)*, a function capturing the price at which a firm with *k* units of financial capital (in the form of debt and equity) can raise an incremental unit of capital in the capital markets. This allows us to 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.[10] 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 the notion that the firm has been "cut out" of the capital markets. Figure 1 captures this definition graphically:

[9] For the time being, we assume that the supply curves of equity and debt capital look similar and refer to both as the supply curve. This is a common simplification in the financial constraints literature (see, e.g., Whited and Wu (2006)). Later in the paper, we discuss how our findings are qualified by this simplification.

[10] An inelastic supply curve implies, as Fazzari, Hubbard, and Petersen (1988) put it, that "the cost of new debt and equity may differ substantially from the opportunity cost of internal finance generated through cash flow and retained earnings." Fazzari et al. point to frictions in the capital markets, especially asymmetric information between firms and investors, as driving a wedge between the internal and external costs of capital.

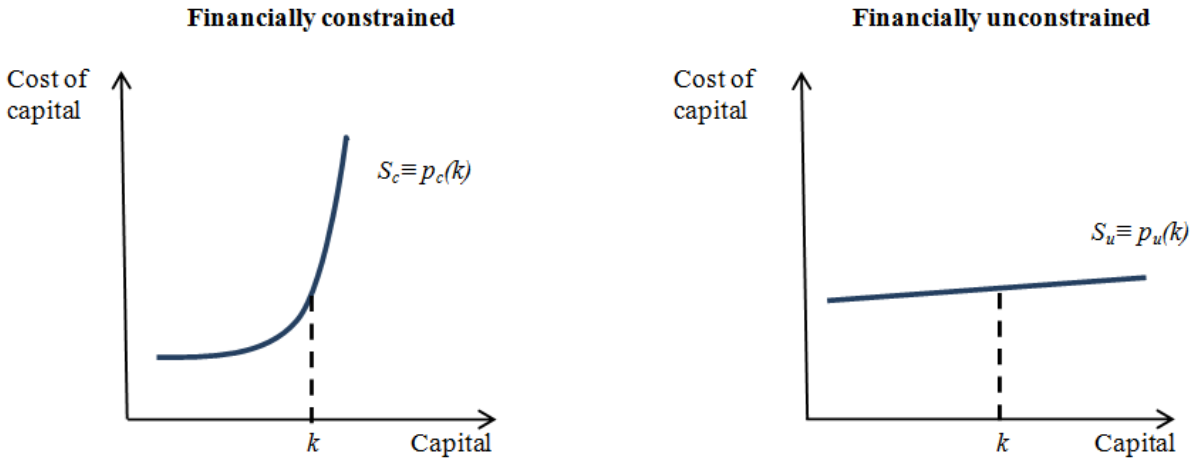


Figure 1. The figure shows the supply of capital curves faced by two hypothetical firms, both currently holding k units of capital. The firm on the left is financially constrained. The firm on the right is financially unconstrained.

This discussion highlights the challenge associated with measuring financial constraints: a firm's supply of capital curve is not readily observable to the econometrician. This is why the literature instead attempts to infer the elasticity of the capital supply curve indirectly, by either looking at what managers say (e.g., KZ index, HP index) or by focusing on some 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 and 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 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 classified as constrained or unconstrained by five popular measures. This follows the traditional econometric approach used to estimate the shape of a supply curve (see, e.g., Hayashi's (2000) textbook, p. 189).

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

The standard trade-off theory of capital structure predicts that the demand for debt of a firm

expecting to be profitable should increase in the marginal tax rate it faces, as debt confers a tax benefit given that interest payments are tax-deductible in the U.S. In recent work, Heider and Ljungqvist (2013) provide empirical evidence consistent with this prediction. Specifically, they exploit staggered changes in corporate income tax rates across U.S. states to show that firms increase their long-term leverage by 104 basis points in response to a tax increase in their home state. Motivated by this evidence, we exploit increases in state corporate income tax rates as plausibly exogenous shocks to the demand for debt. This allows us to estimate the shape of the debt supply curve faced by firms typically classified as either constrained or unconstrained.[11]

Figure 2 illustrates the identification strategy behind our first test. 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 unconstrained firm, on the other hand, should make full use of the additional tax shields by increasing its debt holdings. Empirically, therefore, we can judge how well a financial constraints measure actually captures financial constraints by testing for a higher average sensitivity of debt to tax increases among firms classified as financially constrained than among firms classified as unconstrained.

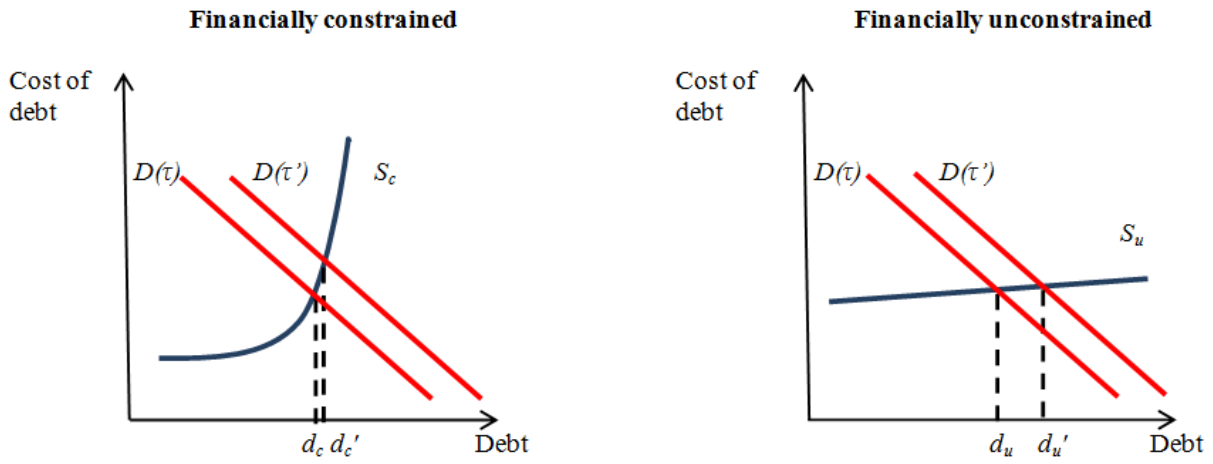


Figure 2. This figure illustrates the identification strategy behind Test 1 by showing how a corporate tax increase from τ to τ' shifts the demand curve for debt, D, and the effect that this shift has on the debt holdings of a financially

[11] We focus exclusively on tax increases as Heider and Ljungqvist show that firms do not reduce their leverage in response to tax cuts, suggesting that the tax sensitivity of leverage is asymmetric.

constrained firm (from d_c to d_c') and a financially unconstrained firm (from d_u to d_u').

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

For our identification strategy to be valid, a key requirement is that we need to be able to capture changes in debt holdings that are the direct result of the tax increases and so are not confounded by shifts in the supply of capital curve. The key concern is that business cycle factors cause both states to raise tax rates and banks to cut lending. 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, thus shifting the supply of debt curve inward.

Heider and Ljungqvist (2013) investigate the extent to which local business cycle effects are correlated with tax policy changes and show that state tax increases are unrelated to GSP growth rates and state unemployment rates. While these results are reassuring, they do not eliminate the concern that the results of our test might be confounded by business cycle effects, which would compromise our ability to identify financial constraints.

Fortunately, the local and staggered nature of state tax increases enables us to follow a differences-in-differences approach, using as a control group firms that have not been affected by a tax increase (see Heider and Ljungqvist (2013) for further discussion of the approach). To better control for local economic conditions, we restrict our control group to firms located in a state adjacent to the state of the tax increase. This differentiates away any changes in debt

holdings that are the result of changes in local economic conditions,[12] thus allowing us to identify the effect that shifts to the debt demand curve induced by state tax increases have on debt holdings.[13]

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. The test thus cannot speak to question whether a particular measure of financial constraints is able to correctly identify constrained firms that are loss-making. Second, Test 1 only exploits 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 being shut out of 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 we cannot unequivocally identify firms that *are* constrained.

3.2.2 Empirical specification

Equation (1) captures our empirical strategy for Test 1:

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

where *i* indexes firms, *j* industries, *s* headquarter states, and *t* fiscal years. The dependent variable *D* is measured in two alternative ways. First, we use long-term book leverage, as is common in the capital structure literature. In addition, we also reproduce all our analyses using the natural logarithm of long-term debt, to ensure that our results are not driven by firms

[12] This differences-in-differences approach would not be feasible if we used federal tax increases instead of state tax increases as a source of identification.

[13] An additional potential concern is that corporate tax increases might coincide with bank tax increases. In this case, the debt demand curve would shift outward and, at the same time, the supply curve would shift inward if local banks responded to higher bank taxes by cutting lending. This would potentially bias our estimated tax sensitivities downwards, reducing the power of our test. However, this is unlikely to be a major concern because most states tax banks and other financial institutions differently from industrial firms. During our sample period, we have 25 tax increases unique to either industrial corporations or banks, and 22 increases that affect both.

increasing their leverage by reducing their asset base without actually raising any debt. Our main regressor of interest, T^{+}, is a dummy variable equal to 1 if the state a firm is headquartered in has increased its corporate income taxes, and 0 otherwise.[14] 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, 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 (as well as nationwide shocks affecting all industries at the same time). As explained above, we focus our analysis on firm-year observations affected by a tax increase as well as firm-years headquartered in neighboring states, thus minimizing 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)).[15] The assumption motivating this power test is that private firms are more likely to be financially constrained than public firms. Thus if Test 1 has power to identify financial constraints, we should find that private firms respond less strongly to tax increases than to public firms.

The results, shown in Table 2, support this prediction and so suggest Test 1 has power. Column 1 shows that the average public firm increases its leverage by 1.1 percentage points in response to a tax rise ($p<0.001$). This is identical to the corresponding point estimate in Heider

[14] Following Heider and Ljungqvist (2013), we lag this variable to ensure that firms have enough time to adjust their debt holdings in response to the tax increases. See Appendix A in Heider and Ljungqvist for a complete list of the tax increases used in our analysis.

[15] The sample of private firms includes only C Corps. Firms that are S Corps or are unincorporated (e.g., sole proprietorships, partnerships, and LLCs) pay personal rather than corporate income taxes and thus are not affected by our corporate income tax shocks.

and Ljungqvist's (2013) Table 10. Column 2 restricts the control group to firm-years neighboring a state affected by a tax rise. 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 and 6 show that these results also hold when we use the change in log long-term debt as the dependent variable: firms significantly increase the amount of debt in their capital structure. This alleviates concerns that our findings might be driven by changes in firms' total assets rather than in their debt holdings. Overall, we conclude 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 fail to 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. To the extent that size correlates with financial constraints, as Hadlock and Pierce (2010) and Whited and Wu (2006) assume, Table 2 thus suggests that Test 1 has power to capture variation in financial constraints across different groups of firms.

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.[16] This suggests that none of the five measures of financial constraints is able to identify firms with unusually inelastic supply of capital curves.

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. The two caveats, as before, are that the test 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 a financially unconstrained firm should not be affected by shocks to capital supply that do not alter its investment opportunity set: if one source of capital becomes less plentiful, an unconstrained firm should be able to substitute towards another. A financially constrained firm, on the other hand, should find itself less able to find substitute sources of capital. These predictions hold whether or not the firm is profitable, allowing us to address the first limitation of Test 1.

[16] In fact, in 7 of the 10 cases in Table 3, the estimated tax sensitivity is *larger* for 'constrained' firms, significantly so in one case: the average non-dividend payer increases leverage by 1.7 percentage points following a tax increase, compared to half that amount among dividend payers (p-value for the difference in coefficients = 0.068).

To operationalize Test 2, we exploit state-level changes in *bank* taxes as shocks to the supply of credit. Most states tax banks differently from corporations, and changes in bank taxes do not tend to coincide with changes in corporate taxes. As changes in bank taxes affect the after-tax profitability of lending, we expect banks to expand lending in states with falling taxes at the expense of states with rising taxes. For a financially constrained firm, falling bank taxes should thus make the supply of bank credit less inelastic and vice versa. This allows us to test whether the debt holdings of firms classified as financially constrained according to the literature are indeed sensitive to state-level variation in bank taxes.

Figure 3 illustrates our identification strategy. Take the case of a bank tax increase, which we assume shifts the debt supply curve inward (an assumption we discuss below). As illustrated by the graph on the right, we expect no meaningful change in debt holdings for an unconstrained firm. A financially constrained firm, on the other hand, faces a relatively more inelastic debt supply curve, so a reduction in debt supply should lead to a large decrease in its debt holdings. An analogous argument applies to bank tax cuts, which shift the debt supply curve outward.

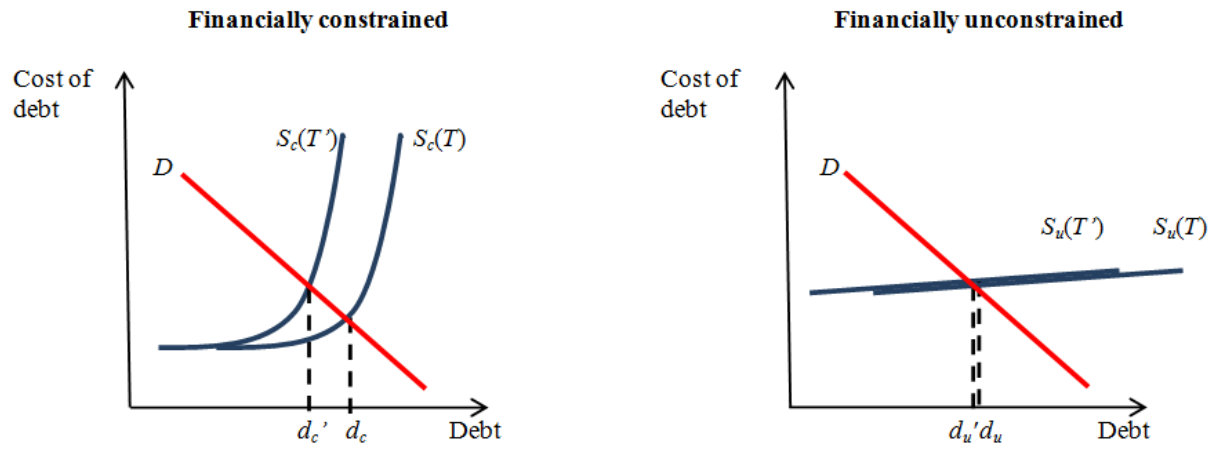


Figure 3. This figure illustrates the identification strategy behind Test 2. It shows how a bank tax increase from T to T' shifts the supply curve of debt, S, to the left and traces out the effect of this shift on the debt holdings of a financially constrained firm (from d_c to d_c') and of a financially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

As in the previous section, we first discuss the identifying assumptions and limitations of Test 2. 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 supply of bank loans.

3.3.1 Identifying assumptions and limitations

The key identifying assumption behind Test 2 is that the supply shocks (i.e., state bank tax changes) have no direct impact on firms' demand for debt, so that we can isolate changes in debt holdings that are the direct result of the supply shocks.[17] In this sense, identification strategy for Test 2 is similar in spirit to Lemmon and Roberts (2010), Chava and Purnanandam (2011), and Lin and Paravisini (2011) who analyze the effects of capital supply shocks on firm investment and funding, performance, and risk and return, respectively.

As in Test 1, the local and staggered nature of state bank tax changes allows us to follow a differences-in-differences approach, using as a control group firms located in neighboring states that have not been affected by the changes. This allows us to differentiate away changes in debt holdings that are the result of changes in local economic conditions, enabling us to identify the effect of shifts in the debt supply curve on firms' debt holdings.

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 an exogenous shock even larger than those that triggered the 2007-08 financial crisis, a large number of previously unconstrained firms would presumably find themselves facing an inelastic supply curve.[18] 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

[17] The fact that most states tax banks differently from other firms alleviates concerns that the identification strategies behind Tests 1 and 2 might significantly overlap, which would weaken their respective powers.

[18] 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 unconstrained firms as inelastic as the S_c curve in Figure 1.

would have poor external validity.

Changes in state bank taxes tend to be relatively small and so are unlikely to fundamentally alter the capital supply curves for firms operating in the state. (MA's 1996 cut in the bank tax rate from 12.13% to 11.72% is fairly representative. See Appendices B1 and B2 for a complete list of the bank tax increases and cuts used in our analysis.) The main concern is thus whether these shocks are large enough to induce a significant change in bank lending behavior.

The first three columns in Table 4 aim to answer this question. We use bank 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 impact (the logarithm of) the total dollar amount of commercial and industrial loans held by banks headquartered in that state,[19] 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 1.9 percent decrease (increase) in the commercial and industrial loans held by banks headquartered in the state. This suggests that changes in state bank taxes induce a significant change in the lending behavior of local banks, 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 limited to firms located in neighboring states to those affected by a bank tax change:

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

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

[19] 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 very large 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.

points) affecting banks operating in the state the firm is headquartered in. 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.[20] To the extent that private firms, particularly small ones, are more likely to face an inelastic debt supply curve, we expect their debt to be more sensitive to shifts in the debt supply curve than that 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 whether firms classified as constrained and unconstrained by the five financial-constraints measures behave in a way that is consistent

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

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 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 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 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 could 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 faced both an inelastic

debt supply curve and an inelastic equity 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 time period, 48.4% of the proceeds of public U.S. firms' equity issues were paid out (via dividends or share repurchases) during the same year, a practice they call "equity recycling."[21] The identifying assumption behind Test 3 is that we should not observe equity recycling among firms facing an inelastic supply of equity curve. Therefore, equity recycling is an indicator that a firm is not concerned about its ability to raise equity and thus is unconstrained.[22]

3.4.1 Empirical specification

Our analysis of what firms do with the proceeds of their equity issues adapts the framework followed 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:

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

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 purchase plans, during the fiscal year. *Other Sources of Funds* comprises operating cash flows and debt issues net of debt repurchases. 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.

[21] Farre-Mensa et al. show that much of this equity recycling is driven by firms paying out the proceeds obtained from the exercise of stock options held by executives and employees as well as from employee stock purchase plans.
[22] There can be a number of reasons why firms find it optimal to recycle equity (e.g., to avoid missing earnings per share targets), which are explored by Farre-Mensa et al. Regardless of the reason, the only premise behind Test 3 is that this behavior is hard to reconcile with being financially constrained.

3.4.2 Power

To establish power, we first compare 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 do not expect them to engage in equity recycling. Table 6 reports the results. 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). Therefore, 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 as issuing equity.[23] A plausible interpretation is that private firms fund increases in their investment or operating needs through a combination of equity issues and reductions in dividend payouts. 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) as firms grow in size.

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.

[23] Sageworks does not report data on share repurchases, so we cannot analyze total payouts for private firms.

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 to '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 similar picture: the behavior of firms the literature would classify 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 due to exogenous increases in state corporate income taxes (Test 1);
- maintain borrowing levels when local banks are hit with a tax shock, even though banks cut (increase) their lending when bank taxes go up (down) (Test 2);
- and use a significant part of the proceeds of equity issuances 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 are hard to reconcile with the notion that firms commonly classified in the literature as financially constrained face an inelastic supply of capital curve and are thus indeed constrained.

As a final validation of our methodological approach, we apply our tests to a subsample of public firms that are highly 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 0, for either the change in long-term book leverage or the change in log long-term 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 when local banks are hit with a tax increase and vice versa (though the effect is only statistically significant for changes in leverage). 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 an average, an inelastic supply of capital curve and are thus 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 differ significantly, on average, in their ability to raise debt or equity capital. Does this mean that 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 in financial constraints.[24]

[24] For instance, Whited and Wu (2006) show that the returns of 'constrained' firms move together and tend to be higher than those of 'unconstrained' firms.

Our summary statistics and cross-tabulations in Table 1 suggest that 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 classified by the five measures. As we will see, the five measures produce sample splits that differ markedly in terms of funding patterns.

Table 9 shows the annual probability that a given 'constrained' or 'unconstrained' firm will issue equity or bonds, or take out a loan.[25] This reveals three important differences across the subgroups. First, 'constrained' firms are substantially more likely to fund themselves by issuing equity than are 'unconstrained' firms.[26] For instance, on average 9.3% of non-dividend paying firms raise equity 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-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, approximately one-fifth (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%).[27] This difference is mostly driven by issues of *public* bonds, which are rare among 'constrained' firms, but it persists if we focus on rule 144A bonds or privately-placed bonds.

[25] Table 9 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).
[26] As is customary, our data on equity issues come from SDC and thus focuses only on "external" (primary) issuances, excluding issuances to directors or employees that result from the exercise of stock options or ESPPs.
[27] 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%).

Third, while ‘constrained’ firms do not use the bond markets, they do have regular access to 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.

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 growing faster 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 have regular access to the public equity and loan markets (though not to 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 least 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 these 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 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 and rarely access either the public or the private bond market. But they appear to have ready access to both the equity markets and the syndicated loan market, which appear to supply capital to them when the firms need it.

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.

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Appendix A. Variable Definitions.

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

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

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

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

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

Investment opportunities is measured 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*].

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

Appendix B1. List of State Bank Tax Increases.

This table lists all increases in the state income tax rate affecting bank and other financial institutions over the tax years 1989-2011. In states with more than one tax bracket, we report the change to the top bracket. To identify these changes, we use data obtained from the Tax Foundation (an abbreviated version of which is available at http://www.taxfoundation.org), 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
IL	1989	Increase in top rate from 4% to 4.8%
NJ	1989	Introduction of a 0.375% tax rate surcharge
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%
AR	1991	Increase in top rate from 6% to 6.5%
NE	1991	Increase in top rate from 3.53% to 3.81%
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
MT	1993	Increase in tax surcharge on tax liability from 2.3% to 4.7%
DC	1994	Introduction of additional 2.5% tax surcharge on tax liability
RI	1996	Increase in top rate from 8% to 9%
NH	1999	Increase in top rate from 7 to 8%
AL	2001	Increase in top rate from 6% to 6.5%
NH	2001	Increase in top rate from 8% to 8.5%
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
CT	2004	Increase in surcharge to 25%
NJ	2006	Introduction of 4% tax surcharge on tax liability
MD	2008	Increase in top rate from 7% to 8.25%
CT	2009	Introduction of 10% tax surcharge on tax liability if revenues > $100m
NC	2009	Introduction of 3% tax surcharge on tax liability
OR	2009	Increase in top rate from 6.6% to 7.9%
IL	2011	Increase in top rate from 7.3% to 9.5%

Appendix B2. List of State Bank Tax Cuts.

This table lists all decreases in the state income tax rate affecting bank and other financial institutions over the tax years 1989-2011.In states with more than one tax bracket, we report the change to the top bracket. To identify these changes, we use data obtained from the Tax Foundation (an abbreviated version of which is available at http://www.taxfoundation.org), 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	Decrease in top rate from 5.5% to 5.4%
WV	1989	Decrease in top rate from 9.75% to 9.6%
AZ	1990	Decrease in top rate from 10.5% to 9.3%
CO	1990	Decrease in top rate from 5.4% to 5.3%
WV	1990	Decrease in top rate from 9.45% to 9.3%
CO	1991	Decrease in top rate from 5.3% to 5.2%
MT	1991	Repeal of 5% surcharge
WV	1991	Decrease in top rate from 9.3% to 9.15%
CO	1992	Decrease in top rate from 5.2% to 5.1%
CT	1992	Decrease in tax surcharge from 20% to 10%
NY	1992	Decrease in tax surcharge from 15% 10%
WV	1992	Decrease in top rate from 9.15% to 9%
CO	1993	Decrease in top rate from 5.1% to 5.0%
CT	1993	Repeal of 10% tax surcharge
NH	1993	Decrease in top rate from 8% to 7.5%
NY	1993	Repeal of 10% tax surcharge
AZ	1994	Decrease in top rate from 9.3% to 9%
HI	1994	Decrease in top rate from 11.7% to 7.92%
MT	1994	Repeal of 4.7% tax surcharge
NH	1994	Decrease in top rate from 7.5% to 7%
NJ	1994	Repeal of 0.375% tax surcharge
CA	1995	Decrease in top rate from 11.47% to 11.3%
CT	1995	Decrease in top rate from 11.5% to 11.25%
DC	1995	Decrease in top rate from 10% to 9.5% (+2 tax surcharges at 2.5% each)
MA	1995	Decrease in top rate from 12.54% to 12.13%
CT	1996	Decrease in top rate from 11.25% to 10.75%
MA	1996	Decrease in top rate from 12.13% to 11.72%
CA	1997	Decrease in top rate from 11.3% to 10.84%
CT	1997	Decrease in top rate from 10.75% to 10.5%
MA	1997	Decrease in top rate from 11.72% to 11.32%
NC	1997	Decrease in top rate from 7.75% to 7.5%
AZ	1998	Decrease in top rate from 9% to 8%
CT	1998	Decrease in top rate from 10.5% to 9.5%
MA	1998	Decrease in top rate from 11.32% to 10.91%
NC	1998	Decrease in top rate from 7.5% to 7.25%
CO	1999	Decrease in top rate from 5% to 4.75%
CT	1999	Decrease in top rate from 9.5% to 8.5%
KS	1999	Decrease in top rate from 4.25% to 2.25%
MA	1999	Decrease in top rate from 10.91% to 10.5%
NC	1999	Decrease in top rate from 7.25% to 7%
AZ	2000	Decrease in top rate from 8% to 7.968%
CO	2000	Decrease in top rate from 4.75% to 4.63%
CT	2000	Decrease in top rate from 8.5% to 7.5%
NC	2000	Decrease in top rate from 7% to 6.9%
AZ	2001	Decrease in top rate from 7.968% to 6.968%
ID	2001	Decrease in top rate from 8% to 7.6%
NY	2001	Decrease in top rate from 9% to 8.5%
NY	2002	Decrease in top rate from 8.5% to 8%
NY	2003	Decrease in top rate from 8% to 7.5%
AR	2005	Repeal of 3% tax surcharge on tax liability
CT	2006	Decrease in tax surcharge from 25% to 20%
NY	2007	Decrease in top rate from 7.5% to 7.1%
WV	2007	Decrease in top rate from 9% to 8.75%

CT	2008	Repeal of 20% tax surcharge
WV	2009	Decrease in top rate from 8.75% to 8.5%
MA	2010	Decrease in top rate from 10.5% to 10%
NJ	2010	Repeal of 4% tax surcharge
MA	2011	Decrease in top rate from 10% to 9.5%
NC	2011	Repeal of 3% tax surcharge
ND	2011	Decrease in top rate from 7% to 6.5%
OR	2011	Decrease 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 non-financial and non-utility public U.S. companies that are classified as 'constrained' and 'unconstrained' by five common measures of financial constraints. For variable definitions and details of their construction, see Appendix A. All pairwise mean differences are significant at the 1% level except those that are in between 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	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
Market-to-book	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*
No. of firm-years		46,970	36,935	62,092	22,174	21,895	23,926	23,138	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, thus illustrating the extent to which the measures produce similar classifications. Specifically, for each measure, the first five rows show the fraction of firms classified as constrained according to the measure that would be classified as constrained under each of the other four measures, while the last five rows report the fraction of firms classified as unconstrained 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 leverage 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. In columns 1 through 4, we use the change in long-term book leverage as the dependent variable, while in columns 5 through 8 we use the change in the logarithm of real long-term debt. 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				Public firms			
	all	neighbors	Private firms		all	neighbors	Private firms	
	(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 obs.	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 leverage 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. In Panel A, we use the change in long-term book leverage as the dependent variable, while in Panel B we use 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 (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)
=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 bank Call Report data from the Federal Reserve to test whether increases (decreases) in state bank taxes induce a decrease (increase) in commercial and industrial lending by banks headquartered in the state. In columns 4 through 9, we estimate standard leverage 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 $t = 0$	-0.019***	-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)						-0.004*			-0.007*
						0.002			*0.004*
x quartile 2						-0.006***			-0.009***
						0.002			*0.003*
x quartile 3						-0.004			-0.009
						0.003			*0.007*
x quartile 4 (largest)						-0.002			0.012
Lagged change in …									
ROA				-0.006	0.001	0.001	-0.017	0.002	0.002
				0.278	*0.001*	*0.001*	*0.594*	*0.006*	*0.006*
tangibility				0.039**	-0.027	-0.027	0.348***	-0.004	-0.004
				0.029	*0.018*	*0.018*	*0.005*	*0.035*	*0.035*
firm size				0.007***	0.004	0.004	0.157***	0.022*	0.022*
				0.004	*0.004*	*0.004*	*0.000*	*0.012*	*0.012*
investment opportunities				0.000	-0.001	-0.001	0.012***	-0.009	-0.009
				0.418	*0.002*	*0.002*	*0.000*	*0.010*	*0.010*
log deposits		0.317***							
		0.059							
log total assets			0.578***						
			0.047						
Diagnostics									
R^2	7.3%	9.7%	12.3%	4.1%	2.3%	2.3%	4.1%	2.4%	2.4%
F test: bank tax change = 0	10.8***	16.0***	9.0***	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.
No. of banks/firms	1,611	1,611	1,611	27,421	13,228	13,228	27,421	13,228	13,228
No. of observations	5,034	5,034	5,034	6,822	19,243	19,243	6,822	19,243	19,243
No. of treated obs.	1,275	1,275	1,275	8,264	3,499	3,499	8,264	3,499	3,499

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

We estimate standard leverage 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. In Panel A, we use the change in long-term book leverage as the dependent variable, while in Panel B we use 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	-0.007***	-0.002	-0.004*
	0.002	*0.002*	*0.001*	*0.003*	*0.003*	*0.002*	*0.003*	*0.001*	*0.003*	*0.002*
Lagged change in …										
ROA	-0.008	-0.004	-0.003	-0.012	-0.019**	-0.002	-0.002	-0.010	-0.008	-0.009
	0.005	*0.022*	*0.005*	*0.051*	*0.009*	*0.006*	*0.006*	*0.030*	*0.006*	*0.013*
tangibility	0.047**	0.001	0.044**	0.030	0.081***	0.012	0.044**	0.022	0.022	0.080***
	0.019	*0.036*	*0.018*	*0.049*	*0.022*	*0.026*	*0.019*	*0.030*	*0.021*	*0.019*
firm size	0.010***	-0.004	0.004	0.019**	-0.001	0.004	0.005	0.006	0.005	0.003
	0.002	*0.005*	*0.003*	*0.008*	*0.005*	*0.004*	*0.003*	*0.005*	*0.005*	*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	6.3%	8.6%	4.9%	12.0%	11.6%	8.3%	10.4%	10.5%	10.7%	11.7%
Wald test: equal tax effect	2.94*		2.1		5.11**		2.73*		0.14	
No. of firms	4,535	2,571	5,613	1,675	3,322	3,320	3,301	1,860	3,297	2,365
No. of observations	15,456	11,814	20,309	7,112	7,585	9,553	8,972	8,950	8,921	8,676
No. of treated obs.	4,892	3,338	6,239	2,025	2,229	2,959	2,871	2,559	2,907	2,411

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	0.003	-0.039**	-0.012	-0.049	0.026	-0.024	-0.015	-0.073***	-0.024	-0.030
	0.015	*0.015*	*0.015*	*0.038*	*0.022*	*0.026*	*0.021*	*0.021*	*0.021*	*0.023*
Lagged change in …										
ROA	-0.040	0.160	-0.018	0.450	-0.112*	-0.001	-0.021	0.730***	-0.043	0.157
	0.039	*0.108*	*0.029*	*0.294*	*0.057*	*0.038*	*0.025*	*0.230*	*0.036*	*0.136*
tangibility	0.368***	0.273	0.357***	0.509	0.504***	0.250	0.206***	0.577**	0.114	0.724***
	0.125	*0.237*	*0.096*	*0.480*	*0.135*	*0.198*	*0.069*	*0.281*	*0.085*	*0.266*
firm size	0.171***	0.105***	0.121***	0.306***	0.126***	0.095***	0.085***	0.147***	0.096***	0.105***
	0.021	*0.037*	*0.023*	*0.063*	*0.043*	*0.025*	*0.024*	*0.036*	*0.026*	*0.030*
investment opportunities	0.014***	-0.004	0.008***	0.044***	0.007	0.008**	0.012***	-0.038*	0.009**	-0.011
	0.003	*0.012*	*0.003*	*0.014*	*0.006*	*0.004*	*0.004*	*0.020*	*0.004*	*0.009*
Diagnostics										
R^2	6.2%	8.1%	5.6%	10.5%	13.0%	7.6%	11.7%	9.4%	10.1%	9.8%
Wald test: equal tax effect	5.00**		0.95		3.25*		3.20*		0.03	
No. of firms	4,535	2,571	5,613	1,675	3,322	3,320	3,301	1,860	3,297	2,365
No. of observations	15,456	11,814	20,309	7,112	7,585	9,553	8,972	8,950	8,921	8,676
No. of treated obs.	4,892	3,338	6,239	2,025	2,229	2,959	2,871	2,559	2,907	2,411

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, while in columns 3 and 4 we perform the same 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 (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)
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 highly 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**	-0.017	
			0.004	*0.046*	
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	0.049	1.421***	
	0.207	*1.413*	*0.050*	*0.475*	
tangibility	0.016	0.321	0.016	0.649	
	0.196	*1.199*	*0.078*	*0.701*	
firm size	-0.001	0.067	0.024*	0.388***	
	0.033	*0.367*	*0.014*	*0.110*	
investment opportunities	-0.005	-0.039	-0.003	0.101***	
	0.023	*0.246*	*0.005*	*0.030*	
Diagnostics					
R^2	6.1%	2.5%	9.3%	19.8%	4.8%
No. of firms	842	842	868	868	1,779
No. of observations	1,465	1,465	3,395	3,395	10,165
No. of treated obs.	239	239	991	991	n.a.

Table 9.
Sources of Financing of 'Constrained' and 'Unconstrained' Firms.

The table shows the annual probability that a given 'constrained' or 'unconstrained' firm will issue equity or bonds, or take out a 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 probability differences are significant at the 5% level or better except those that are in between 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