Estimating the Cost of Control Rights in the Corporate Loan Market*

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June 13, 2019

Abstract

Financial covenants transfer control rights to lenders when borrowers' metrics breach pre-set thresholds. Contingent control rights allow lenders to extract monetary concessions (e.g., fees and renegotiation) and behavioral concessions (e.g., CEO turnover and reduced investment, R&D, and payroll). We develop a simple tradeoff theory that allows us to estimate the cost of control. Our estimates suggest that the cost of control is 46.1 basis points. On average, control payments correspond to a 13.4% discount on the total cost of borrowing (Berg, Steffen, and Saunders 2016). This finding has implications for our understanding of loan pricing and the value of control rights in the private loan market.

JEL Classification: G21, G32, K12, L14, E32, E44

Keywords: syndicated lending, financial covenants, control rights, loan pricing

* For valuable comments and discussion, we thank Jim Albertus, Pietro Bonaldi, Maria Chaderina, Tetiana Davydiuk, Deeksha Gupta, Matt Gustafson, David Haushalter, Andy Koch, Spyros Lagaras, Leming Lin, Tatyana Marchuk, Chester Spatt, Erina Ytsma, seminar participants at Carnegie Mellon University, and participants at the CMU-PITT-PSU Finance Conference.

1 Introduction

Financial covenants, which transfer control rights to lenders when one of the borrower's financial metrics breaches its pre-set contractual threshold, have risen to take a prominent role in the corporate loan contracting space in the last 30 years (Bradley and Roberts 2015). In theory, covenants provide an interesting laboratory to study debtholder-shareholder conflicts not only because they play an important role in completing the contracting space *ex ante* (Demiroglu and James 2010, Murfin 2012, Matvos 2013; Demerjian and Owens 2016, and Prilmeier 2017), but also because they provide scope for renegotiation and wealth transfers *ex post*.

In practice, covenant violations are associated with a series of economically significant consequences for borrowers, many of which are consistent with lenders maximizing the value of their claim.[1] These consequences include changes in investment (Chava and Roberts 2008; Nini, Smith, and Sufi 2009), governance and executive compensation (Nini, Smith, and Sufi 2012; Balsam, Gu, and Mao 2018; Ferreira, Ferreira, and Mariano 2018), debt issuance and capital structure (Roberts and Sufi 2009; Nini, Smith, and Sufi 2009), innovation (Gu, Mao, and Tian 2017), employment (Falato and Liang 2016), and financial reporting (Sweeney 1994; DeFond and Jiambalvo 1994; Dichev and Skinner 2002). Given these sizable *ex post* costs borne by

[1] This literature has grown immensely: between 2008 and 2018, Web of Science reports that 50 empirical papers on financial covenants in the corporate loan market have been published in the top 3 finance and accounting journals. For an example magnitude from this literature, Chava and Roberts (2008), which has nearly 1,400 Google Scholar citations at the time this sentence was written, measures a 13% decline in borrowers' investment rates in response to covenant violations.

borrowers and the potential benefits for lenders, we ask what price supports this equilibrium transfer of contingent control rights: what is the cost of control in the corporate loan market?

To answer this question, we build a simple theoretical framework based on tradeoffs. All else equal, lenders prefer high spreads and acquiring control, but will trade them off. Likewise, borrowers prefer low spreads and retaining control, but will trade them off. If relatively homogeneous lenders compete to make a loan, the winning lender must offer terms that weakly exceed the borrower surplus generated by the next best offer (i.e., following the intuition of second price auctions). By this logic, if a borrower obtains multiple loans, these loans will generate the same surplus for the borrower. Therefore, for borrowers that obtain multiple loans, the observed loans trace the borrowers indifference curve in cost-control space, holding all other terms constant. Given some theoretical conditions (which we detail below), the slope of the borrower's indifference curve can be interpreted as the cost of control. Empirically, our objective is to estimate this slope.

We benefit from the development of new measures that capture the total cost of borrowing (Berg, Saunders, and Steffen 2016) and the *ex ante* probability of covenant violations (Murfin 2012; Demerjian and Owens 2016). The total cost of borrowing (*TCB*) aggregates pricing information across the contingent interest rates charged borrowers depending on loan usage as well as the potentially complex fee structures that accompany most loans. Similarly, rather than rely on the number of included financial covenants as a measure of covenant intensity, we use *Pr(Violation)*, a simulation-based measure of the likelihood of a future covenant violation that incorporates information in the joint distribution of the financial metrics that underlie each covenant package (Demerjian and Owens 2016). This measure, in particular, allows us to interpret our estimates as the cost of moving from 0% to 100% likelihood of a covenant violation and, therefore, a transfer of control rights.

Our preferred estimate of the cost of control is 46.1 basis points (bps). The specification that delivers this quantification is saturated with borrower by year, lender by year, lender by borrower, and loan type by loan purpose by year fixed effects as well as a rich set of time-varying loan term interactions. These controls ensure that we are relying on cross-loan variation for the same borrower in the same year, holding fixed loan characteristics and average lender preferences. In sensitivity analysis of our cost of control estimates to plausible alternative specifications, we obtain estimates in the range of 40 to 50 basis points. Our estimates imply that, for the average loan in our sample with any covenants, control payments from the lender to the borrower imply a 13.4% discount relative to the total cost of borrowing.

An important part of our theoretical framework is that we identify four important conditions necessary for the logic underlying our approach to hold. While the rich fixed effects structure that we employ is designed to satisfy these conditions and so make the coefficient on the probability of violation in our regressions interpretable as the cost of control, we also further investigate each of the four conditions. To do so, we investigate the robustness of our findings in subsets of the data where the conditions are most likely to hold as well as provide some preliminary evidence on some extensions not explicitly considered in our theory.

The first condition requires that we observe multiple loans for the borrower in the same time period. While this condition is necessarily satisfied if the estimation of the relevant coefficient is feasible with borrower by year fixed effects, it does raise two important questions. The first is how long of a time period to choose. We find that the estimated cost of control is monotonically decreasing as we lengthen the time period in which the multiple loans must be observed from one quarter all the way to two years. This pattern is consistent with the theoretical intuition that with longer periods, borrower by time period fixed effects account for unobserved borrower quality less well. This leads to attenuation bias because, for example, when a borrower is doing better it will tend to both pay less for its loans and also surrender less control. The second issue is potential complementarity in terms across the multiple loans, such as the possibility that giving up control in one loan makes it less costly for the borrower to give up control in the other. However, we find no statistical difference in our estimates when we look at borrowers receiving more than two loans in the same year or receiving more than two loans in different sub-year periods. This suggests that complementarities across loans are not a first order determinant of the cost of control and further support the generalizability of our estimates to borrowers that only receive a single loan in a given year.

The second condition is that there is sufficient competition to lend to the borrower such that winning lenders must each offer the same surplus. To assess this condition, we construct several measures of potential lending competition in the borrower's geographic, industry, and size segment. For each measure, we find no evidence that more competition leads to significant differences in the estimated cost of control, consistent with sufficient competition for the theoretical condition to hold in our sample.

Holding non-price and control terms constant is the third required condition. Given that we control for these other terms (collateralization, amount, and maturity) together with rich dynamic interactions, this condition is of somewhat less concern. More interesting is whether these other terms interact with the cost-control tradeoff, consistent with some complementarity in contracting. We find insignificant interactions for all terms other than maturity. Longer maturity loans imply a moderately lower cost of control, consistent with the trust underlying longer relationships substituting for explicit contracting. However, our preferred estimates of the cost of control are bounded by the estimates implied by the interquartile range of maturity, which suggests that our preferred estimates are relevant to the typical loan.

Finally, we relax the simplifying assumption of a linear borrower indifference curve. This is potentially relevant not just because of nonlinearities, perhaps due to control becoming increasingly costly for the borrower to give up, but because of the possibility of corner solutions that might lead to bias in the implied slope. However, when we drop loans with an *ex ante* probability of violation of zero or one, or allow for some parametric or nonparametric nonlinearity, we find little difference in the estimated cost of control, suggesting that linearity is a reasonable approximation. Additionally, we explore a number of nonparametric specifications, all of which imply quantitatively similar estimates of the cost of control. These additional robustness checks provide support for our empirical model and baseline specification.

The two closest papers to ours in the control rights literature are Bradley and Roberts (2015) and Matvos (2013). These papers take different structural approaches to estimate the price of including one additional contractual term in the loan. In Matvos (2013), this takes the form of restrictive financial covenants, and in Bradley and Roberts (2015), this also includes dividend restrictions, collateral, asset sweeps, debt sweeps, and equity sweeps. We build on these papers in two ways. First, we take advantage of the recent literature on the measurement of covenant tightness (Murfin 2012; Demerjian and Owens 2016) to study the cost associated with the *ex ante* probability of violation rather than covenant inclusion. This measurement innovation is critical because the *ex ante* probability of violation varies significantly conditional on covenant inclusion. For example, among loans with covenants, the interquartile range of this measure in our sample is 4.1% to 95.7%. Second, we develop and apply a simple tradeoff theory that places restrictions on an easily estimated reduced form specification that recovers the cost of control.

Our findings have implications for our understanding of the pricing of debt claims and the value of contingent control rights. In particular, our findings complement recent work on the contractual determinants of debt prices, including the certification effect of share retention by the lead bank (Focarelli, Pozzolo, and Casolaro 2008), syndicate composition (Lim, Minton, and Weisbach 2014), the specialness of bank credit (Schwert 2019), the value of tying other bank services to loans (Drucker and Puri 2005), pipeline risk (Bruche, Malherbe, and Meisenzahl 2017), seniority (Bao and Hou 2017), and the role of upfront fees and state contingent interest rates (Berg, Steffen, and Saunders 2016). Specifically, our findings highlight the control discount implied by the equilibrium tradeoff of effective interest rates and control rights made by borrowers and lenders. Because control transfers between borrowers and lenders are cyclical (Murfin 2012; Bird, Ertan, Karolyi, and Ruchti 2019), our results imply a systematic relationship between the magnitude of the control discount and credit cycles. This relationship underscores the importance of adjusting for the control discount when drawing inference about borrower risk from loan interest rates.

We also contribute to the literature that focuses on covenants as an *ex ante* contracting tool and the *ex post* effects of covenant violations. In theory, covenants may be used to provide *ex ante* incentives for borrowers to make decisions *ex post* that are compatible with lender preferences, or to allow lenders to intervene when borrower creditworthiness declines (Jensen and Meckling 1976; Myers 1977; Smith and Warner 1979; Dewatripont and Tirole 1994; Rajan and Winton 1995; Dichev and Skinner 2002). Covenants are frequently used and set tightly, particularly for riskier firms with fewer investment opportunities (Demiroglu and James 2010; Murfin 2012; Demerjian and Owens 2016). The empirical literature has documented a series of economically significant *ex post* consequences of covenant violations for borrowers, spanning real investment, capital-raising, governance, and more (see, e.g., Chava and Roberts 2008). The negative effects for borrowers can be summarized by significant stock market declines that follow covenant violation announcements, which may be as large as 3% of equity value (Beneish and Press 1993). However, consistent with practitioner anecdotes, recent evidence indicates that control rights are rarely exercised (Zinbarg 1975; Bird, Ertan, Karolyi, and Ruchti 2019), calling into question the price that a lender would be willing to pay for *ex ante* tight covenants. Our estimates of the cost of control contribute to our understanding of the equilibrium transfer and use of contingent control rights and so discipline theories on covenant use.

The paper proceeds as follows. Section 2 outlines our theoretical framework and identifies the key necessary underlying theoretical conditions. Section 3 describes the data and empirical specification. Section 4 presents our main results on the cost of control. Section 5 provides further investigation of the theoretical conditions. Section 6 concludes.

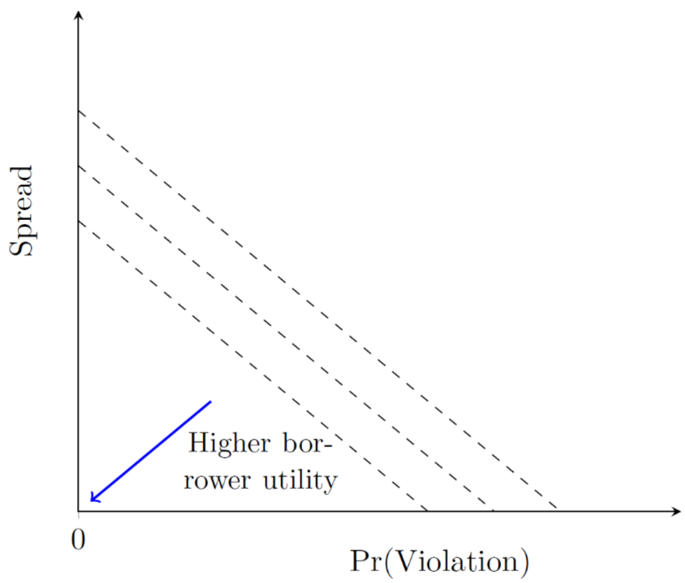
2 Theory

In this section, we develop a simple theoretical argument that isolates the equilibrium cost-control tradeoff made by lenders and borrowers. Our goal is identify aspects of this theoretical argument that can be estimated with observational data of corporate loan contracts. This theoretical discussion depends on a few key conditions that are necessary for the logic underpinning our empirical specification to hold.

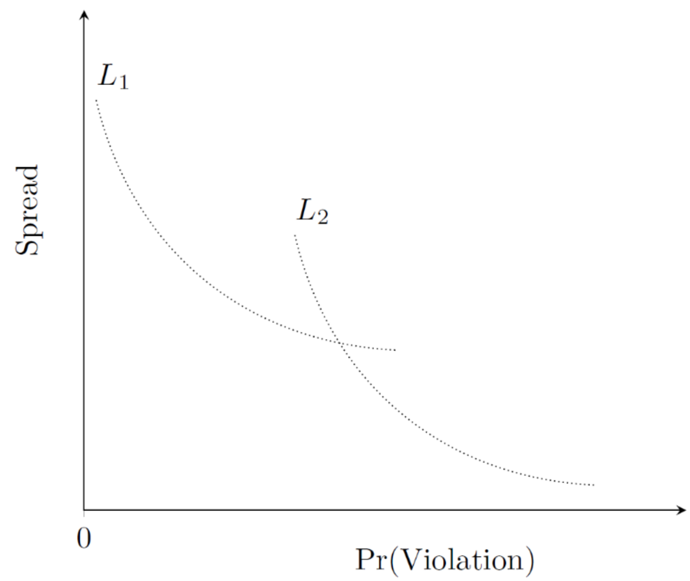
2.1 Theoretical Framework

Let a loan be a combination of the following characteristics, the spread, m, the probability of control rights being transferred to the lender, C, and other terms of the loan, represented by ϕ. A loan contract offered by lender i may therefore be described by the vector $l_i[m_i, C_i, \phi_i]$.

The borrower trades off the spread, m with the amount of contingent control rights given up to the lender in the form of financial covenants, C. All else equal, the borrower prefers to pay a lower spread and to retain control. Given this tradeoff, indifference curves representing the terms of a loan can be represented in two dimensions as follows.

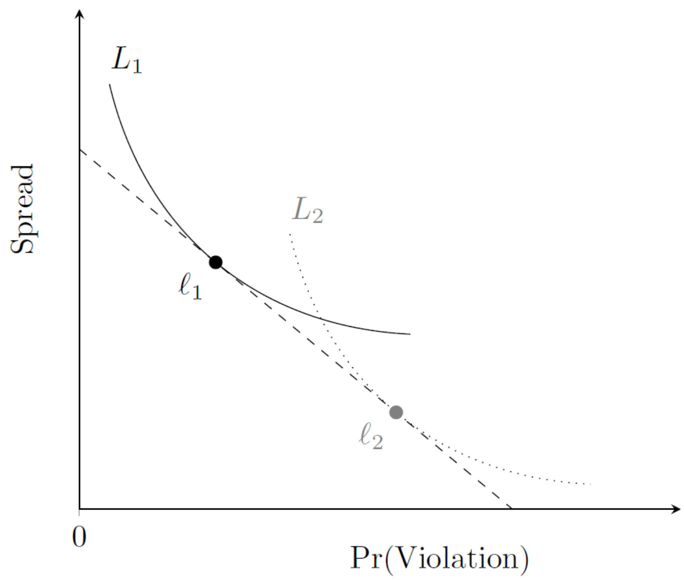


The lender also trades off the spread it receives with the probability of obtaining control. Of course, conversely to borrower preferences, the lender prefers the loan to have a higher spread and more control rights. Different lenders may exhibit a different willingness to trade off these terms as well, perhaps due to different screening or monitoring technologies. Given this tradeoff, indifference curves for two potential lenders, L_1 and L_2, can be represented in cost-control space as follows.



Because both the borrowers and the lenders trade off spread and control, an interior solution requires that a realized loan reflects the same marginal rate of substitution between spread and control for the borrower and the lender. In this case, the realized loan must therefore be the point of tangency between the two respective indifference curves. Assuming all necessary regularity conditions to represent indifference curves with utility, the tangent indifference curves reflect the utility of the loan to the borrower and to the lender, respectively. The further out from the origin is the borrower's indifference curve, the lower the utility.

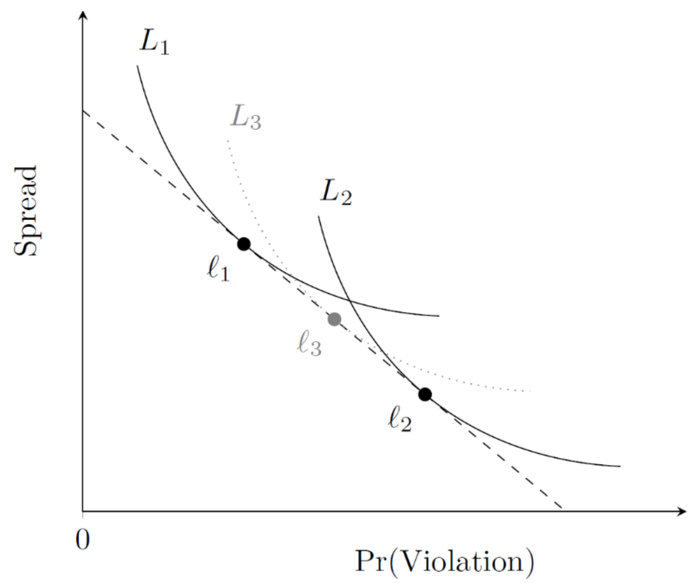
To explain the characteristics of equilibrium loans observed by the econometrician, we consider competition between lenders. Suppose a borrower wishes to obtain a loan and two lenders, Lender 1 and Lender 2, each make an offer.[2] To win the business, Lender 1 only needs to offer terms that provide the amount of borrower surplus (i.e., utility) that is at least as large as what would be provided by Lender 2's loan offer, l_2. Therefore, if Lender 1 wins, the observed loan, l_1, will lie on the tangency between L_1 and the borrower's indifference curve corresponding to the surplus offered by Lender 2. This equilibrium is shown below:



While this framework shows how lender competition leads to a particular example of cost and control being traded off in equilibrium, our empirical objective is to trace out this tradeoff. To do that, we need multiple observations from the same borrower indifference curve; that is, borrowers who receive multiple loans. We thus introduce a third lender. Following the same logic as above, the winning loans of Lender 1 and Lender 2 will be determined by the

[2] We describe this example with two lenders without loss of generality. There may, of course, be more lenders that are willing to finance a loan for the borrower. In this case, we can think of Lender 1 and Lender 2 as the lenders that make the two best offers.

tangencies of their respective indifference curves with the borrower indifference curve determined by the surplus of the loan offered by Lender 3.



Given the realized loans we observe, l_1 and l_2 from Lenders 1 and 2, we have two pairs of coordinates in cost-control space (m_1, C_1) and (m_2, C_2) that lie on the same borrower indifference curve. Thus if we are interested in measuring the cost of control in spread for a single borrower, then we can find it as follows.

$$Cost\ of\ Control_i = \frac{m_1 - m_2}{C_1 - C_2}. \tag{1}$$

In the subsequent sections, our goal is to estimate a linear approximation of this slope, averaged over borrowers, which we interpret as the spread discount that lenders must offer the average borrower to acquire a given level of contingent control rights.

2.2 Necessary Conditions

In this section, we outline a series of necessary conditions such that the empirical counterpart of the cost-control slope actually corresponds to the cost of control, as embodied in the borrower's tradeoff between the cost of the loan and the amount of control given up. Essentially, the following four conditions provide restrictions that we will use in designing our empirical specification in Section 3.

Condition 1. *Multiple loans for the same borrower.*

In one sense, this condition is just a function of having sufficient observations for the calculation of a slope, but the more interesting part of the condition is the definition of "same borrower." If the second part were relaxed, the requirement of observing multiple loans would be trivial. The reason this is so important is that the theoretical logic only works to make the slope interpretable as the cost of control if the indifference curves of the lenders offering the two loans are both tangent to the same borrower indifference curve. Importantly, once this condition is satisfied, an appropriate empirical specification, as we describe in Section 3, will simply average out all of these borrower-specific slopes to get at the average cost of control in the market. The fact that one would want to look within-borrower versus across borrowers is intuitive; the tension comes from the question of whether all loans to the same borrower reflect the same indifference curve. The further apart in time are these loans, the less likely this is to be true. On the other hand, the closer together these loans are required to be, the fewer observations will be available for estimation.

On top of this statistical power issue, the potential tradeoff of any empirical implementation of the theory is that the closer the specification gets to using only identifying variation holding borrower quality fixed, the more one must think about what simultaneity might do to the tradeoff between spreads and control. In one sense, this just implies a potential limitation to the generalizability of our cost estimates, since we can estimate the cost only using borrowers receiving multiple loans.[3] However, it is also interesting to consider how the borrower might think differently about giving up control when doing so across multiple loan contracts (and often multiple lenders or loan syndicates).

Condition 2. *Sufficient lender competition.*

The first implication of this condition is simply that the theoretical argument above falls apart without multiple lenders. More generally, the theoretical argument concerning how lenders compete with each other seems intuitively to be more likely to hold if there are many lenders, rather than the knife edge case of only two or three. For example, one might be concerned about lender collusion, and such coordination becomes harder to sustain the more potential lenders there are.

A second key premise of the theoretical argument above is that all of the winning lenders must offer the borrower the same surplus to win the borrower's business. This second price auction argument depends on lenders competing on a relatively level playing field, such that the

[3] It is important to note that this caveat about generalizability is implicit in most of the related literature, since the inclusion of borrower fixed effects is a common way to deal with unobserved borrower heterogeneity.

terms of the (offered) loan identify the surplus created by the loan. However, if a particular lender differs from other lenders along some dimension that enters the borrower's objective function explicitly, such as reputation or the disclosure requirements imposed on borrowers, that lender will not need to offer the same surplus to the borrower. For example, if borrowers benefit from being associated with high quality lenders, the lender with the best reputation will not have to pay as much (in terms of a discount in spreads) to secure the same level of control as other lenders.

On the other hand, in order for estimation of the slope to be feasible, some lender heterogeneity, for example driven by differences in lending technology or strategy, is necessary. This is so that different sets of loan terms are agreed across different loans, despite the borrower's preferences being held fixed. Since the natural way to deal with the main concern of this condition is to control for lender heterogeneity in various ways, this caveat warns against eliminating too much such heterogeneity.

Condition 3. *Other terms held constant.*

An implicit assumption made in the above model discussion is that the only first-order relevant terms of the loan are the cost of the loan and the amount of control. Of course, in practice other aspects of the contract, such as the amount of the loan and its maturity, are also relevant. In order to be able to interpret the slope in equation (1) as the cost of control, it is important that these other terms are held fixed as long as they are correlated with the spread or control. For example, if larger loans involve more control in equilibrium, and larger loans also happen to be more costly, all else equal, then the slope implied by equation (1) will be a downward biased measure of the cost of control. That is because when we see high control, the spread reflects not just the purchase of control (pushing the spread down), but also the large amount (pushing the spread up).

Condition 4. *Linear indifference curve.*

The final condition is that the borrower's tradeoff between spreads and control is linear. Because nonlinearities must exist for interior solutions (i.e., other than no control or full control), this is largely a matter of simplicity for our graphical arguments and empirical specifications. Empirically, linearity is a common assumption. On one hand, one can view our estimates as reflecting a linear approximation of the true borrower indifference curve, and on the other, the empirical model is easily extended to relax this assumption, as we do in Section 5.

3 Data and Empirical Methodology

3.1 Data

The data for our investigation come from two main sources: firm financials from COMPUSTAT and loan-level information from Thomson Reuters' DealScan. We link these data using Michael Roberts' link table to match DealScan borrowers to COMPUSTAT and Michael Schwert's link table to match DealScan banks to COMPUSTAT. Finally, we obtain data on the *ex ante* probability of covenant violations from Peter Demerjian's website[4] and the total cost of borrowing from Tobias Berg's website.[5] Our estimation sample covers the period from 1994 to 2012, since overlapping data on the total cost of borrowing and the probability of covenant violations are only available for this period. For loans that meet the sample inclusion criteria of Demerjian and Owens (2016) but have no financial covenants reported in Dealscan, we assume the probability of violation to be zero. In the full sample with non-missing values of the total cost of borrowing and the probability of violation, we have 27,406 distinct loan facilities, covering 4,838 borrowers and 216 lenders. Table 1 shows the sample selection procedure in detail.

Two key preceding measurement innovations allow us to estimate the cost of control: the total cost of borrowing (Berg, Steffen, and Saunders 2016), or *TCB*, and the probability of violation (Murfin 2012; Demerjian and Owens 2016), or *Pr(Violation)*. *TCB* is a measure of the cost of borrowing that takes into account loan price factors beyond the all-in-drawn spread.[6] For example, it captures potentially complex and state contingent loan fee structures, dynamic rate adjustments, and contingent rates for drawn and undrawn funds. *Pr(Violation)* builds on Murfin (2012), which develops a measure of *ex ante* covenant strictness based on a multivariate normal distribution of covenant slack that accounts for covariation between covenant types, by

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[4] http://faculty.washington.edu/pdemerj/data.html

[5] http://www.tobias-berg.com/index.php/research/

[6] In untabulated results, we find that the magnitude of our baseline estimate is attenuated when we use alternative measures of the cost of borrowing based on the all-in-drawn spread or the all-in-drawn spread plus upfront fees. This attenuation is consistent with the importance of measuring complex fee and rate structures that otherwise make the all-in-drawn spread a noisy proxy for the cost of borrowing.

simulating the joint dynamics of covenant slack across covenant types.[7] It therefore provides a nuanced measure of the probability of violation that predicts actual violations far better than alternatives like the number of covenants. This is intuitive because the inclusion of a particular covenant is not informative about the relative tightness of the covenant or how the financial metric that underlies that particular covenant covaries with other financial metrics that may or may not have covenants written on them. This measure is especially important to our analysis because it allows us to interpret our estimates as the cost of moving from 0% to 100% likelihood of a covenant violation and, therefore, a transfer of control rights.

Panel A of Table 2 provides summary statistics of the main variables of interest, including *TCB* and *Pr(Violation)*. The average loan in our sample has an *ex ante* probability of covenant violation of 23.7%, though this understates the typical intensity of *Pr(Violation)* since about half of the loans in our sample have values of zero. Among the 12,919 loans with positive values of *Pr(Violation)*, the average is 45.3% and the median is 26.0%, suggesting that the conditional distribution is positively skewed. The average *TCB* is 155 basis points and has a standard deviation of 140 basis points. The *TCB* distribution is also positively skewed. The loans in our sample exhibit characteristics that are representative of other studies that use data at the intersection of DealScan and COMPUSTAT. The average loan amount is $429M, but has an interquartile range of $75M to $500M. Loan maturities are tightly distributed around a mean

[7] In untabulated results, we replace *Pr(Violation)* from Demerjian and Owens (2016) with its parametric counterpart, *Strictness*, as in Murfin (2012), and we find somewhat larger estimates in our baseline specification.

of 4 years; the interquartile range is 3 years to 5 years. Finally, 57.2% of loans are secured by collateral.

Because our empirical strategy relies on within-borrower-year variation in *TCB* and *Pr(Violation)*, a potential concern is whether our estimates can be generalized to the subset of borrowers that issue only one loan during any given year that they obtain any loan financing. To investigate this issue, we present Panel B of Table 2, which compares loans issued by borrowers that obtain a single loan to those issued by borrowers that obtain multiple loans within a given calendar year. Only 6,704 loans are issued in single loan borrower-year tuples, representing about one-quarter of our sample. This table shows that loans issued by borrowers that obtain multiple loans within a given year tend to be larger and have longer maturities, but also have higher *TCB*, on average. They are also more likely to issue term loans and secure their loans with some form of collateral. Interestingly, though, the two subsamples of borrower-years have indistinguishably different *Pr(Violation)*, which suggests that the selection of financial covenants may not differ significantly across the two subsamples. Overall, Panel B of Table 2 highlights the importance of controlling for loan characteristics, which vary across borrowers that are selected into and out of our sample.

3.2 Empirical Methodology

The goal of the empirical specification is to estimate the slope derived in Section 2.1. The first key to this process is choosing an appropriate fixed effects structure so that the slope we estimate indeed corresponds to the tradeoff outlined in the model.

Our regression model is as follows:

$$TCB_{blt} = \alpha + \beta \cdot Pr(Violation)_{blt} + u_{terms \times t} + u_{purpose \times type \times t} + u_{b \times t} + u_{\ell \times t} + u_{b \times \ell} + \varepsilon_{blt} \quad (2)$$

where subscripts b, l, and t represent borrower, lender, and year, respectively. The rich fixed effects structure is key to satisfying the theoretical conditions outlined in Section 2.2. First, we include borrower by year fixed effects to satisfy the requirement that identification comes from multiple loans for the same borrower, where the unobservable characteristics of the borrower are allowed to vary by year. Second, we include loan type and purpose, and control for loan terms (the amount, maturity and a collateral dummy) so that we can recover the relationship between cost and control, holding the other characteristics of the loan fixed. We further interact these loan characteristics with each other as well as with year in case the underlying economics that they account for vary over time. Finally, we include lender fixed effects, and interact them with year, with borrower, and with the above set of loan characteristics (and interactions) to ensure sufficient lender homogeneity to support the assumption that different lenders offer the same utility if they are going to win the business of a particular borrower. While this is set of fixed effects leaves us with quite a saturated model that we believe does a good job of satisfying the theoretical conditions in Section 2.2, we also return to each of these conditions with further tests in Section 5.

Before we present the results from estimating equation (2) in the following section, we start by illustrating the importance of the fixed effects structure graphically. In Figure 1, we show a series of scatter plots of the total cost of borrowing against the *ex ante* probability of violation. Panel A of Figure 1 shows a clear positive correlation—loans with a higher probability of violation are actually more costly on average. This is in contrast with the theoretical discussion of Section 2. The key issue is that when not restricting to within-borrower-time variation in loan terms, borrower heterogeneity swamps the underlying tradeoff of the borrower giving up some control in exchange for a discount on spreads. Specifically, better borrowers will have both low spreads and give up little control, whereas worse borrowers will have higher spreads and have to give up a lot of control, giving rise to a positive correlation between the two terms. To demonstrate that this borrower heterogeneity is driving the pattern in Panel A of Figure 1, in Panels B and C of Figure 1 we present scatter plots of the total cost of borrowing against the residual probability of violation. Panel B presents estimates that control for *Borrower* × *Year*, *Type* × *Purpose* × *Year,* and *Interacted Terms* × *Year* fixed effects, and Panel C adds *Lender* × *Year* and *Borrower* × *Lender* fixed effects to the specification of Panel B. In Panels B and C, we recover a clear negative correlation—control is indeed costly for lenders to purchase.

4 Main Results

In this section, we present the results of estimating equation (2) with successively more restrictive fixed effects and then document the robustness of the findings to alternative functional form choices for each of our key variables.

4.1 Baseline specifications

Table 3 presents estimates from equation (2), and then we subsequently add the lender fixed effects and interactions in Table 4 discussed in the next subsection. In all tables, note that to consistently describe the economic magnitude of each of our estimates, we define the "cost of control" implied by each specification as the difference in total cost of borrowing (measured in basis points) going from an *ex ante* probability of violation of zero to an *ex ante* probability of violation of one.

To start, column (1) of Table 3 aligns with the graphical evidence from Panel A of Figure 1. The implied cost of control is -55 basis points, and remains negative, though becoming statistically insignificant from zero, as we add year, loan type and purpose, and loan term fixed controls. When including borrower fixed effects, the estimate of the cost becomes positive, and after also including borrower by year fixed effects, the estimate becomes 30.3 basis points. After including all of the type, purpose, and loan term interactions (including allowing all of these to vary by year), the estimated cost of control increases to 39.7 basis points.

4.2 Controlling for lender heterogeneity

We next address unobserved lender heterogeneity in Table 4. After reproducing our specification from column (10) of Table 3 in column (1) of Table 4, we add lender by year fixed effects in column (2), and find that the estimated cost of control increases to 44.9 basis points. Additionally including lender by borrower fixed effects leads to a slight increase in the estimate, to 46.1 basis points. For an additional check on the potential issues due to lender heterogeneity, in Table 4, we also present specifications that interact lender fixed effects with both type by purpose by year and with interacted terms by year. The former slightly increases the estimated cost of control and the latter slightly decreases it.

Notably, all of the specifications that do a reasonable job of meeting the theoretical conditions outlined in Section 2.2 (i.e. beginning with column (9) in Table 3 and covering all of Table 4) yield estimates falling in the range of 40-50 basis points. In picking a preferred specification to use as a baseline for the rest of the paper, we want to trade off controlling for some lender heterogeneity while not overly saturating the model. This is because, as we discuss above, some heterogeneity in lender preferences is desirable for actually providing the variation needed for identification. Specifically, we choose column (3) of Table 4 as our preferred specification. This specification accounts for changing lender preferences over time and for unobserved aspects of the lender-borrower match or relationship, in the addition to all of the controls from Table 3.

Our preferred estimate of the cost of control is therefore 46.1 basis points. That is, we find that, on average, lenders must offer borrowers a discount of 46.1 basis points on the total cost of borrowing in order to gain full control. Acquiring full control would mean that the *ex ante* probability that the borrower will be in breach of the loan, so that the lender will get control rights, is effectively one. In practice, this would happen if the covenant ratios were chosen such that the borrower were already in breach at initiation of the loan. For the average loan in the sample, which has an ex ante probability of violation of 0.237, our estimate implies that the lender offers a discount of 10.9 basis points to acquire this level of control.

To put the estimated cost of control in context, it is helpful to think of it as implying payments for control rights. These payments can be compared with average costs of borrowing, loan sizes, or equity values. Since the average total cost of borrowing in the sample is 155 basis points, acquiring full control would cost the lender 46.1 bps / 155 bps = 29.7% of the loan spread. Since the average loan has a *Pr(Violation)* of 23.7%, lenders offer a (23.7% × 46.1 bps) / 166 bps = 6.6% discount. In the sample of loans with non-zero values of *Pr(Violation)*, the average *Pr(Violation)* is 45.3%, which suggests that conditional on including covenants in the loan contract, lenders offer a (45.3% × 46.1 bps) / 156 bps = 13.4% discount.

4.3 Functional form robustness

For all of the preceding results, we hold the measurement of the two key variables constant. That is, we measure the total cost of borrowing as the spread equivalent in basis points, and we measure the amount of control as the probability that the borrower will be in breach. While we believe that these choices follow the spirit of the theoretical framework, we also want to check whether similar estimates of the cost of control obtain from alternative choices by checking a variety of alternative functional form assumptions for the relationship between these two variables. Specifically, we use the percentile, decile, natural log or winsorized (at the 1% level) total cost of borrowing, and the percentile, tercile, or decile of the probability of violation.

Table 5 presents our estimates for each combination of baseline and alternative choices for each variable (i.e. our baseline specification and 19 alternatives) otherwise following the specification of column (3) of Table 4. Overall, the results are consistent in statistical significance with our earlier results. More importantly, they imply relatively similar magnitudes, where the range they cover is approximately centered around our preferred estimate.

5 Investigating theoretical conditions

The empirical specification, and particularly the fixed effects structure, are designed to satisfy the four theoretical conditions in Section 2.2, such that the estimated coefficients correspond to the cost of control. In this section, we conduct further investigation of each of these conditions with two distinct objectives. The first is to establish the robustness of our preferred estimate of the cost of control from Table 4. The second is to provide some preliminary evidence on both the external validity of these estimates as well as the economic importance of some potential extensions to the basic model.

5.1 C1—Multiple loans for same borrower

This condition is mechanically satisfied if the estimation of the relevant coefficient is feasible with our fixed effects structure, in the sense that if we did not observe multiple loans for the same borrower in the same time period, we could not recover the coefficient of interest. However, the condition does raise several important questions. The first is how long of a time period to choose, with one extreme being the whole sample period and the other being a single day. The underlying tradeoff is that a longer period means that borrower by time period fixed effects do a worse job of controlling for borrower heterogeneity (as long as it is time varying), while the shorter the period, the less variation is left for identification.

We investigate the choice of time period in Table 6 using our preferred specification from column (3) of Table 4. After first reproducing this specification in column (1), in column (2) we find that switching from calendar years to fiscal years, so that the "year" lines up more directly with changes in information about the borrower, does not matter at all for our estimate. More interestingly, in subsequent columns, we vary the time period used for defining the fixed effects from as short as a quarter to as long as two years. We find that the estimated cost of control is monotonically decreasing with time period length, moving from 76.0 basis points (at a quarter) to 22.8 basis points (at two years). This is consistent with the endogeneity issue suggested by Figure 1. As the time period gets longer, the downward bias from the fact that better borrowers will tend to both pay less for their loans and also surrender less control becomes more pronounced. This logic extends to within borrower variation: borrowers will obtain loans with lower spreads and surrender less control when they are doing well, but pay more for their loans and surrender more control when they are doing poorly. Thus our preferred estimate may somewhat understate the true cost of control, though requiring multiple loans in very short time periods may lead to concerns about generalizability.

The second question raised by this condition is the potential for complementarity in terms across loans when the loans are happening close together in time. It is not a priori clear what effect such complementarity might have on our estimates. On the one hand, giving up control in one loan may make it less costly to give up control on the other if the former induces borrowers to make operational or financial changes that mitigate the consequences of giving up control. Alternatively, the effect could go in the other direction if giving up control to multiple lenders makes the occurrence of a breach more damaging, say because of having to satisfy stakeholders with divergent views or because the borrower has limited resources to satisfy lenders.

In Table 7, we provide some evidence on this kind of complementarity by allowing for the cost of control to vary if either the borrower gets more than two loans in the same period, or if the borrower gets at least two loans in the same month, quarter, or six month period. The idea is that if complementarities exist, they should be particularly pronounced in these cases. However, we find little evidence for significant complementarity—in columns (2)-(5) of Table 7, the interaction between the ex ante probability of violation and a dummy variable for "busy" borrowers is statistically indistinguishable from zero. We draw two conclusions from these findings. First, complementarities in terms across loans are not a first order determinant of the cost of control. Second, it appears reasonable to generalize our estimated cost to the full set of borrowers, many of whom do not get multiple loans in the same year.

5.2 C2—Sufficient lender competition

The specifications from Table 4, which introduce fixed effects to control for many sources of unobserved lender heterogeneity, provide evidence that potential lenders are competing on a level enough playing field that they must make comparable offers to win the business of the borrower. However, the other key aspect of our theoretical approach to lender competition is that there is sufficient competition for the second price auction framework to be descriptive. While this is likely to be true on average since we see many different lenders active in the data and because we require there to be multiple loans to the same borrower in a particular year, for some borrowers there may be relatively little competition due to few viable lender options. These borrowers may have less bargaining power, especially if there is a single dominant lender in their particular market, leading to a bias in the estimated cost of control. We investigate this issue by allowing the cost of control to vary according to the level of competition in the borrower's market. We define the level of lender competition in the relevant market as the number of lenders active in the industry (two-digit SIC) and size bracket (i.e., 0-$150M; $150M-$500M; >$500) for three different levels of geographic granularity: the state, city, and zip code of the borrower's headquarters. We also define three dummy variables equal to one if the number of lenders in each market is above the median in the sample.

In Table 8, we present estimates of the cost of control and how it varies by the level of competition by including interactions with either the continuous or discrete measures of lender competition described above, using our preferred specification as in column (3) of Table 4. Regardless of which measure we use, the coefficients on the interactions are always statistically indistinguishable from zero and also quite small in magnitude, such that the estimated cost of control is almost identical in relatively competitive and relatively uncompetitive lending markets. We thus conclude that there is in practice sufficient lender competition for our approach to recover the cost of control. This finding is intuitive because even in the absence of lenders, borrowers are likely to have alternative financing options that achieve similar utility.

5.3 C3—Other terms constant

While the rich set of fixed effects and interactions in our specification are designed to hold the effects of other loan terms constant, it remains possible that there are interactions between these other terms and the cost of control. We investigate this possibility in Table 9, starting with whether the loan is secured. We find a negative but statistically insignificant interaction term of secured with the probability of violation. When we allow for an interaction with the amount of the loan, either measured in billions of dollars or as a dummy variable for an amount above the sample median, we again see statistically insignificant results.

For the third important term, the maturity of the loan, we see a more interesting pattern. Interactions with either the maturity in years or a dummy variable for a maturity above the sample median are both positive and statistically significant. This would mean that longer loans are associated with a lower cost of control, consistent with the trust underlying longer relationships substituting for explicit contracting. However, the economic significance of this heterogeneity is relatively weak, because maturity does not vary much. Going from the 25th percentile of maturity (3 years) to the 75th percentile (5 years) only yields a range for the cost of control of 39.2 to 54.7 basis points, which does not deviate much from our preferred estimate of 46.1 basis points. Together, this set of results suggests that the controls in our preferred specification are doing a good job of holding other important loan terms fixed and further, that complementarities across loan terms do not significantly affect borrowers' tradeoffs between the cost and the loan and how much control they give up.

5.4 C4—Linear IC

In our theoretical framework, we represent the borrower's indifference curve as linear for ease of exposition, and our empirical specification imposes a linear relationship between the total cost of borrowing and the ex ante probability of violation. While we are happy to interpret our estimates as reflecting the linear approximation of a potentially nonlinear relationship, in Table 10, we also check for evidence of interesting nonlinearities in the relationship. Specifically, we experiment with including a quadratic term, though we find that its difference from zero is not statistically significant. We also try allowing the cost of control to vary across terciles of the probability of violation and find similar results, with some evidence of a relatively lower cost for intermediate levels of control.

The other functional form issue that may be relevant is the possibility of corner solutions which might lead to bias in the (linear) slope. To see whether this is a significant problem, we replicate our preferred specification from Table 4, dropping loans with either a probability of violation of zero or one (or both). In each case, the estimated cost of control is statistically indistinguishable from our preferred estimate, varying from 34.3 basis points to 43.5 basis points. Thus, even though corner solutions are not uncommon in the data (e.g. many loans do not involve giving up any control), they appear to fit on the same average borrower indifference curve.

6 Conclusion

We develop a simple tradeoff theory based on competition among lenders to finance a borrower's loans. Because successful lenders must only offer terms that beat the surplus generated by the borrower's best alternative offer, observed loans generate the same surplus for the borrower. When the borrower receives multiple loans in the same year, these loans' cost and control terms trace out the borrower's indifference curve. We estimate a linear approximation of the average borrower's indifference curve using exactly this within-borrower variation in cost and control terms, holding other terms fixed. Our estimates suggest that the cost of control is 46.1 basis points. Among loans with financial covenants, the average *ex ante* probability of violation implies control payments that correspond to a 13.4% discount on the total cost of borrowing (Berg, Steffen, and Saunders 2016). This finding has implications for our understanding of loan pricing and the value of control rights in the private loan market. For example, credit tightening dynamics (e.g., Murfin 2012) imply that these control discounts are cyclical, which suggests that unadjusted spreads are an attenuated measure of how sensitive the cost of borrowing is to credit cycles. Additionally, our empirical methodology could be extended or applied in different settings to understand equilibrium contracts.

Appendix A: Variable Definitions

Variable Name	Definition
Total Cost of Borrowing (bps)	Measured as in Berg, Saunders, and Steffen (2016).
Pr(Violation)	Measured as in Demerjian and Owens (2016).
Amount ($B)	Loan amount.
Maturity (yrs)	Loan maturity in years.
Secured	Indicator that equals one if Dealscan determines that the loan is secured by collateral.
Loan purpose	A set of indicators that equal one if Dealscan determines that the primary purpose of the loan is related to investment (i.e., "Capital expend."), M&A (i.e., "Takeover", "Merger", "Acquis. line"), working capital (i.e., "Work. cap."), corporate purposes (i.e., "Corp. purposes"), or other.
Loan type	Revolvers and term loans are included. An indicator that equals one if the loan package includes at least one term loan is included.

Appendix B: Covenant Calculations

Covenant Name	Calculation (Compustat codes)
Debt-to-EBITDA	(DLC + DLTT) / EBITDA
Debt-to-Equity	(DLC + DLTT) / SEQ
Debt-to-Tangible NW	(DLC + DLTT) / (AT – INTAN – LT)
Leverage	(DLC + DLTT) / AT
Current ratio	ACT/LCT
Quick ratio	(RECT + CHE) / LCT
Cash interest coverage	EBITDA / INTPN
Interest coverage	EBITDA / INTPN
Debt service coverage	EBITDA/(INTPN + Lagged DLC)
Fixed charge coverage	EBITDA/(INTPN + Lagged DLC + XRENT)
Net worth	AT – LT
Tangible net worth	AT – INTAN – LT
EBITDA	EBITDA

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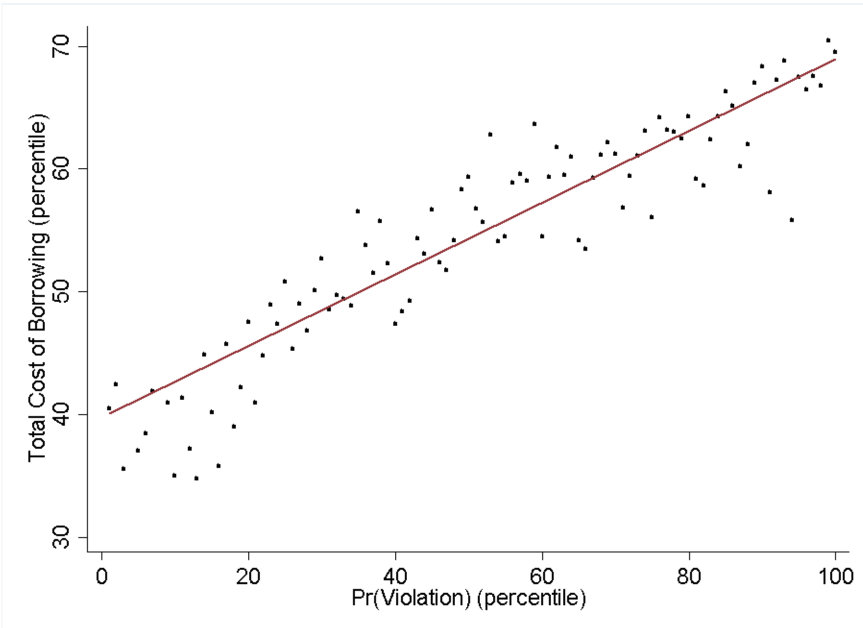
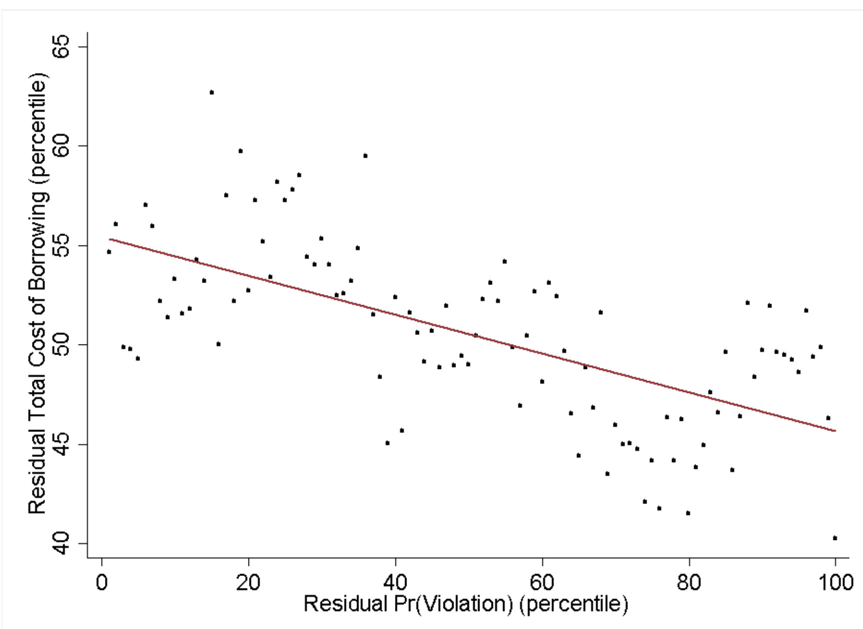
Figure 1. Identification

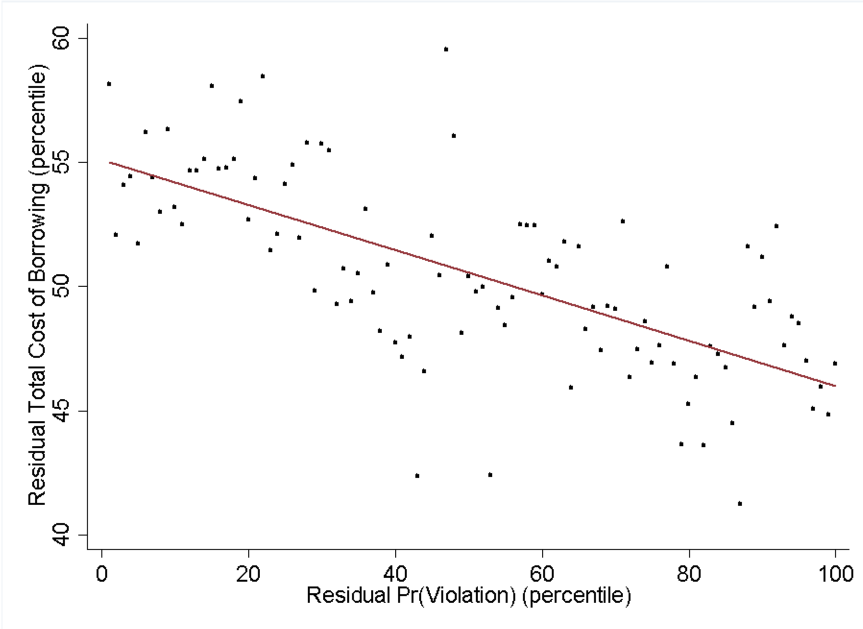
This figure plots binned residuals of TCB and Pr(Violation) from projections of each variable on nothing (Panel A), *Borrower* × *Year*, *Type* × *Purpose* × *Year*, and *Interacted Terms* × *Year* fixed effects (Panel B), or *Borrower* × *Year*, *Type* × *Purpose* × *Year*, *Interacted Terms* × *Year* , *Lender* × *Year* , and *Borrower* × *Lender* fixed effects (Panel C). These projections correspond to models presented in Column (1) of Table 3 (Panel A), Column (10) of Table 3 (Panel B), and Column (3) of Table 4 (Panel C).



Panel A. Univariate model



Panel B. Baseline model (saturated)



Panel C. Lender heterogeneity model (saturated)

Table 1. Sample Selection

This table presents the sample selection procedure. Because data on the total cost of borrowing is available only from 1995 to 2012, we limit our overall sample period to that window. To generate our main estimation sample, we first identify all loan facilities between 1995 and 2012, merge this with the *LenderShares* data table of Dealscan and drop loans with missing lead banks. We follow Bharath, Dahiya, Saunders, and Srinivasan (2011) and define a lender as lead bank if it receives a lead arranger credit, has a role of "Agent", "Admin. agent", "Arranger", or "Lead bank", or if it is the sole lender. We then merge this interim dataset with the Dealscan-Compustat Linking Database (Chava and Roberts 2008) and, finally, with *Total Cost of Borrowing* data made available by Berg, Saunders, and Steffen (2016) and *Pr(Violation)* data made available by Demerjian and Owens (2016).

	Facility–Lead Bank Pairs	*Facilities*	*Packages*	*Borrowers*
Dealscan 1995 – 2012		302,824	207,477	78,799
Non-missing lead bank (≥ 1)	336,907	−49,631	−29,468	−6,452
Successful COMPUSTAT link	−165,856	−124,976	−84,663	−47,473
Non-missing TCB & Pr(Violation)	−143,645	−107,115	−78,148	−20,036
Main estimation sample:	**27,406**	**21,102**	**15,198**	**4,838**

Table 2. Summary Statistics

Panel A of this table presents summary statistics for the regression variables of interest. Variable definitions are in Appendix A. Panel B of this table presents a comparison of these characteristics for two subsamples of loans in our data: those borrower-year observations with just one loan, and those borrower-year observations with multiple loans.

Panel A. Summary Statistics

	Obs.	Mean	SD	P25	Median	P75
Pr(Violation)	27,046	0.237	0.379	0	0.004	0.311
Pr(Violation): (0, 1]	14,331	0.453	0.420	0.041	0.260	0.957
TCB (bps)	27,046	155	140	51	113	214
TCB (bps)/Pr(Violation): (0, 1]	14,331	156	123	67	122	209
Amount ($B)	27,046	0.429	0.859	0.075	0.2	0.5
Maturity (yrs.)	27,046	4.130	1.955	3	5	5
Secured	27,046	0.572	0.495			
Number of Covenants	27,046	1.26	1.54	0	0	2
Number of Covenants/Any Covenants	12,892	2.68	1.12	2	3	3
Term loan	27,046	0.491	0.500			
Purpose:						
Working capital	27,046	0.179	0.383			
Corporate purposes	27,046	0.305	0.461			
M&A	27,046	0.168	0.374			
Investment	27,046	0.005	0.071			
Other	27,046	0.342	0.475			

Panel B. External Validity Check

	Single Loan	>1 Loan	Difference	*p-value*
Obs.	6,704	20,702		
Pr(Violation)	0.236	0.237	-0.001	0.833
TCB (bps)	115	168	-53	<0.001
Amount ($B)	0.259	0.484	-0.225	<0.001
Maturity (yrs.)	3.440	4.354	-0.914	<0.001
Secured	0.512	0.591	-0.079	<0.001
Term Loan	0.179	0.592	-0.413	<0.001
Purpose:				
Working capital	0.242	0.158	0.084	<0.001
Corporate purposes	0.294	0.309	-0.015	0.019
M&A	0.091	0.193	-0.102	<0.001
Investment	0.004	0.005	-0.001	0.235
Other	0.369	0.334	0.035	<0.001

Table 3. The Cost of Control: Baseline Estimates

This table presents fixed effects regression estimates of *TCB*, the total cost of borrowing as in Berg, Saunders, and Steffen (2016), on *Pr(Violation)*, the *ex ante* probability of covenant violation as in Demerjian and Owens (2016). *Terms* include an indicator that identifies loans secured by collateral, loan maturity, and loan amount. All other variables are defined in Appendix A. Heteroskedasticity-robust standard errors are clustered by lead bank, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Pr(Violation)	54.96***	59.17***	32.57***	7.67***	-7.27**	-30.31***	-30.52***	-36.67***	-40.07***	-39.70***
	(7.85)	(7.27)	(2.74)	(2.49)	(3.16)	(11.88)	(11.96)	(13.10)	(12.67)	(13.54)
Controls:										
Year		X	X	X	X	X	X	X	X	X
Type			X	X	X	X	X	X	X	X
Purpose			X	X	X	X	X	X	X	X
Terms				X	X	X	X	X	X	X
Borrower					X	X	X	X	X	X
Borrower × *Year*						X	X	X	X	X
Type × *Purpose*							X	X	X	X
Interacted Terms								X	X	X
Type × *Purpose* × *Year*									X	X
Interacted Terms × *Year*										X
Adjusted R^2	0.0220	0.0749	0.4227	0.4777	0.6175	0.7046	0.7046	0.8757	0.8788	0.8803
Obs.	27,406	27,406	27,406	27,406	26,289	20,702	20,702	20,702	20,693	20,693

Table 4. The Cost of Control: Lender Heterogeneity

This table presents fixed effects regression estimates of *TCB*, the total cost of borrowing measure as in Berg, Saunders, and Steffen (2016), on *Pr(Violation)*, the *ex ante* probability of covenant violation measure as in Demerjian and Owens (2016). *Terms* include an indicator that identifies loans secured by collateral, loan maturity, and loan amount. All other variables are defined in Appendix A. Heteroskedasticity-robust standard errors are clustered by lead bank, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)
Pr(Violation)	-39.70***	-44.89***	-46.10***	-50.14***	-40.85***
	(13.54)	(12.77)	(13.77)	(14.30)	(12.93)
Controls:					
Year	X	X	X	X	X
Type	X	X	X	X	X
Purpose	X	X	X	X	X
Terms	X	X	X	X	X
Borrower	X	X	X	X	X
Borrower × *Year*	X	X	X	X	X
Type × *Purpose*	X	X	X	X	X
Interacted Terms	X	X	X	X	X
Type × *Purpose* × *Year*	X	X	X	X	X
Interacted Terms × *Year*	X	X	X	X	X
Lender × *Year*		X	X	X	X
Lender × *Borrower*			X	X	X
Type × *Purpose* × *Lender* × *Year*				X	X
Interacted Terms × *Lender* × *Year*					X
Adjusted R^2	0.8803	0.7115	0.4651	0.3813	0.2874
Obs.	20,693	20,435	18,224	17,399	17,325

Table 5. The Cost of Control: Functional Form Robustness

This table presents fixed effects regression estimates of *TCB*, the total cost of borrowing measure as in Berg, Saunders, and Steffen (2016), on *Pr(Violation)*, the ex ante probability of covenant violation measure as in Demerjian and Owens (2016). Aside from the measurement of *TCB* and *Pr(Violation)*, each coefficient and standard error pair is estimated using the model shown in column (3) of Table 4. Heteroskedasticity-robust standard errors are clustered by lead bank, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

Dependent variable: *TCB*					
Condition:	*Main*	*Percentile*	*Decile*	*Natural Log*	*Winsorized 1%*
	(1)	(2)	(3)	(4)	(5)
Pr(Violation)	-46.10***	-6.84***	-0.69***	-0.21***	-45.19***
	(13.77)	(1.25)	(0.13)	(0.04)	(13.57)
Pr(Violation): *Percentile*	-0.41***	-0.072***	-0.0070***	-0.0021***	-0.41***
	(0.07)	(0.009)	(0.0010)	(0.0003)	(0.07)
Pr(Violation): *Tercile*	-21.51***	-3.62***	-0.36***	-0.11***	-21.18***
	(3.66)	(0.43)	(0.05)	(0.01)	(3.62)
Pr(Violation): *Decile*	-4.32***	-0.76***	-0.074***	-0.023***	-4.26***
	(0.74)	(0.10)	(0.010)	(0.003)	(0.74)

Table 6. The Cost of Control: Alternative Time Periods

This table presents fixed effects regression estimates of *TCB*, the total cost of borrowing measure as in Berg, Saunders, and Steffen (2016), on *Pr(Violation)*, the *ex ante* probability of covenant violation measure as in Demerjian and Owens (2016). Aside from the measurement of time periods, each coefficient and standard error pair is estimated using the model shown in column (3) of Table 4. Heteroskedasticity-robust standard errors are clustered by lead bank, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pr(Violation)	-46.10***	-45.41***	-75.96***	-47.95***	-37.86***	-27.71***	-22.72**
	(13.77)	(12.78)	(24.83)	(22.76)	(12.93)	(8.21)	(9.47)
Period (yrs):	Calendar	Fiscal	0.25	0.5	0.75	1.5	2
Adjusted R^2	0.4651	0.4654	0.3418	0.4130	0.4130	0.5005	0.5171
Obs.	18,224	18,230	16,960	17,467	17,467	19,363	20,031

Table 7. The Cost of Control: Simultaneity

This table presents fixed effects regression estimates of *TCB*, the total cost of borrowing measure as in Berg, Saunders, and Steffen (2016), on *Pr(Violation)*, the *ex ante* probability of covenant violation measure as in Demerjian and Owens (2016), interacted with measures of loan simultaneity. Aside from the interaction terms, each coefficient and standard error pair is estimated using the model shown in column (3) of Table 4. 1[*>2 loans*] is an indicator that equals one if the borrower issued more than two loans during the year and zero otherwise. $1[\textit{Multiple loans}]^{Month}$, $1[\textit{Multiple loans}]^{Quarter}$, and $1[\textit{Multiple loans}]^{6m}$ are indicators that equal one if the borrower issued multiple loans during any given month, quarter, or six month period during the year and zero otherwise. Heteroskedasticity-robust standard errors are clustered by lead bank, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)
Pr(Violation)	-46.10***	-51.26***	-49.70***	-50.73***	-53.19***
	(13.77)	(14.39)	(16.23)	(15.96)	(16.32)
Pr(Violation) × 1[*>2 loans*]		15.95			
		(19.74)			
Pr(Violation) × $1[\textit{Multiple loans}]^{Month}$			27.26		
			(19.47)		
Pr(Violation) × $1[\textit{Multiple loans}]^{Quarter}$				24.98	
				(18.25)	
Pr(Violation) × $1[\textit{Multiple loans}]^{6m}$					27.86
					(18.89)
Adjusted R^2	0.4651	0.4651	0.4671	0.4662	0.4659
Obs.	18,224	18,224	18,224	18,224	18,224

Table 8. The Cost of Control: Lender Competition

This table presents fixed effects regression estimates of *TCB*, the total cost of borrowing measure as in Berg, Saunders, and Steffen (2016), on *Pr(Violation)*, the *ex ante* probability of covenant violation measure as in Demerjian and Owens (2016), interacted with measures of lender competition. D is a measure of lender competition based on the number of lenders actively issuing loans to borrowers within the same location (i.e., State, City, or Zip Code), two-digit SIC industry, and size bracket (i.e., \$0-\$150M, \$150M-\$500M, or >\$500M). $1[D > D^{P50}]$ is an indicator variable that equals one if a given observation is above the sample median of D. Heteroskedasticity-robust standard errors are clustered by lead bank, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Pr(Violation)	-46.10***	-44.96***	-43.99***	-46.38***	-45.42**	-44.75**	-47.16**
	(13.77)	(16.23)	(13.60)	(17.64)	(22.99)	(21.44)	(20.76)
Pr(Violation) × D		-0.18		0.08		-0.43	
		(0.67)		(1.92)		(4.54)	
Pr(Violation) × $1[D > D^{P50}]$			-3.79		-1.25		2.23
			(15.06)		(25.37)		(26.14)
D:		State/SIC2/Size		City/SIC2/Size		Zip/SIC2/Size	
Adjusted R^2	0.4651	0.4650	0.4650	0.4650	0.4650	0.4650	0.4650
Obs.	18,224	18,224	18,224	18,224	18,224	18,224	18,224

Table 9. The Cost of Control: Term Tradeoffs

This table presents fixed effects regression estimates of *TCB*, the total cost of borrowing measure as in Berg, Saunders, and Steffen (2016), on *Pr(Violation)*, the *ex ante* probability of covenant violation measure as in Demerjian and Owens (2016), interacted with loan terms. *Secured*, *Amount*, and *Maturity* are loan terms as defined in Appendix A. 1[*Large Amount*] and 1[*Long Maturity*] are indicators that equal one if the loan has an amount or maturity that exceeds the sample median value and zero otherwise, respectively. Heteroskedasticity-robust standard errors are clustered by lead bank, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)
Pr(Violation)	-46.10***	-22.30**	-49.04***	-50.19***	-77.92***	-66.76***
	(13.77)	(11.15)	(14.55)	(17.32)	(19.55)	(14.77)
Pr(Violation) × *Secured*		-22.05				
		(20.64)				
Pr(Violation) × *Amount ($B)*			7.08			
			(4.53)			
Pr(Violation) × 1[*Large Amount*]				7.20		
				(10.26)		
Pr(Violation) × *Maturity (yrs)*					7.74***	
					(2.31)	
Pr(Violation) × 1[*Long Maturity*]						36.55***
						(8.66)
Adjusted R^2	0.4651	0.4755	0.4650	0.4651	0.4659	0.4697
Obs.	18,224	18,224	18,224	18,224	18,224	18,224

Table 10. The Cost of Control: Nonlinearities

This table presents fixed effects regression estimates of *TCB*, the total cost of borrowing measure as in Berg, Saunders, and Steffen (2016), on various transformation of *Pr(Violation)*, the *ex ante* probability of covenant violation measure as in Demerjian and Owens (2016). *Pr(Violation)*^{T2} and *Pr(Violation)*^{T3} are indicators that equal one if the loan has *Pr(Violation)* in the second or third terciles of the *Pr(Violation)* distribution, respectively. *Pr(Violation)*^{2} is the square of *Pr(Violation)*. Heteroskedasticity-robust standard errors are clustered by lead bank, and presented in parentheses. ***, **, and * denote results significant at the 1%, 5%, and 10% levels.

	(1)	(2)	(3)	(4)	(5)	(6)
Pr(Violation)	-46.10***	-81.18*				
	(13.77)	(44.11)				
Pr(Violation)^{2}		37.18				
		(55.91)				
Pr(Violation)^{T2}			-26.87***			
			(7.07)			
Pr(Violation)^{T3}			-41.24***			
			(12.91)			
Pr(Violation) < 1				-34.38**		
				(13.48)		
$0 <$ *Pr(Violation)*					-43.47**	
					(18.93)	
$0 <$ *Pr(Violation)* < 1						-42.46**
						(20.99)
Adjusted R^2	0.4651	0.4651	0.4651	0.4764	0.1727	0.1653
Obs.	18,224	18,224	18,224	17,142	9,141	8,123