The Supply-Side Determinants of Loan Contract Strictness

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Abstract

Using a novel measure of contract strictness based on the ex-ante probability of a covenant violation, I investigate how lender-specific shocks impact the strictness of the loan contract that a borrower receives. Exploiting between-bank variation in recent portfolio performance, I find evidence that banks write tighter contracts than their peers after suffering defaults to their own loan portfolios, even when defaulting borrowers are in different industries and geographic regions than the current borrower. The effects of recent defaults persist after controlling for bank capitalization, although negative bank equity shocks are also strongly associated with tighter contracts. The evidence is consistent with lenders learning about their own screening technology via defaults and adjusting contracts accordingly. Finally, contract tightening is most pronounced for borrowers who are dependent on a relatively small circle of lenders, with each incremental default implying covenant tightening equivalent to that of a ratings downgrade.

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1 Introduction

Just as credit volumes have swung wildly over the past five years, the terms of loan contracts issued have been equally fickle. Financial covenants requiring borrowers to maintain financial ratios within predetermined ranges were abandoned *en masse* during the easy credit period from 2002-2006. Since then, contracts have swung the other way, with financial trip wires set such that lenders receive contingent control rights for even modest borrower deterioration. Meanwhile, the effects of binding covenants on borrowers are substantial, ranging from limited access to otherwise committed credit facilities (Sufi 2009) to increased lender influence over the real and financial decisions of the firm ((Beneish and Press (1993), Chava and Roberts (2008), Nini, Smith, and Sufi (2009a, 2009b), Roberts and Sufi (2009a)).[1]

What drives variation in the strictness of the equilibrium loan contract? To date, the literature has primarily focused on the role of borrower characteristics in determining the degree of contingent control lenders receive. Smith and Warner's (1979) seminal discussion of covenants concludes that "there is a unique optimal set of financial contracts which maximize the value of the firm," attributing covenant choice to the particular features of a given project. The theory and evidence presented since strongly suggest that, on average, riskier firms receive contracts with stricter covenants (see Berlin and Mester (1991), Billett, King, and Mauer (2007), Rauh and Sufi (2009), and Demiroglu and James (2009), among others).

Instead, this paper examines the previously unexplored supply-side of the borrower/lender nexus. I ask, holding borrower risk fixed, how do lenders impact the strictness of the equilibrium contract and what factors influence changing lender preferences for contingent control? While there is a substantial collection of research documenting the ways in which various shocks to lenders influence credit availability (Bernanke and Gertler (1995), Peek and Rosengren (1997), Kang and Stulz (2000), Paravisini (2007), for example), to date no paper that I am aware of has considered the effects of supply-side factors on the state-contingent nature of credit that banks offer.

In particular, I focus on the recent default experience of the lender as a potential shock to its contracting tendencies.[2] This choice is motivated by a number of recent papers which strongly suggest that defaults to lender loan portfolios affect lending behavior at the defaulted-upon banks. Chava and Purnanandam (2009), for example, provide evidence that banks with exposure to the 1998 Russian sovereign default subsequently cut back lending to their borrowers. Berger and Udell (2004) link overall loan port-

[1]Firm investment, capital structure, cash management, merger activity, and even personnel have been linked to lender-borrower renegotiations following covenant violations.

[2]Defaults refer to payment defaults and not technical defaults on the contract such as covenant violations.

folio performance to the tightening of bank credit standards and lending volumes. Finally, Gopalan, Nanda, Yerramilli (2008) show that individual corporate defaults affect lead arranger activity in the syndicated loan market. Taken together, these papers suggest that variation in lender default experience may provide a plausible source of supply-side variation in lender contracting choice as well.

As the basis of my analysis, I develop a new measure of loan contract strictness which approximates the probability that the lender will receive contingent control via a covenant violation. Applying this new strictness measure to DealScan loan data, I find that banks tend to write tighter contracts than their peers after having suffered defaults to their own loan portfolios, holding constant borrower risk and controlling for time effects. The result is robust to a number of alternative specifications. In particular, by considering only defaults occurring in unrelated industries and/or in distinct geographic areas from the current borrower, I rule out the possibility that a default by one borrower informs undiversified lenders about the risk of other potential borrowers. The evidence would suggest, for example, that a default by a high tech firm in California impacts the contract offered to a mining company in West Virginia by way of their common lender.

These lender effects are economically large. For the average borrower, two incremental defaults to a lender's portfolio induce contract tightening equivalent to what a borrower could expect to receive following a downgrade in its own long-term debt rating. The data also confirms the predictions of prior work– that stricter loan contracts curtail the *de facto* amount of credit available to the borrower. An incremental default on the lender prior to contracting and the resulting covenant tightening are associated with a 0.6%-3.0% contraction in net debt issuance by the borrower in the three years after the covenants go into effect.

What drives lenders to tighten contracts? I explore two distinct hypotheses. The first hypothesis is that tightening is a result of depletion of bank capital mechanically associated with borrower defaults. If capital shocks influence a lender's contracts, but are also correlated with recent defaults, then any analysis which excludes capital may suffer from an omitted variable bias. In addition to investigating bank capital effects, I consider a second hypothesis– that banks use recent defaults to update beliefs regarding their own screening ability.

The theoretical predictions as to how a lender's contracts might be influenced by its capital position are mixed. On one hand, limited liability for bank shareholders may induce gambling when the bank is under-capitalized. As a result, banks may write looser contracts with larger losses in bad states of the world in exchange for higher interest rates in good state of the world. Alternatively, the large costs associated with recapitalization may cause thinly capitalized banks to hedge against insolvency, writing tighter contracts as insurance in the event of borrower distress.[3] Including bank capital controls in the benchmark specification will help shed light on the effect of capital on contracts, while simultaneously providing sharper inference on the effect of lender portfolio defaults.

The inclusion of controls for bank capital yields two noteworthy results. First, the effect of recent lender default experience on contract terms persists, even after controlling for the capital depletions associated with loan losses. Second, bank capitalization has an independent effect on contracts, providing a second channel through which contract terms are influenced by lender effects. Well-capitalized banks tend to write looser contracts, controlling for borrower risk, while negative shocks to bank equity are associated with stricter contracts. The direction of the effect is consistent with under-capitalized banks behaving more conservatively to protect their remaining capital.

The evidence that defaults induce lenders to tighten their loan contracts, independent of their capital position, suggests perhaps that contract strictness depends on information content in the defaults. Yet if the prior tests have adequately controlled for borrower characteristics and macroeconomic risk, then the information content in defaults must pertain to the lender itself. In the next tables, I explore one particular variant of this lender learning hypothesis– that banks find defaults to their own portfolios informative about their ability to screen risky borrowers. A large number of defaults, for example, may lead bank managers to update their beliefs regarding the effectiveness of credit scoring models, the abilities of their loan officers, or the adequacy of bank policies. Conditional on poor borrower screening, the bank may reasonably write stricter contracts to compensate for their uncertainty regarding borrower risk. Tighter covenants provide the lender with the option to restructure contracts or reduce credit availability as information about borrower risk is revealed, effectively substituting ex-post monitoring for weakened ex-ante screening.

If defaults inform the lender about its own screening ability, then defaults on the most recently originated loans will be the most informative. In contrast, the performance of loans originated in the distant past (or "legacy loans") will be made less meaningful by employee turnover and institutional changes to credit policy that occur over time. Consistent with these predictions, I find that banks are considerably more sensitive to defaults on recently originated loans than to defaults on legacy loans and that contract sensitivity to defaults is almost monotonically decreasing with the time since origination.

Of course, in the syndicated loan market, defaults may also inform participant banks about the lead arranger's screening ability (see Gopalan, Nanda, and Yerramilli (2008), for example). Because loan

[3]Zhang (2009), for example, shows that stricter covenants improve recovery rates in the event of borrower default.

participants rely upon the lead arranger to vouch for the borrower's creditworthiness, they may require tighter contracts from the lead arranger to compensate for reputational damage due to defaults. Drucker and Puri (2008), for example, show that lenders use tighter covenants as a substitute for reputation in the secondary loan market. Yet I find that covenants in bilateral loans are equally, if not more sensitive to the lender's recent default experience than are covenants in syndicated loans.

In the final section of the paper, I address the question of why borrowers accept stricter contracts and the resulting increased lender intervention when their own risk is unchanged. Going back to Smith and Warner's claim that "there is a unique optimal set of financial contracts which maximize the value of the firm", one would expect that in a frictionless bank market, unaffected lenders would step in to provide the borrower's "optimal" contract. As a result, contracts which deviate from this idealized contract will not be observed by the econometrician.

Bank-borrower relationships, however, are sticky. In practice, borrowers are often best served by a small, close-knit circle of relationship banks and not by a perfectly competitive mass of investors. Petersen and Rajan (1994, 1995) argue that smaller bank groups provide lenders the opportunity to collect rents from future business, thereby facilitating upfront borrower-specific investments required to resolve information asymmetries. Empirically, attempts to increase the breadth of lender relationships increase the price and reduce the availability of credit (Petersen and Rajan (1994, 1995), Cole (1998)).

Yet dependence on a smaller group of lenders is a double-edged sword. Evidence from Slovin, Sushka, and Polonchek's (1993) event study around Continental Illinois Banks' failure and subsequent rescue suggested that borrowers without other bank relationships or access to bond markets were more exposed to their lender's risk. Detragiache, Garella, and Guiso (2000) also argue that smaller bank groups subject the borrower to lender liquidity risk, resulting in early liquidation of some projects.

My final tables compare contract sensitivity to lender effects– both those related to recent default experience as well as bank capital effects– for borrowers with varying degrees of dependence on a small number of relationship lenders. Using the number of banks which have lent to a borrower over its last four loans as a proxy for the breadth of a borrower's outside options, the evidence strongly suggests that lender effects are competed away for borrowers with access to a broader base of lenders, while borrowers who are locked-in to a smaller circle of relationship banks are more likely to be subjected to contract tightening by affected lenders.

Similarly, public debt markets provide an alternative to bank financing for reputable borrowers. I compare contract sensitivity for firms with access to the commercial paper market to those without. I find that borrowers without access to cheap alternative sources of financing are more exposed to contract variation based on lender defaults and capital.

In sum, the evidence suggests that borrowers who rely upon a limited number of relationship banks and/or lack access to alternative sources of cheap capital are exposed to considerable lender-induced contract variation, precisely because of their limited outside options. The economic significance of this variation is substantial. For a locked-in borrower, a single default to its lead lender's portfolio induces contract tightening equivalent to that of a downgrade to its own long-term debt rating– twice the size of the effect observed in the full sample.

Of course, my analysis requires an empirical measure of contract strictness– and one which corresponds to a well-defined meaning of "strictness"– as well as the appropriate data and identification scheme. The next section discusses measurement issues and the data to be used before finally presenting the empirical analysis.

2 Methodology

2.1 Measurement

I begin by developing a loan-specific measure of contract strictness that captures the ex-ante probability of a forced renegotiation between lender and borrower. In practice, covenant violations allow for lender-driven renegotiation by providing the lender with a state contingent call option on the loan if, for example, cash-flows fall below some agreed upon level. In this event, the lender can demand immediate repayment, or require amendment fees, collateral, or a shorter maturity. As a result, I will view "stricter" contracts as those which provide the lender contingent control in more states of the world by making trip wires more sensitive. A number of earlier papers provide varied measures of covenant strictness that reflect this sentiment (Bradley and Roberts (2004), Puri and Drucker (2008), Billett, King, and Mauer (2007), Dyreng (2009), and Demiroglu and James (2009) provide a handful of examples). I attempt to develop a measure that nests the best qualities out of each of these.

Four desirable properties of any strictness measure jump out immediately– properties which have motivated prior measures of covenant strictness in the literature. First, all else equal, a contract with more covenants– that is, covenants binding more of the borrower's financial ratios– will give the lender more contingent control and therefore, should be treated as stricter. For example, a contract with a single cash flow covenant is less strict than a contract with both cash and leverage covenants. In response, one could count the number of covenants included in a contract. Bradley and Roberts' (2004) covenant intensity index, for example, captures this idea, although they also consider non-financial covenants.

Yet, by itself, a count index will fail to capture a second dimension of strictness: the initial slack allowed for each of the specified covenants. Holding the number of covenants fixed, covenants which are set closer to the borrower's current levels will be triggered more often, giving the lender an option to renegotiate in more states of the world. To date, however, slack has only been measurable one covenant at a time and therefore does not capture strictness accurately in transactions that use complementary covenants together. Looking only at transactions with a single covenant also severely limits sample size and forces the empiricist to use a non-random subset of borrowers. Demerjian (2007) points out that borrower characteristics dictate which ratios are governed by covenants. For example, borrowers with losses are more likely to use net worth covenants. As a result, one can imagine that any measure based *only* on the slack of a net worth covenant, for example, might provide inference which is only valid for a subset of borrowers.

Third, scale matters. Setting slack equal to one implies a very strict cash flow covenant (a one dollar reduction in cash-flows will trigger default), but a current ratio covenant devoid of meaning (the ratio of current assets to total assets can vary between .01 and 1 without event). As a result, it becomes necessary to scale contractual slack differently for different covenant ratios.

Finally, the covariance of ratios is important. Since renegotiation is triggered if even a single covenant is tripped, contracting on independent ratios increases the probability of a violation (again, holding all else equal). A contract with a total net worth covenant, for example, is unlikely to be made markedly stricter by the addition of a tangible net worth covenant.

Having determined that this measure should reflect the number, slackness, scale and covariance of covenants, consider a single financial ratio r which receives a shock in the period after the loan is granted,

$$r' = r + \epsilon \sim N(0, \sigma^2). \tag{1}$$

If a covenant for r is written such that $r' < \underline{\mathrm{r}}$ allocates control to the lender, then

$$p \equiv 1 - \Phi\left(\frac{r - \underline{\mathrm{r}}}{\sigma}\right) \tag{2}$$

represents that ex-ante probability of lender control, where Φ is the standard normal cumulative distribution function. This measure incorporates both covenant slackness and scale by normalizing ratios by their respective variances. To capture the number of covenants and their covariance, I generalize the prior two equations to a multivariate setting.

For contracts with more than one financial covenant, consider an $N \times 1$ vector of financial ratios $\mathbf{r}$ which receives an N dimensional shock, migrating to $\mathbf{r}'$,

$$\mathbf{r}' = \mathbf{r} + \epsilon \sim N_N(\mathbf{0}, \Sigma). \tag{3}$$

If the covenant for the n^{th} element of $\mathbf{r}$ is written such that $r'_n < \underline{\mathrm{r}}_n$ allocates control to the lender, then

$$STRICTNESS \equiv p = 1 - F_N\left(\mathbf{r} - \underline{\mathrm{r}}\right) \tag{4}$$

where F_N is the multivariate normal CDF with mean $\mathbf{0}$ and variance Σ.[4]

The resulting measure of contract strictness is increasing in the number of covenants included in a given contract and also accounts for the fact that combinations of independent covenants are more powerful than covenants written on highly correlated ratios. The multivariate generalization also continues to capture both slack and scale, satisfying the four measure benchmarks laid out above, while providing a natural economic interpretation as a stylized probability of lender control based on covenant violation. Finally, the measure of strictness is easily estimable using loan covenants reported in DealScan and the borrowers' actual financial ratios at the time of issuance from Compustat. In practice, I estimate Σ as the covariance matrix associated with quarterly changes in the financial ratios of levered Compustat firms. Σ may also be allowed to vary by SIC industry.

2.2 Data

I apply my proposed strictness measure to loans reported in Loan Pricing Corporation's (LPC) DealScan loan database. DealScan reports loan details from syndicated and bilateral loans collected by staff reporters from lead arrangers and SEC filings from 1984-2008. Included in the loan details are covenant levels for individual contracts. Covenant levels are then merged with accounting data available from Compustat using a link file graciously provided by Michael Roberts and Sudheer Chava (as used in Chava and Roberts (2008)).

With both contract and borrower data in place, estimating strictness is straightforward. Slack is measured in the first period of the contract as the difference between the observed ratio and the minimum

[4]To see this, note that the probability of no default occurring over all n covenants is equivalent to all ϵ's being within the allowable slack, $r_n - \underline{\mathrm{r}}_n$. Since this probability is equal to the CDF evaluated at $\mathbf{r} - \underline{\mathrm{r}}$, the probability of one or more defaults occurring will equal the complement of the CDF evaluated at $\mathbf{r} - \underline{\mathrm{r}}$.

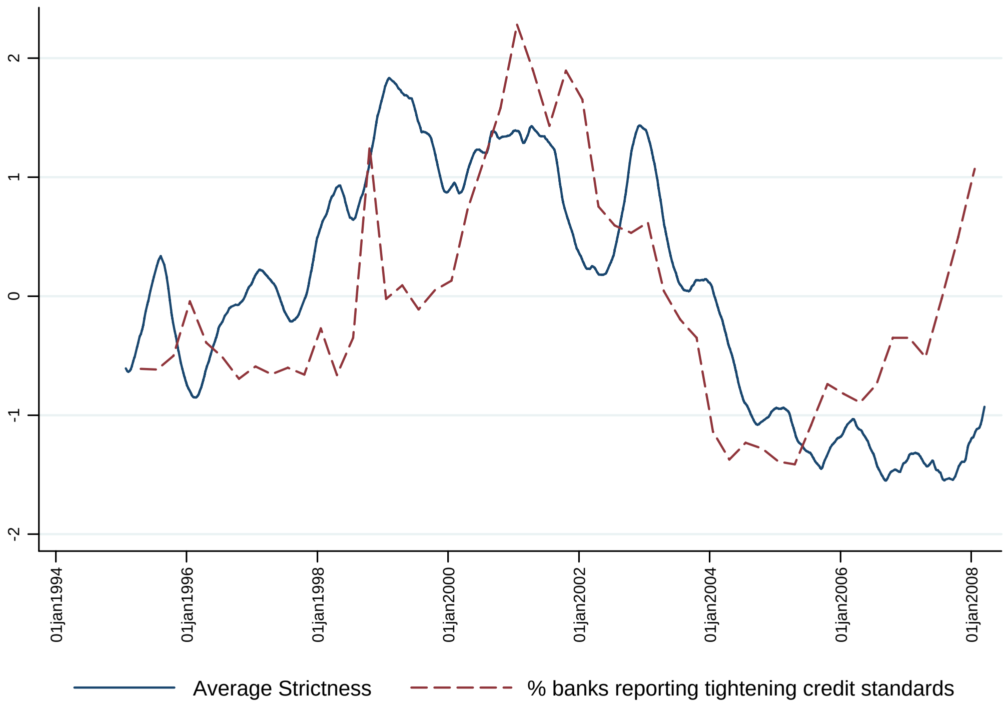

Figure 1: Average contract strictness over time, plotted against the Federal Reserve survey of senior loan officers, percentage of respondents reporting tightening credit standards. The moving average is calculated using a tent shaped kernel over 180 day bandwidth, such that $\overline{STRICTNESS}_t \equiv \sum_{|T-t|\leq 180}\left[w_T\left(\sum_{i\in T}\widehat{STRICTNESS_i}\right)\right]$, where $w_T = min\left[\frac{1-\frac{|T-t|}{181}}{\sum_{i\in T}\left(1-\frac{|T-t|}{181}\right)},0\right]$. Both plots are standardized.

allowable ratio (or the negative of the difference in the case of a maximum ratio), both taken in natural logs for the following reported covenants: minimum EBITDA/debt, current ratio, quick ratio, tangible net worth, total net worth, EBITDA, fixed charge coverage, and interest coverage, and maximum debt/equity, debt/tangible net worth, and capital expenditure. These covenants capture the vast majority of the database.[5]

I eliminate contracts which appear to be in violation within the first quarter. This leaves 2,613 loan contracts. Note that transactions are reported at the package and facility level in DealScan, where packages are collections of facilities (loans or lines of credit) with linked documentation. Since covenants are only reported at the package level, this is the relevant unit of observation for a contract. Given the lack of independence between identical facility level observations for loans with multiple tranches, significance would be dramatically (albeit spuriously) increased by using facility level observations rather than package level observations. Of the remaining contracts, 20.60% have multiple lead arrangers, each

[5]For covenants which include measures of cash-flow or income, these are calculated on a rolling four-quarter basis. Other calculation details will be made available in an internet appendix for the interested reader.

of which are matched to the contract. After matching loan packages to the relevant lead arrangers, I have 3,172 borrower-lender contracts available for analysis.

In order to generate my measure of contractual strictness defined in the prior section, I first estimate the variance-covariance matrix associated with the quarterly changes in financial ratios being contracted upon using Compustat data for the Compustat/DealScan merged firms identified in Chava and Roberts (2008). Given that the distribution of shocks may not be identical for all firms, the variance-covariance matrix is allowed to vary for different one-digit SIC industries. Although the results presented hereafter allow for this variation, they are substantially the same as results estimated using a single variance-covariance estimate.

Given that slack for each covenant is measured with error, my final measure of strictness will also be subjected to measurement error. Measurement error is a product of imperfect observation at two levels. First, specific covenant language varies on a contract-by-contract basis, so that a financial ratio referenced in one contract may require a marginally different calculation than that of another. Second, even with perfect knowledge of the calculation used in a given contract, variations may reference non-GAAP accounting data presented and certified by the CFO but not available within Compustat or publicly at all.

Fortunately, measurement error will not induce attenuation bias in the estimates presented, as long as contract strictness is treated as a dependent variable. Instead, measurement error will be absorbed into the model's error term and, at worst, the measure will simply fail to find traction in the data. Moreover, any measurement error is likely be largely driven by borrower-specific components, which will be captured in borrower fixed-effects estimation used in the analysis.

With strictness calculated for each contract, Figure 1 presents a moving average time-series plot of contract strictness and demonstrates the measure's intuitive time-series properties.[6] Average contract strictness peaks in the sample near the 1998 Russian financial crisis and subsequent collapse of Long-Term Capital Management, and drops off considerably between 2003 and 2007 during covenant-lite lending. Strictness is also plotted against a well-worn measure of supply side strictness: the Federal Reserve survey of senior loan officers reporting tightening credit standards. The two measures are closely related, with a correlation coefficient of 0.6. The correlation suggests the measure is informative of lender attitude, and gives hope that supply-side issues will be important in predicting contract variation.

Meanwhile, if contract strictness proxies for the probability of contingent lender control, then it should predict actual contract violations. I find strong evidence that this is the case. Using a list of covenant

[6]The moving average is calculated using a tent-shaped kernel with 180 day bandwidth.

violations provided by Nini, Smith, and Sufi (2009b),[7] I estimate probit regressions of whether or not a violation occurred during the tenor of the loan on the proposed measure of contract strictness, the borrower's Altman Z-score and squared Z-score as well as dummy variables for the borrower's S&P long-term debt rating. I also include controls for loan characteristics, including the loan's maturity in months, amount, the presence of collateral, and number of participants. Following Nini, Smith, and Sufi's suggestion, I only consider new violations, excluding violations where the borrower had a prior violation in any of the subsequent four quarters.[8] The results, left to the Appendix (Table A1), confirm the new measure has a strong association with the probability of a violation. For the sake of comparison, I repeat the analysis with two alternative measures– the number of financial covenants and, for loans with a net worth or tangible net worth covenant, the slack of that covenant at the time of issuance, scaled by total assets. Neither measure does well in comparison. The number of financial covenants is not significant in any of the specifications. Meanwhile, slack of the net worth covenant has the correct sign and is significant by itself, although it forces the analysis on a drastically reduced sample. It is no longer significant, however, when it has to compete with the proposed broader measure of strictness.

2.2.1 Other data

To test the effect of lender variation in recent default experience on contract strictness, I count the number of loan defaults suffered by the lead lender during the 360 days leading up the date a given contract was negotiated (see below for further discussion on how I arrive at this date). Because I am interested in economically significant defaults which might plausibly impact the behavior of a corporate loan officer, I use borrower defaults reported by Standard & Poor's (S&P) in Compustat's ratings database. S&P reports defaults when it believes a borrowers will fail to pay all or substantially all of its obligations as they come due. This count may miss defaults by small, unrated borrowers, but will capture visible defaults likely to sway loan officer behavior.

Using the merge file from Chava and Roberts (2008), the defaulting borrowers are matched back to DealScan, which provides the list of loans for each defaulting borrower, as well as the participant banks in each of those loans. After removing loans which were not outstanding at the time of default based on their reported origination and maturity dates, I am left with a record of all the defaults for a given lender and the approximate timing of those defaults (S&P reports monthly). For each new loan contract, I then construct the default count for the lead lenders in that loan leading up to its issuance. Finally,

[7] Please refer to the data appendix in their paper for details.

[8] Note, for borrowers with multiple contracts outstanding, I do not observe which contract caused the violation– only that a violation occurred.

I time-demean default counts by lender; after all, large lenders may have a large absolute number of defaults even in the best of times. The main results of the paper hold if defaults are instead recorded as the natural logarithm of 1+total lender defaults.

In terms of defining lenders, DealScan typically reports as lead arranger the name of the institution listed on the cover page of a loan document. Often, this results in regional branches or offices being listed as the lender of record. Because these distinctions are often legal rather than operational, I aggregate these entities into their parent institution, using the Federal Financial Institutions Examination Council's National Information Center and annual reports as a source of ownership information when necessary. Similarly, investment banking and mortgage financing operations are grouped under their parents. In dealing with bank mergers, I create a new institution if two banks merge and it causes both to change their name (e.g. JPMorgan and Chase become JPMorgan Chase).

Finally, it is necessary to make mild assumptions about the timing of contracts. DealScan reports the effective date of a package as the day which the loan contract is dated. However, the terms of a loan are negotiated well in advance of this date. Practitioner estimates suggest that the average syndicated transaction takes 8 weeks between the date the borrower awards the lead bank a mandate (a contract to act as the lead arranger) and the date the loan is effective (Rhodes (2000)). For subsample of DealScan loans reporting both mandate and closing dates, the timing is broadly consistent, with a mean (median) time in market of 89 (63) days. Meanwhile, several additional weeks may transpire between the time a bank submits its first bid to a borrower and receives a signed mandate. It is during this pre-mandate phase when banks commit to term sheets proposing their required covenant levels.

To account for this time lag, I report the contracting date of a loan as 90 days prior to the DealScan reported start date. Regressions of contract strictness against leads and lags of macroeconomic indicators seem to confirm the appropriateness of this assumption. Contracts which closed in December, for example, respond to aggregate defaults, stock market returns, and credit spreads in September, suggesting a 90 day lag between contracting and closing.

Because a lender's loan losses may impact its behavior by way of its balance-sheet, the analysis also requires financial information from the lender. I have hand-matched DealScan lender names to 205 banks and non-bank financial institutions in Compustat's various quarterly databases (Banks, North America, and Global). Matching is done using bank names only. In the event lenders are wholly-owned subsidiaries of banks and bank holding companies, the ultimate parent is considered the lender. When possible, ownership structure is discerned via the Federal Financial Institutions Examination Council's

National Information Center.

Table 1 presents summary statistics for the final sample of loans for which we have both a Compustat-DealScan match and for which covenant information is available. I compare this to the full DealScan-Compustat merged sample. Borrowing firms were typically large, with mean total assets of $2.86 billion and median total assets of $686.18 million in the first quarter after the loan closed. This is roughly consistent with the size of borrowers not reporting covenants in the DealScan-Compustat merge, with mean total assets of $3.51 billion and median total assets of $599.86 million, although the sample of borrowers without covenants is more positively skewed. Nearly half of the loans are to borrowers with long-term debt ratings from Standard & Poor's, with a median rating of BBB–, just at the threshold between junk and investment grade. Loans have a mean (median) maturity of 46.74 (48) months, a mean (median) size of $339.72 million ($150 million), attract an average (median) of 8.11 (6) participant banks, and most importantly, have a mean (median) strictness of 21.15% (16.60%). Finally, I also report the characteristics of lead lenders for the sample loans. Lenders have average (median) total assets of $647.95 billion ($429.96 billion), mean and median capitalization of 8% and experience an average (median) of 1.48 (0) defaults in the 90 days leading up to a loan contracting date.

3 Contract strictness and recent default experience

Having developed a measure of contract strictness based on the probability of contingent lender control due to covenant violation, I now wish to exploit variation in recent default experience as a potential shock to the contracting lender. Recent default experience has been linked to lender behavior in a number of recent papers (Berger and Udell (2004), Gopalan, Nanda, Yerramilli (2008)). While these papers focus primarily on the propensity to make future loans, the subsequent analysis will ask if, conditional on a loan being made, the terms of that loan are affected by recent lender defaults.

My first test of the effects of lender defaults on contract strictness fall to the specification below:

$$STRICTNESS_{i,t} = \alpha_i + \gamma_t + \beta X_{i,t} + \lambda DEFAULTS_{i,t} + \epsilon_{i,t} \tag{5}$$

where i indexes borrowers. The central issue in identifying recent default experience as a pure lender effect will be to ensure that the recent default experience is not correlated with any any unexplained borrower risk remaining in $\epsilon_{i,t}$. Consequently, the controls in $X_{i,t}$ attempt to capture observable proxies for borrower risk. In particular, I allow separate intercepts for each S&P long-term credit rating, with the omitted dummy variable capturing unrated firms. I also include the Altman Z-score of the borrower at the time of issuance and the square of the borrower's Z-score as an additional control to capture repayment risk for unrated firms and to allow for potentially lagged responses to distress by rating agencies.[9]

Yet borrower risk characteristics may be unobservable to the econometrician, in which case tests for the effects of lender defaults on contract strictness may be biased by selection effects. Issues with selection typically arise in corporate finance settings when the explanatory variables are chosen by the firm, and the factors driving that choice also explain variation in the outcome. Selection in this model is slightly more subtle and depends on borrowers and lenders matching based on unobservable borrower characteristics which are correlated with defaults.

To illustrate the point, consider two borrowers with different characteristics who issue each period. At the same time, their potential lenders experience varying degrees of defaults. If the borrowers and lenders are randomly assigned to one another, then pooled OLS is unbiased and efficient. If, however, the borrowers are matched to their lenders based on characteristics unobservable to the econometrician, then estimates of λ will be potentially biased, with the direction of the bias dependent on the how characteristics are correlated with lender defaults. If, for example, lenders select safer firms after suffering defaults, then estimates of λ will be negatively biased, reflecting the reduced contract strictness attributable to the safer borrower pool. Alternatively, if banks gamble for resurrection by seeking out risky borrowers after defaults, estimates of λ will be positively biased, as tighter contracts are required for the riskier borrowers.

In order to alleviate this selection on unobservables problem, borrower fixed effects are critical to the analysis. By time-demeaning all variables by borrower, the effects of unobservable borrower characteristics which are fixed over time are removed from the error term, $\epsilon_{i,t}$, thereby mitigating bias due to selection.

The specification laid out in equation (5) also includes controls for other loan characteristics, such as whether or not the transaction is secured, the log of deal maturity (in months), the log of deal amount, and the log of the number of bank participants. In each case, for transactions with multiple tranches, these are calculated as the maximum of all tranches within a package. Regrettably, these controls are likely to be jointly chosen together with contract strictness. In the absence of credible instruments, I have estimated all results with and without loan controls in unreported tables. Estimates are qualitatively and quantitatively similar, ensuring that potential endogeneity of these controls is not driving my key

[9] I calculate Altman's Z-score as 1.2*Working Capital/Assets+1.4*Retained Earnings/Assets+3.3*EBITDA/Assets+.6*Market Value of Equity/Liabilities+.999*Sales/Assets. The benefit of using Z-scores as opposed to individual components of the Z-score or modifications thereof is the reduction in the dimension of the problem (see Altman, 1968). Replacing Z-score with a less parsimonious vector of controls for borrower risk including leverage, EBITDA/Assets, market to book ratio, log(Assets), and current ratio does not change any results.

results.

Clean identification also requires that lender defaults do not proxy for unobservable macroeconomic risk. In particular, time-series variation in contract strictness appears to have important business-cycle components which affect all banks and borrowers simultaneously. Year dummies ensure that the effects of recent defaults are not an artifact of the business-cycle risk, but that rather, within a given period, contract strictness sorts according to relative lender loan performance. Aggregate measures of macroeconomic risk, including economy-wide defaults, or alternatively, more granular time dummies, may substitutes for year dummies. I discuss this further below. In each case, the assumption that allows for identification is that, while total defaults may be correlated with aggregate risk, the distribution of defaults across lenders should not be. I address the possibility that regional or industry-specific risk might weaken this assumption later in Table 3.

Panel A of Table 1 begins by estimating the fixed-effect regression of loan strictness on recent defaults and appropriate controls, as described above. Column (I) counts defaults (described in the Methodology section) for the lead arranger in the 360 days leading up to a given loan's contracting date and subtracts off the lender's average yearly defaults in the sample to remove possible lender size effects. Columns (II)-(V) break down the defaults for the periods 0-90 days prior to contracting, 90-180 days prior to contracting, 180-270 days prior to contracting, and 270-360 days prior to contracting, in each case, time-demeaning counts by lender.

The results suggest a significant contracting tightening by banks in response to recent defaults. The effects of defaults over the 360 days prior to contracting suggest a 0.19 increase in strictness for a given borrower for each incremental annual default to the lead lender. This response is significant at the 1% level. Meanwhile, Columns (II)-(V) are consistent with a short-lived effect. The experience in the past 90 days is significant at the 1% level, whereas the effect steps down for less recent defaults. Meanwhile, the borrower's Z-score and ratings dummies are also significant, with riskier firms receiving stricter contracts as predicted. Loan controls are not significant and can be removed from the regression without meaningfully affecting coefficients on the variables of interest.

Returning to potential selection problems, recall my claim that fixed effects would mitigate selection effects by removing unobservable borrower characteristics which are fixed over time. Li and Prabhala (2005), however, point out that fixed effects may not resolve selection problems if the offending unobservables migrate over time. In particular, we may observe a spurious positive relation between contract strictness and defaults if defaulted-upon banks tend to lend to borrowers which have become *unobservably* riskier over time.

If this were the case, and assuming that unobservable risk is positively related to observable proxies for borrower risk, we would expect to see lenders selecting more junk-rated borrowers and borrowers with lower (worse) Z-scores after periods of default. In contrast, there is weak evidence in the sample that, if anything, lenders migrate to observably safer borrowers after default, suggesting that any selection bias will be towards zero. Lender-demeaned defaults have a correlation of 0.04 with their borrower's Altman's Z-scores (which increase as borrower risk is reduced). While the correlation is admittedly small, it is significantly different from zero at the 2% level. Meanwhile, defaults have a -0.04 correlation with Borrower ratings for rated firms, where ratings are assigned numerical values from 2 (AAA) to 27 (default) as in Compustat's rating database, although the correlation is not significant (p-value of 0.13). The results are broadly consistent substituting regressions of Z-score/ratings on defaults and year dummies for univariate correlations. This seems to suggest that selection issues should be small and, if anything, will work against finding significant lender effects.

Given that Columns (II)-(V) of Panel A suggested that banks are most sensitive to defaults occurring in the 90 days immediately prior to contracting, going forward I focus on this 90 day period when looking at recent lender experience. One could still be concerned that the annual time dummies are not fine enough to capture high frequency changes in macroeconomic risk. Panel B responds to these concerns by replacing time dummies with the sum of total defaults in the economy over the matching 90 day period, so that controls for aggregate risk are at the same frequency as lender-specific defaults. If, in fact, the lender's defaults are capturing unobservable macroeconomic risk, then aggregate defaults over the same period will provide a better proxy for that risk and drive out the effects of a given lender's idiosyncratic experience. Instead, Columns (I) and (II) of Panel B report the lender's own default experience continues to drive contracting, controlling for the aggregate defaults over the same time period. Meanwhile, the significance of coefficients on aggregate defaults seems to suggest that lenders do respond to the recent defaults of other banks in their contracts, but place special weight on defaults to their own loans. The addition of alternative macroeconomic controls such as the return on the S&P 500 market index over the same 90 day period as reported on CRSP, credit spreads (returns on Moody's Baa-Aaa rated bonds), and quarterly GDP growth neither affect the coefficient on the lender's own defaults, nor its response to defaults on other banks.[10]

[10] Alternatively, time dummies may be set at the monthly or quarterly level. In unreported results, the coefficient on 90 day defaults remains significant at the 5% level using quarterly dummies and at the 10% level using monthly dummies. The models, however, risk being over-specified in smaller subsamples, where a given month or quarter may have only a handful of loans, especially given the presence of borrower fixed effects and ratings dummies.

Finally, before moving on to an economic interpretation of results, I briefly address the issue of truncation in the sample and how it may effect my estimates. Truncation occurs when sample selection is at least partly based on the dependent variable– for example, if we only observe loans above a certain level of strictness. Regrettably, covenant data is only available for loans which report the existence of covenants. As a result, loans with a strictness measure of zero may be endogenously excluded from the sample. Truncated regression techniques correct for potential sample selection biases that may result (see Wooldridge 2002, or Davidson and Mackinnon 1993, for a more complete discussion). In order to ensure that key results are not driven by sample selection issues related to variable truncation, I re-estimate the specification in Column (V) using a truncated regression framework. Because truncated regression are not well behaved with borrower fixed effects, however, I treat the panel as pooled. In unreported tables, I find that the effects of defaults 90 days prior to contracting on covenant strictness remain significant at the 5% level.[11]

3.1 Do lender defaults proxy for industry or region-specific risk?

A valid concern with the estimates provided in Table 2 is that lender defaults may proxy for geographic or industry-specific risk. If, for example, lenders specialize in a particular region, then their own defaults will be relatively more informative than the defaults of banks lending broadly or specializing in unrelated regions. In such a case, neither time-dummies, nor aggregate default counts would capture the borrower risk that a given lender is facing. A similar story could be told for lenders which specialize in a particular industry.

It happens to be the case that the 205 lenders identified in the sample are large and diversified enough to limit the likelihood of this scenario. Nevertheless, to sharpen identification, Table 3 removes defaults which are related to the current borrower by way of home state (or country for non-US borrowers), one-digit SIC code, or both. The regression now tests whether a default by high-tech firm in California, for example, can affect the contract written for a mining company in West Virginia by way of their common lender, controlling for economy-wide risk via time dummies. If a given lender's defaults are related to contract strictness solely because regional or industry-specific concentrations make that lender's defaults more informative of borrower risk than defaults to rival lenders, then removing defaults which face similar risk factors to the current borrower will eliminate this effect.

[11] Instead of treating the analysis as a truncated regression, it is tempting to include all loans for which no covenant data was reported, setting strictness equal to zero for these loans, and estimating a Tobit regression which allows for "bottom-coding" of the strictness variable (Wooldridge, 2002). This implicitly assumes, however, that the lack of data on covenants implies none were written for a given loan. In contrast, a model of truncation requires a much milder assumption– that borrowers who had no covenants do not report them.

In Table 3, Columns (I), (II), and (III) project contract strictness on lender defaults in one-digit SIC codes and states (or countries for non-US borrowers) which are distinct from those of the contracting borrower. As before, default counts are time-demeaned by lender. In each case, I find the estimated coefficient on recent defaults is significant at the 1% level. Coefficients are also of comparable magnitude to the estimates in Table 2 (even a bit larger), reinforcing the theme that lender defaults are not a function of borrower risk, but a distinct lender effect.

3.2 Interpreting economic significance

How large are the effects of recent defaults on the contract the borrower receives? If interpret the derived strictness measure as a true probability of contingent lender control within the quarter, then at the median, the marginal default increases the probability of lender control over the course of a year by approximately 1.5%. Alternatively, it may be more useful to understand the magnitude of lender effects in terms of changing borrower risk. For example, we might ask, how many lender defaults are required to move contracts by the equivalent of a borrower ratings downgrade? Regressing changes in a rated borrowers' contract strictness from loan-to-loan on changes to its long-term credit rating, we find a regression coefficient of 1.25, significant at the 1% level. Comparing this magnitude to that of recent lender defaults, we can roughly estimate that the effect of a ratings downgrade on a borrower's contract is approximately equal to that of two additional defaults to the lender's loan book (less than a one standard deviation change). Meanwhile, if we look at the market-wide loosening of contracts from 2000 and 2004, the median lender experienced six more defaults in its worst quarter than in its best quarter.[12] Therefore, for a representative lender and borrower during this period, default variation would induce contract variation comparable to the effect of three borrower ratings changes.

What is the effect of this contract tightening on borrowers? Several recent papers would suggest a strong negative relationship between covenants and access to debt markets going forward. Roberts and Sufi (2009a) use regression discontinuity to show that covenant violations result in sharp and persistent declines in net debt issuance. They observe a reduction in debt issuance that lasts for two years and is sufficiently large to move firms from the 75th to the 45th percentile of the within-firm leverage distribution. Nini, Smith, and Sufi (2009b) perform a similar analysis and find a new covenant violation leads to an 8-15% reduction in debt issuance in the year following a violation. Given the way in which I have defined strictness– as the probability a lender will receive contingent control *based on a covenant violation*– we

[12] In defining the median lender, I focus on lenders which were active in five or more quarters during that time period, leaving 80% of the sample.

should expect stricter contracts to result in a similar reduction in debt issuance by borrowers.

To interpret the economic significance that bank defaults have on borrower's access to debt by way of contract tightening, I provide coarse estimates of the size of these effects in my sample. Rather than focus on covenant violations, I examine the relationship between ex-ante covenant strictness and net debt issuance by the borrower in the period after covenants become effective. This disallows Roberts and Sufi's regression discontinuity design approach to identification, but captures firms curtailing debt issuance in order to avoid violations, as well as firms' response to violations. Given that Roberts and Sufi find deleveraging in the two years following a violation, I examine debt issuance of firms in the three years after a contract is issued (the left hand side variables is $ln(Debt)_{t+12} - ln(Debt)_t$, where t is indexed as the calendar quarter in which a loan is issued). Regressing debt issuance on contract strictness as well as firm and loan controls in Table 4, I find that a unit increase in strictness translates into roughly a 1% decrease in debt issuance in the three years after the covenant becomes effective.

Of course, firms may accept stricter covenants if they anticipate limited debt issuance in the future. To address this endogeneity, I exploit the earlier results– that contract strictness depends on the default experience of the lender in the 90 days leading up to contracting, controlling for borrower characteristics. This default count serves as an instrument in the two-stage least squares regression presented in Column (II), where the exclusion restriction is based on the fact that idiosyncratic lender defaults are uncorrelated with the borrower's cost of complying with stricter covenants. To mitigate concerns that defaulted-upon lenders may restrict debt through means other than financial covenants (for example, they may be less likely to participate in future issuances by the borrower), I exploit the timing between contracting and facility active date and control for the number of defaults in the 90 days *after* contracting but *before* the facility effective date.[13] As long as lenders' response to defaults is short lived (as is the case for covenants), the instrument will be valid. Meanwhile, we can infer that it satisfies rank conditions based on the results in Tables 2 and 3.

The results of two-stage least squares in Column (II) of Table 4 confirm what the prior literature and my own OLS estimates would suggest. Debt issuance drops by as much as 5% for a unit increase in contact strictness. Note, the increase in magnitude over OLS estimates is perhaps not surprising, given that contract strictness is measured with error and, as a result, the estimated OLS coefficient will be attenuated towards zero. In addition to sidestepping the potential endogeneity issue described above, two-stage least squares corrects for attenuation bias, assuming the measurement error associated with

[13] Recall, that based on the average time that transactions spend in mandate and syndication stage, I mark the contracting date when covenants are set as 90 days prior to the facility effective date. See the section on Other Data for a discussion of this timing.

strictness is independent of the error around recent defaults. Using OLS and 2SLS estimates as a range for the effect of strictness on debt issuance, and given that an incremental default induces a tightening of approximately 0.6, I approximate that an incremental default reduces borrower debt issuance by 0.6%-3.0%.

What drives lenders to tighten their contracts in reaction to recent defaults? In the following two sections, I examine two potential hypotheses regarding the economic mechanism through which recent default experience manifests itself as a lender effect in contracting. I begin by addressing the possibility that tightening is a result of bank capital depletion mechanically associated with borrower defaults. I find that the effect of recent lender default experience on contract terms persists, even after controlling for the capital depletions associated with loan losses, although bank capital has its own, distinct effect on contracting. Second, I consider the hypothesis that banks use recent default experience to update beliefs regarding their own screening ability and find evidence which is broadly consistent with this story.

3.3 Distinguishing capital effects from other effects

What is the mechanism through which recent default experience influences contracting behavior? One obvious possibility involves the capital shocks associated with loan losses, which over time, are written down from the bank's equity. If bank capital drives contracting choices, earlier tables may suffer from omitted variable bias.

The effect of capitalization on bank behavior has been thoroughly studied as it pertains to the credit channel literature, although to my knowledge, this has not included any discussion of contract strictness. Loan losses and other shocks to capital are known to curtail lending as banks anticipate binding regulatory capital constraints in some future states of the world (Van den Heuvel (2001), Dewatripont and Tirole (1994)). These effects may dictate not only lending volume, but also the risk profile of the loans extended. As Gambacorta and Mistrulli (2004) point out, however, the expected relation between capital shocks and new loan quality is not an uncontroversial prediction. One line of argument suggests that large costs associated with recapitalization will induce marginal banks to insure against losses by favoring safe assets in order to protect solvency. Alternatively, lower franchise values of thinly capitalized banks, together with limited liability, may induce gambling. Potential gaming of deposit insurance and regulatory capital schemes further confound these predictions (see Flannery (1989), Gennotte and Pyle (1991), Hellman et al. (2000), Kim and Santomero (1988), and Rochet (1992)). While lender risk preference in this context tends to focus on the tightness of credit standards upheld by banks (their willingness to lend to risky borrowers), conditional on borrower approval, the terms of the loan contract may also depend on bank capital. Ultimately, this is an empirical question.

In order to distinguish between the capital effects of recent defaults and other effects, Table 5 controls for bank capitalization and shocks to bank capitalization. From Compustat, I calculate the capitalization of the lead bank as Shareholder Equity/(Total Assets-Cash) or (Compustat seqq/(atq-cdbtq) for lenders in the Banks database, and seqq/(atq-cheq) for those in the Fundamentals or Global databases) as of the quarter the facility became active in DealScan. This timing allows for a lag following the contracting date (defined in the Methodology section as 90 days prior to the closing date in order to capture the average time between mandate and closing) such that defaults have adequate time to flow through the balance-sheet. Regressing leads and lags of bank capitalization on lenders' defaults confirms the appropriateness of the lag. Bank capital in the quarter the loan was effective is strongly negatively associated with defaults 90 days prior to contracting, but not defaults in the prior or subsequent quarters. As noted below, the results are robust to the inclusion of different leads and lags of capital.

Using specifications otherwise identical to Panel A of Table 2, I find bank capital has a strong effect on contract strictness, again, conditional on borrower risk and economic conditions. The regression reported under Column (I) controls for both the lagged level of capitalization, as well as the quarterly change in capitalization. Both coefficients are negative significant at the 1% level, suggesting that well-capitalized banks tend to write looser contracts, and that negative shocks to capital induce banks to write stricter contracts. Controlling for either just the lagged level of bank capitalization or the change in capitalization separately, the coefficients remain negative and significant at the 10% and 5% levels, respectively. The unambiguous effect of bank capitalization on contract strictness is consistent with banks behaving more risk aversely with respect to contracts as their capital is depleted.

What about the non-capital effects of recent default experience? The regression reported in Column (IV) includes capitalization controls in lags and levels in addition to the number of lender defaults over the past 90 days. Both capital effects and the effect of recent default experience persist, with the coefficient on recent lender defaults again positive and significant at the 1% level. Meanwhile, in unreported results, the inclusion of an additional eight leads and two lags of the change in bank capital– thereby allowing a considerable lag for the effect of defaults on the lender's balance sheet– does not drive out the effects of recent defaults.

The evidence in Table 5 is noteworthy in two respects. First, the suggested effect of capital on contracting tendencies is a new result in its own right. Second, Column (IV) suggests that the effect of defaults on contract strictness is not driven by changes to the lender's balance sheet. This points to the possibility that lenders learn something from their defaults. I develop and test this hypothesis in the next section.

3.4 Recent defaults and screening ability

While Table 5 presented evidence that bank capital affects the nature of the contract a borrower receives, the persistence of lender default effects in the presence of capital controls suggests that the two effects are distinct. If the effects of loan defaults are not driven by balance sheet concerns, then an alternative hypothesis is that they carry informational content used by the lender in its contracts. Tables 2 and 3, however, went to great lengths to rule out the possibility that lender defaults helped lenders learn about borrower risk.

So what information content do banks attribute to their recent default experience? One potential hypothesis is that banks interpret recent defaults as a reflection on their own screening technology.[14] A large number of defaults, for example, may lead bank managers to update their beliefs regarding the abilities of their loan officers, the adequacy of of bank policies and procedures, or the effectiveness of credit scoring models at identifying borrower risk. Conditional on poor borrower screening, the bank may reasonably write stricter contracts to compensate for their uncertainty regarding borrower risk. After all, active monitoring provides a natural substitute for ex-ante screening. Strict covenants provide the bank the option to renegotiate contracts with borrowers or to limit drawdowns on revolving lines of credit as information is revealed, so that the ex-ante risk assessment becomes less critical.

To test this hypothesis, I compare the differential effect of defaults on loans originated recently and on loans originated in the distant past (or "legacy loans"). As a result of employee turnover and institutional changes to credit policy, the performance of legacy loans should be less informative about the bank's current screening ability than that of new loans. Meanwhile default on newly originated loans and legacy loans should be equally informative about borrower-specific risk and/or the state of the economy. Said differently, defaults on recently originated loans provide management with crisper identification of the talent of their current crop of loan officers and/or the effectiveness of credit models and lending policies being applied to current lending decisions. If defaults affect contract strictness by informing the bank about its own screening ability, then the coefficient associated with defaults on new loans will be larger than the coefficient associated with legacy defaults.

[14] Here, screening refers to the ability of a bank to assess creditworthiness before granting credit approval, in the spirit of Broecker (1990).

Table 6 carries out the test described above. Defaults to lender portfolios 90 days before contracting are counted as before, only sorted into bins based on origination date. All bin counts are again time-demeaned by bank. Meanwhile, including controls for bank capital (both in levels and in differences as in Column IV of Table 5) ensures that the effects are not driven by bank capital.

Columns (I)-(V) in Table 6 report fixed effects regressions of contract strictness on defaults during the 90 days prior to contracting for loans originated in the 720 days prior to contracting, between 720 and 1,440 days prior to contracting, 1,440 and 1,800 days prior to contracting, 1,800 and 3,600 days prior to contracting, and more than 3,600 days prior to contracting, respectively. Whereas all coefficients on recent defaults are positive, only defaults on the newest loans are significant. Moreover, the coefficient magnitudes step down monotonically as loan origination date moves further away from the contracting date. Finally, a regression including all four bins together provides further support for the result in Column (VI). The null hypothesis that the coefficients for the newest and oldest loans are equal is rejected at the 10% level, with a χ^2 statistic of 2.69.

An alternative hypothesis which is also consistent with Table 6 is that other banks learn about the lead arranger's screening ability through recent defaults. After all, other banks will also view defaults on recently originated loans as informative about the lead arranger's screening ability and may be less likely to participate in its syndications. Gopalan, Nanda, and Yerramilli (2008) suggest that, in response, lead arrangers may become less active or retain a larger stakes in the loan. Alternatively, they may ask the borrower for more favorable terms to attract participants. Drucker and Puri (2008), for example, show that tighter covenants facilitate loan sales when the lead arranger is not reputable.

If covenant tightening were driven by damage to the lender's external reputation, however, we would expect the coefficient on defaults to be larger for syndicated loans than for bilateral loans, where the contracting lender's external reputation is less relevant.[15] In order to test this, I create a variable for whether or not a loan is bilateral based on DealScan information. *Bilateral* is set equal to one if DealScan reports the distribution method as either "Sole Lender" or "Bilateral", yielding 253 or 10% of all packages. Otherwise, *Bilateral* is equal to zero.

Table 7 interacts the number of defaults on the lender's loan portfolio in the 90 days leading up to contracting with whether or not the loan was bilateral. If the bank's external reputation is driving contracting changes, the coefficient on *Bilateral* should be negative and significant. Instead, the coefficient is positive (although not significant). This seems inconsistent with lenders tightening contracts to

[15] It is possible that, at some point, loans originated bilaterally could be sold in the secondary market where lender reputation is important. Drucker and Puri (2008), however, suggest that 99% of loans traded in the secondary market were originally syndicated.

compensate for damage to their external reputations.

Combined, Tables 2 through 7 present evidence that bank contracts are dictated by the idiosyncratic default experience– as well as the capital position– of the contracting bank. I have argued that the information content in defaults is not about borrower risk. Instead, the evidence is consistent with lenders using their own default record to learn about their own ability to effectively screen borrowers.

All of this raises the question, why do borrowers submit to contract changes that do not reflect changes in their underlying risk? In the next section, I present evidence that lender effects depend critically on borrowers' breadth of bank relationships and access to alternative sources of cheap financing, and that borrowers who, because of frictions in the bank market, are "locked-in" to lender relationships are exposed to substantial lender-induced contract variation.

4 Lender effects and borrower outside options

Evidence that at a given point in time, contract strictness sorts with the severity of a bank's recent loan loss history and its capitalization raises the question, why do borrowers submit to stricter contracts if their own risk is unchanged? In competitive bank markets, borrowers should choose looser contracts written by unaffected banks over the tight contracts written by affected banks. As a result, in equilibrium, the observed contracts should not provide evidence of any lender effects.

Bank markets, however, are sticky and borrowers are often best served by a small group of lenders and not a perfectly competitive mass of investors. The information produced by banks about a borrower's prospects requires upfront relationship-specific investment by the lender. Meanwhile, smaller bank groups facilitate these investments by providing the lenders with an opportunity to collect rents from future business. Attempts to increase the breadth of lender relationships have been shown to increase the price and reduce the availability of credit (Petersen and Rajan (1994, 1995), Cole (1998)).

Small, tight-knit, bank groups, however, are not an unmitigated good. Rajan (1992) and Sharpe (1990) consider the hold-up costs of bank relationships, whereby the act of becoming informed "locks-in" borrowers to their relationship bank. As a result, even in ex-ante competitive bank markets, lenders exert monopoly power over their borrowers ex-post, forcing firms to accept contacts with non-competitive terms. Detragiache, Garella, and Guiso (2000) suggest that smaller bank groups also subject the borrower to lender liquidity risk. Lenders receiving liquidity shocks may choose to terminate profitable projects early, exposing the borrower to its bank group's funding risk. Slovin, Sushka, and Polonchek (1993) provide evidence consistent with this. They document that borrowers' stock price reactions to the failure and subsequent rescue of Continental Illinois were greatest for borrowers without other bank relationships or access to bond markets.

My final tables explore the relationship between the breadth of lender relationships a borrower maintains and the sensitivity of its contracts to lender shocks. The evidence suggests that dependence on a small group of lenders may be costly ex-post. In particular, broader bank groups hedge borrowers against contract tightening which is unrelated to changes in their own creditworthiness.

Table 8 separates borrowers based on the number of banks used over the last four transactions in order to capture the breadth of a borrower's outside bank options. The current loan is excluded from the lender count so as to limit concerns that the subsamples were determined endogenously. Columns (I) and (II) split the sample into borrowers for which the number of lenders used was below and above the median. Sorting equally ensure that statistical tests will have adequate power to detect differences in coefficients. Because all borrowers have less than four prior transactions at some point in the sample, rather than excluding loans to these borrowers from the analysis, the lender count is scaled by the number of prior transactions used in the calculation.

Comparing Columns (I) and (II), I find that contracts are substantially more dependent on the recent default experience of the lead lender for those borrowers with smaller bank groups. In particular, the coefficient on recent defaults increases by 0.90, with the difference significant at the 5% level. Linking to the earlier interpretation of economic significance in Section 3, in which I noted that a downgrade in the average rated borrower's long-term credit rating increased strictness by 1.25, we can see that for these borrower, the incremental default has the same effect as a ratings downgrade.

Moreover, evidence of bank capital effects also varies across the subsamples. The effects of bank capital on contract strictness are larger in magnitude for tight-knit bank groups, although only the level (as opposed to the change in capital) is significantly different. Taken together, the change in coefficients for the three bank-related regressors is significant at the 1% level, with a $\chi^2(3)$ test statistic of 12.47.

Borrowers, however, may also substitute bank loans for non-bank sources of financing. Diamond (1991) argues for a "life-cycle effect" in firm financing in which borrowers establish their reputation with relationship banks before ultimately graduating to arm's length public markets such as the commercial paper, corporate bond, and equity markets. Just as borrowers with access to multiple banks were able to side-step lender-induced variation in their contracts via competition, my final table suggests that access to arm's-length sources of financing will serve a similar purpose.

Table 9 considers access to the commercial paper market as a natural substitute for bank loans, following Kashyap, Stein, and Wilcox (1993) and Bernanke, Gilchrist, and Gertler (1996), among others. Commercial paper, in contrast to other public debt markets, draws a strict line between firms which are eligible to issue and those which are not, allowing for clean identification of the subsamples. Specifically, borrowers with short-term ratings below A-2 are typically excluded from this market (Nayar and Rozeff (1994) provide several references describing the salient features of the commercial paper market for the interested reader). Moreover, a large majority of loan packages of the DealScan-Compustat covenant sample feature a revolving facility which may be drawn-down and repaid as needed for cash-management purposes. This feature of the loan market is more consistent with commercial paper issuance, which may be used seasonally or for working capital purposes.

I exploit this ratings cut-off in the commercial paper market and identify CP issuers as those borrowers with short-term ratings as good as or better than A-2. I classify borrowers without short-term ratings and those with short-term ratings worse than A-2 as non-CP issuers. Comparing the two subsamples in Columns (I) and (II), the results strongly suggest that borrowers without access to alternative cheap financing find that their contracts are most dependent on time-varying lender attributes. The coefficient on recent defaults is 0.49 smaller for borrowers which have access to commercial paper issuance. The difference is significant at the 10% level, in spite of the small sample of CP issuers. Lender capital effects are also smaller for borrowers with access to commercial paper, although only the change in capital is significant. Meanwhile, a $\chi^2(3)$ statistic of 9.20 testing that the lender effects are jointly different in the subsamples is significant at the 1% level.

Of course, the ratings hurdle that allows commercial paper issuance may signal the creditworthiness of the firm rather than its access to non-bank financing. Column (III) controls for creditworthiness by looking only at borrowers which possess long-term debt ratings above the investment grade threshold of BBB–, but which are not considered commercial paper issuers based on the criteria above. The remaining sample, down to just 279 usable observations, continues to suggest a difference in the magnitude of lender effects for the different subsamples, with CP issuers less exposed to contract variation based on lender factors. A $\chi^2(3)$ statistic of 3.21 testing that the lender effects are jointly different is significant at the 10% level. Meanwhile, although lender effects are not significantly different on an individual basis, the difference in coefficients is consistent with earlier results.

I began this section asking how lender experience and capital effects manifest themselves in contracts amid bank competition for borrowers' business. The final tables suggest perhaps that lender effects do not survive perfect lender competition. Rather, evidence of lender contract effects are driven by borrowers who are reliant upon a small group of relationship lenders and those without access to arm's length debt markets. Whereas earlier work would suggest that these bank-dependent borrowers suffer reduced credit availability following lender shocks (Chava and Purnanandam (2009) and Khwaja and Mian (2008) for example), these results suggest that, conditional on receiving credit, the nature of credit they receive will also be substantially changed. In particular, debt contracts are stricter, making financing more state-contingent and subject to more frequent lender intervention.

5 Conclusion

While prior work exploring the use and strictness of loan covenants has spoken to the interaction between borrower characteristics and contracting choices, I present evidence supporting the importance of lender effects in contracts as well. In particular, I find that banks write tighter contracts than their peers after suffering defaults to their loan portfolio, even when defaulting borrowers are in different industries and geographic regions than the contracting borrower. Moreover, bank capital provides a second channel that determines the strictness of contracts, although this appears to be distinct from the effects of recent defaults.

In understanding the economic mechanisms through which recent defaults may matter, I find evidence that defaults on recently originated loans are more informative than older "legacy" loans held by banks. I argue that this is consistent with bank managers updating their beliefs about their own screening ability, given that old loans were likely to be issued by different loan officers or under antiquated policies.

Finally, evidence seems to point to stickiness in the borrower-lender relationship as perpetuating lender effects. I find borrowers with outside financing options are not subjected to lender-induced contract variation. Instead, it is those borrowers who are most dependent on the relationship aspect of the bank market who have their contracts adjusted based on the changing conditions of their lenders. For these borrowers, stricter contracts impose de-facto state-dependent credit rationing.

The evidence presented raises additional unanswered questions. If, when stressed, lenders allow themselves the option to renegotiate in the future via stricter covenants, how do the affected lenders use this option? While recent research has shed light on the details of renegotiation following technical violations in the context of borrower condition and prospects (Roberts and Sufi (2009a), Nini, Smith, and Sufi (2009a, 2009b) for example), the analysis above alludes to the possibility that lender-specific factors– such as recent default experience and capitalization– may also reasonably influence renegotiation outcomes.

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Table 1: Summary Statistics and Sample Selection. I present summary statistics at the loan level for the merged DealScan-Compustat sample and the sub-sample for which covenants used to calculate loan contract strictness are reported . For loans with multiple lead arrangers, bank summary statistics represent the average of the lead arrangers.

	DealScan-Compustat Sample					
	No. of Loans	Mean	SD	10th	50th	90th
Firm Characteristics						
Total Assets ($M)	22,020	3,515.27	11,993.31	51.71	599.86	8,268.60
EBITDA/Assets	18,305	0.12	0.13	0.03	0.12	0.23
Sales/Assets	21,608	1.15	0.92	0.25	0.98	2.20
Market Value of Equity/Book Liabilities	20,091	3.07	10.77	0.28	1.28	6.04
Has S&P long-term debt rating	22,789	0.43	0.50	-	-	1.00
S&P long-term debt rating	9,813	12.87	3.67	8.00	13.00	17.00
Altman Z-score	15,055	3.62	4.67	0.97	2.83	6.55
Loan Characteristics						
Maturity (months)	20,942	49.06	289.52	12.00	42.00	82.00
Amount ($M)	22,775	349.08	1,016.78	10.00	120.00	800.00
Secured	22,789	0.49	0.50	-	-	1.00
No. of participants	22,789	6.65	9.02	1.00	3.00	16.00
No. of lead arrangers	22,789	1.18	0.52	1.00	1.00	2.00

	DealScan-Compustat Covenant Sample					
	No. of Loans	Mean	SD	10th	50th	90th
Firm Characteristics						
Total Assets ($M)	2,613	2,860.27	8,156.15	63.96	686.18	5,843.10
EBITDA/Assets	2,613	0.15	0.07	0.07	0.14	0.24
Sales/Assets	2,613	1.34	0.90	0.44	1.17	2.38
Market Value of Equity/Book Liabilities	2,557	2.84	4.29	0.51	1.65	6.08
Has S&P long-term debt rating	2,613	0.44	0.50	-	-	1.00
S&P long-term debt rating	1,142	12.36	2.78	9.00	12.00	16.00
Altman Z-score	2,450	3.99	2.94	1.68	3.49	6.59
Loan Characteristics						
Contract Strictness	2,613	21.15	19.46	0.10	16.60	49.85
Maturity (months)	2,588	46.74	21.85	12.00	48.00	61.00
Amount ($M)	2,613	339.72	655.14	15.00	150.00	750.00
Secured	2,613	0.54	0.50	-	1.00	1.00
No. of participants	2,613	8.11	8.35	1.00	6.00	18.00
No. of lead arrangers	2,613	1.21	0.44	1.00	1.00	2.00
Bank Characteristics*						
Lender Total Assets ($BN)	2,385	647.95	3,164.13	53.30	429.96	1,337.91
Lender capitalization	2,384	7.59%	2.14%	4.91%	7.88%	9.48%
Defaults on lender portfolio- past 90 days	2,613	1.48	2.42	-	-	5.00

Table 2: Contract Strictness and Recent Defaults. Panels A and B present borrower fixed-effects regressions. The dependent variable is loan strictness, as described in the methodology section. Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

	Panel A				
Loan Strictness	I	II	III	IV	V
Defaults on lender portfolio- past 360 days	0.19***				
	(0.07)				
Defaults on lender portfolio- past 90 days		0.57***	0.57***	0.56***	0.59***
		(0.19)	(0.19)	(0.18)	(0.18)
Defaults on lender portfolio- 90-180 days		0.16	0.17	0.16	
		(0.19)	(0.19)	(0.18)	
Defaults on lender portfolio- 180-270 days		-0.07	-0.04		
		(0.17)	(0.17)		
Defaults on lender portfolio- 270-360 days		0.16			
		(0.19)			
ln(Maturity)	-0.83	-0.79	-0.80	-0.81	-0.81
	(0.82)	(0.81)	(0.81)	(0.81)	(0.81)
ln(Amount)	1.26	1.28	1.30	1.30	1.28
	(0.93)	(0.92)	(0.93)	(0.93)	(0.93)
Secured	-0.78	-0.79	-0.74	-0.74	-0.78
	(1.51)	(1.51)	(1.50)	(1.50)	(1.50)
ln(# of participants)	1.11	1.15	1.10	1.11	1.13
	(0.96)	(0.97)	(0.98)	(0.98)	(0.98)
Borrower Z-score	-3.95***	-3.96***	-3.99***	-3.98***	-3.98***
	(0.59)	(0.59)	(0.59)	(0.59)	(0.59)
Borrower Z-score^2	0.05***	0.05***	0.05***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	2145	2145	2145	2145	2145
R-squared (partial, excluding unreported fixed effects)	0.193	0.197	0.196	0.196	0.196
Ratings Dummies	YES	YES	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES	YES

Table 2: Contract Strictness and Recent Defaults (cont.)

	Panel B				
Loan Strictness	I	II	III	IV	V
Defaults on lender portfolio- past 360 days					
Defaults on lender portfolio- past 90 days	0.54***	0.55***	0.52***	0.55***	0.58***
	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)
Defaults on lender portfolio- 90-180 days	0.08				
	(0.18)				
Defaults on lender portfolio- 180-270 days					
Defaults on lender portfolio- 270-360 days					
ln(Maturity)	-1.02	-1.05	-1.11	-1.04	-1.08
	(0.82)	(0.82)	(0.81)	(0.82)	(0.81)
ln(Amount)	1.14	1.08	1.12	1.09	1.21
	(0.90)	(0.90)	(0.89)	(0.92)	(0.89)
Secured	-0.72	-0.69	-0.60	-0.65	-0.65
	(1.56)	(1.56)	(1.54)	(1.56)	(1.56)
ln(# of participants)	1.16	1.22	1.22	1.16	1.13
	(0.96)	(0.95)	(0.95)	(0.95)	(0.95)
Borrower Z-score	-4.17***	-4.18***	-4.18***	-4.16***	-4.18***
	(0.61)	(0.61)	(0.61)	(0.61)	(0.61)
Borrower Z-score^2	0.05***	0.05***	0.05***	0.05***	0.05***
Aggregate defaults - past 90 days	0.12	0.16**	0.19**	0.17***	0.17***
	(0.09)	(0.06)	(0.08)	(0.06)	(0.06)
Aggregate defaults - 90-180 days	0.05				
	(0.09)				
Baa-Aaa credit spreads			-1.86		
			(2.73)		
S&P 500 return - past 90 days				0.62	
				(6.34)	
Quarterly GDP growth					0.32
					(0.21)
Observations	2145	2145	2145	2137	2145
R-squared (partial, excluding unreported fixed effects)	0.169	0.169	0.169	0.170	0.171
Ratings Dummies	YES	YES	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES	YES	YES
Year Dummies	NO	NO	NO	NO	NO

Table 3: The Effects of Geographically and Industrially Distinct Defaults. Table 3 presents borrower fixed-effects regressions. The dependent variable is loan strictness, as described in the methodology section. Recent default counts in (I), (II) and (III) exclude defaults in the same 1-digit SIC code as the contracting borrower, the same state (or country for non-US borrowers), or both, respectively. Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

Loan Strictness	Different SIC I	Different State/Country II	Different SIC & State/Country III
Defaults on lender portfolio- past 90 days	0.67***	0.60***	0.64***
	(0.18)	(0.19)	(0.19)
ln(Maturity)	-0.83	-0.80	-0.81
	(0.81)	(0.81)	(0.81)
ln(Amount)	1.30	1.29	1.28
	(0.93)	(0.93)	(0.93)
Secured	-0.78	-0.79	-0.78
	(1.50)	(1.51)	(1.51)
ln(# of participants)	1.12	1.08	1.08
	(0.98)	(0.97)	(0.97)
Borrower Z-score	-3.97***	-3.96***	-3.96***
	(0.59)	(0.59)	(0.59)
Borrower Z-score^2	0.05***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)
Observations	2145	2145	2145
R-squared (partial, excluding unreported fixed effects)	0.197	0.195	0.196
Ratings Dummies	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES
Year Dummies	YES	YES	YES

Table 4: Defaults, Strictness, and New Debt Issuance. Table 4 regresses debt issuance in the 3-year period after the contract is issued on contract strictness. In the 2SLS regression, I instrument for loan strictness using defaults on the lead lender's portfolio in the 90 days prior to contracting (contracting is assumed to be 90 days prior to the facility effective date in order to alow for the mandate, syndication, and documentation processes to be completed- see section on Other Data). I also control for defaults on the lender's portfolio between contracting and the loan effective date (by definition, the 90 days after contracting). Other controls include the borrower's Z-score, the squared Z-score, ln(Maturity), ln(Amount), whether or not the loan was secured, and ln(# of participants). Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

	OLS	2SLS
$\ln(\text{Total Debt})_{t+12}$ - $\ln(\text{Total Debt})_t$	I	II
Loan Strictness	-0.01***	-0.05*
	(0.00)	(0.03)
Defaults on lender portfolio- 90 days after contracting		0.00
		(0.02)
Observations	1328	1328
R-squared (partial, excluding unreported fixed effects)	0.245	-
Kleibergen-Paap rk LM statistic (underidentification test)		6.85***
Other Controls	YES	YES
Ratings Dummies	YES	YES
Borrower Fixed Effects	YES	YES
Year Dummies	YES	YES

Instrumented: Loan Strictness
Instrument: Defaults on lender portfolio- 90 days prior to contracting

Table 5: Capital Effects and Recent Defaults. Table 5 presents borrower fixed-effects regressions. The dependent variable is loan strictness, as described in the methodology section. Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

Loan Strictness	I	II	III	IV
Defaults on lender portfolio- past 90 days				0.63***
				(0.18)
ΔLender capitalization$_t$	-1.49***		-1.15***	-1.43***
	(0.43)		(0.43)	(0.43)
Lender capitalization$_{t-1}$	-0.57***	-0.41*		-0.56***
	(0.21)	(0.21)		(0.21)
ln(Maturity)	-0.57	-0.65	-0.59	-0.61
	(0.87)	(0.87)	(0.88)	(0.87)
ln(Amount)	1.02	1.13	1.09	1.07
	(1.00)	(0.99)	(1.01)	(0.99)
Secured	-1.06	-1.11	-0.97	-1.08
	(1.64)	(1.67)	(1.64)	(1.63)
ln(# of participants)	0.80	0.92	0.84	0.86
	(1.06)	(1.06)	(1.06)	(1.07)
Borrower Z-score	-4.53***	-4.51***	-4.52***	-4.56***
	(0.59)	(0.60)	(0.60)	(0.59)
Borrower Z-score^2	0.05***	0.05***	0.05***	0.05***
	(0.01)	(0.01)	(0.01)	(0.01)
Observations	1860	1886	1860	1860
R-squared (partial, excluding unreported fixed effects)	0.213	0.211	0.208	0.220
Ratings Dummies	YES	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES

Table 6: Contract Strictness and Legacy Defaults. Table 6 presents borrower fixed-effects regressions of loan strictness, as described in the methodology section, on the number of defaults in the 90-days prior to contracting, capitalization, and controls. Defaults are sorted based on whether the defaulting loans were originated in the 720 days prior to contracting, between 720 and 1,440 days prior to contracting, 1,440 and 1,800 days prior to contracting, 1,800 and 3,600 days prior to contracting, or more than 3,600 days prior to contracting. Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

Loan Strictness	I	II	III	IV	V	VI
(i) Lender defaults (loans<720 days old)	0.61**					0.62*
	(0.25)					(0.34)
(ii) Lender defaults (720 days old<loans<1,440 days old)		0.59**				0.44
		(0.29)				(0.45)
(iii) Lender defaults (1,440 days old<loans<1,800 days old)			0.43			0.72
			(0.32)			(1.04)
(iv) Lender defaults (1,800 days old<loans<3,600 days old)				0.25		-0.05
				(0.31)		(0.82)
(v) Lender defaults (loans>3,600 days old)					0.23	-1.14
					(0.31)	(1.03)
(i)-(v)						1.76*
ΔLender capitalization$_t$	-1.42***	-1.35***	-1.36***	-1.37***	-1.38***	-1.37***
	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)	(0.44)
Lender capitalization$_{t-1}$	-0.52**	-0.53**	-0.53**	-0.55**	-0.55**	-0.55**
	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)	(0.22)
ln(Maturity)	-0.76	-0.74	-0.77	-0.78	-0.75	-0.72
	(0.92)	(0.92)	(0.92)	(0.92)	(0.92)	(0.91)
ln(Amount)	1.76*	1.73*	1.75*	1.75*	1.73*	1.73*
	(1.04)	(1.04)	(1.04)	(1.04)	(1.04)	(1.04)
Secured	0.25	0.34	0.39	0.31	0.32	0.36
	(1.68)	(1.69)	(1.70)	(1.69)	(1.69)	(1.69)
ln(# of participants)	0.98	0.92	0.91	0.88	0.89	0.93
	(1.09)	(1.09)	(1.09)	(1.09)	(1.09)	(1.10)
Borrower Z-score	-1.18***	-1.20***	-1.22***	-1.21***	-1.21***	-1.18***
	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)	(0.29)
Borrower Z-score^2	0.01**	0.01**	0.01**	0.01**	0.01**	0.01**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	1857	1857	1857	1857	1857	1857
R-squared (partial, excluding unreported fixed effects)	0.150	0.153	0.150	0.149	0.149	0.155
Ratings Dummies	YES	YES	YES	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES	YES	YES

Table 7: Effects of defaults on internal and external reputation. Table 7 presents borrower fixed-effects regressions of loan strictness, as described in the methodology section, on the number of defaults in the 90-days prior to contracting, capitalization, and controls. I allow for an interaction between defaults on the lender's portfolio in the 90 days leading up to contracting and whether or not the current loan is syndicated or bilateral. The variable bilateral is equal to one if DealScan reports the distribution method as "sole lender" or "bilateral". Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

Loan Strictness	
Defaults on lender portfolio- past 90 days	0.60**
	(0.18)
Defaults on lender portfolio- past 90 days X Bilateral	1.44
	(0.88)
Bilateral	-1.23
	(3.01)
ΔLender capitalization$_t$	-1.35***
	(0.43)
Lender capitalization$_{t-1}$	-0.56***
	(0.21)
ln(Maturity)	-0.62
	(0.86)
ln(Amount)	1.03
	(0.98)
Secured	-1.06
	(1.62)
ln(# of participants)	0.86
	(1.06)
Borrower Z-score	-4.59***
	(0.59)
Borrower Z-score^2	0.05***
	(0.01)
Observations	1860
R-squared (partial, excluding unreported fixed effects)	0.221
Ratings Dummies	YES
Borrower Fixed Effects	YES
Year Dummies	YES

Table 8: Contract Sensitivity and Lender Relationships. Table 8 presents borrower fixed-effects regressions of loan strictness, as described in the methodology section, on the number of defaults in the 90-days prior to contracting, capitalization, and controls. To estimate the breadth of lender relationships available to a borrower, I count the number of banks which have lent to a given borrower, going back up to four transactions. Because some borrowers have less than four prior deals, the number of lenders is scaled by the number of prior loans observed, up to four. Columns (I) and (II) split the sample into borrowers for which the number of lenders used in the prior four transactions was less than or greater than median. Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

	# Lender Relationships		
	≤median	>median	
Loan Strictness	I	II	I-II
Defaults on lender portfolio- past 90 days	1.24***	0.35*	0.90**
	(0.32)	(0.21)	
ΔLender capitalization$_t$	-0.52	-0.24	-0.27
	(0.32)	(0.21)	
Lender capitalization$_{t-1}$	-3.14***	-0.23	-2.90***
	(0.92)	(0.45)	
ln(Maturity)	-2.24	0.41	
	(1.40)	(1.16)	
ln(Amount)	1.13	1.22	
	(1.47)	(1.62)	
Secured	0.24	-1.16	
	(2.31)	(2.43)	
ln(# of participants)	2.94*	-0.87	
	(1.78)	(1.60)	
Borrower Z-score	-5.37***	-16.23***	
	(0.72)	(4.20)	
Borrower Z-score^2	0.05***	1.12***	
	(0.01)	(0.40)	
Observations	801	805	
R-squared (partial, excluding unreported fixed effects)	0.300	0.267	
Ratings Dummies	YES	YES	
Borrower Fixed Effects	YES	YES	
Year Dummies	YES	YES	$\chi2(3)$= 12.47***

Table 9: Contract Sensitivity and Alternative Financing. Table 9 presents borrower fixed-effects regressions of loan strictness, as described in the methodology section, on the number of defaults in the 90-days prior to contracting, capitalization, and controls. Column (I) examines a sub-sample of borrowers with short-term ratings at or above A-2 as these firms have access to commercial paper markets. Column (II) examines borrowers without short-term ratings and those rated weaker than A-2. (III) considers firms which are rated investment grade according to their long-term ratings, but do not have short-term ratings or are rated below A-2. Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

Loan Strictness	CP Issuer I	Non-CP Issuer II	I-II	Investment Grade, Non-CP Issuer III	I-III
Defaults on lender portfolio- past 90 days	0.19	0.68***	-0.49*	0.77*	-0.58
	(0.15)	(0.24)		(0.40)	
ΔLender capitalization$_t$	-0.30	-1.73***	1.43*	-0.98	0.68
	(0.46)	(0.57)		(1.02)	
Lender capitalization$_{t-1}$	-0.13	-0.69***	0.55	-0.37	0.23
	(0.25)	(0.24)		(0.28)	
ln(Maturity)	-0.27	-0.39		3.83	
	(1.11)	(1.24)		(2.54)	
ln(Amount)	2.90*	0.79		-0.54	
	(1.62)	(1.19)		(2.01)	
Secured	1.62	-2.47		-1.95	
	(2.89)	(1.79)		(4.95)	
ln(# of participants)	-2.27	1.48		-0.84	
	(1.54)	(1.24)		(1.55)	
Borrower Z-score	-13.89***	-4.79***		-17.57***	
	(3.57)	(0.63)		(5.04)	
Borrower Z-score^2	1.08***	0.05***		0.85*	
	(0.29)	(0.01)		(0.48)	
Observations	373	1479		279	
R-squared (partial, excluding unreported fixed effects)	0.486	0.228		0.389	
Ratings Dummies	YES	YES		YES	
Borrower Fixed Effects	YES	YES		YES	
Year Dummies	YES	YES	$\chi2(3)$= 9.20***	YES	$\chi2(3)$= 3.21*

Appendix

Table A1: Measure Validation. I present probit regressions of borrower covenant violations occuring during the tenor of a given loan contract on three measures of loan strictness for that contract: Strictness (the new measure described in the methodology section), the number of financial covenants, and the slack of the net worth or tangible net worth covenant (ATQ-LTQ-Covenant Level or ATQ-LTQ-INTANQ-Covenant Level, respectively, in each case scaled by book assets). Covenant violation data comes from Nini, Smith, and Sufi (2009). I consider only new covenant violations, consistent with the authors' instructions, by excluding violations where the borrower had a violation within the past four quarters. Standard errors are clustered by borrower, robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

Covenant Violations	I	II	III	IV	V	VI
Strictness	0.74***			0.75***	1.04***	1.06***
	(0.19)			(0.20)	(0.27)	(0.29)
Number of Financial Covenants		0.03		-0.01		-0.01
		(0.03)		(0.04)		(0.05)
Slack Net Worth Covenant			-1.31***		-0.48	-0.47
			(0.47)		(0.51)	(0.51)
ln(Maturity)	0.48***	0.48***	0.57***	0.48***	0.58***	0.58***
	(0.07)	(0.07)	(0.09)	(0.07)	(0.09)	(0.10)
ln(Amount)	-0.14***	-0.12***	-0.15**	-0.14***	-0.14**	-0.15**
	(0.04)	(0.04)	(0.06)	(0.04)	(0.06)	(0.06)
Secured	0.30***	0.34***	0.51***	0.30***	0.46***	0.46***
	(0.08)	(0.08)	(0.11)	(0.08)	(0.11)	(0.11)
ln(# of participants)	0.01	0.00	0.01	0.01	0.01	0.01
	(0.06)	(0.06)	(0.08)	(0.06)	(0.09)	(0.09)
Borrower Z-score	0.01	-0.00	-0.03	0.01	-0.02	-0.02
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Borrower Z-score2	-0.00	0.00	0.00	-0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	2050	2050	982	2050	982	982
Log likelihood	-959.44	-967.89	-507.677	-959.41	-500.37	-500.32
Ratings Dummies	YES	YES	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES	YES	YES