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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 payment 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 compression in bank equity is also strongly associated with tighter contracts. The evidence is most consistent with lenders using their default experience to make inference about their screening ability and adjusting contracts accordingly. Finally, contract tightening is most pronounced for borrowers who are dependent on a relatively small circle of lenders, with a one standard deviation increase in lender defaults implying covenant tightening nearly equivalent to that of a two-notch downgrade in the borrower's own credit rating.

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Just as credit volumes have swung wildly over the past several years, the terms of loan contracts issued have been equally fickle. Financial covenants requiring borrowers to maintain financial ratios within pre-determined ranges were abandoned *en masse* during the easy credit period from 2002-2006. In the aftermath of 2008's financial crisis, contracts 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 (1992), 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 (2008), Lin and Paravisini (2010), 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 portfolio performance to the tightening of bank credit standards and lending volumes. Finally, Gopalan, Nanda, and 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 based on 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, a one standard deviation increase in defaults to a lender's portfolio induces contract tightening roughly equivalent to what a borrower could expect to receive following a downgrade in its own long-term debt rating.

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.[3] 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.[4] 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 lender capitalization levels and changes. Second, after partialling-out the independent effect of defaults, bank capitalization seems to provide 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 contractions in bank equity is associated with stricter contracts. The direction of the effect is consistent with under-capitalized banks behaving more conservatively to protect their remaining capital, or alternatively, with lenders who write risky contracts requiring additional capital cushion.

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. 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 stronger 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 older, less informative, legacy loans.

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 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 (loans not intended to be sold to other banks by the lender) are equally, if not more, sensitive to the lender's recent default experience than are covenants in syndicated loans, indicating that the importance of the lender's reputation in the secondary loan market may be limited.

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 defaults 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. Under the threat of stricter loan contracts, these borrowers benefit from access to cheap non-bank financing. More importantly, however, even within the bank market, these typically larger and more established borrowers tend to enjoy greater competition among banks for their business. Using sharp ratings cut-offs which dictate access to the commercial paper market, I find that commercial paper issuers are substantially less exposed to contract variation based on lender defaults.

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, the magnitude of the effect observed is as much as twice that of the full sample, such that a one standard deviation increase in lender defaults has an effect on the borrower's contract roughly equivalent to the effect of a two-notch downgrade (precisely, a 1.87 notch downgrade) in the borrower's own credit rating.

I. Methodology

A. Measurement

The analysis promised requires an empirical measure of contract strictness– and one which corresponds to a well-defined meaning of "strictness"– along with the appropriate data and identification scheme. In this section, I'll propose 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 the option to demand immediate repayment on a loan which has yet to reach its stated maturity if, for example, borrower 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). My goal is to provide a measure that captures the intuitive properties from 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 covenant slack–that is, the distance between the borrower's accounting numbers at the time the contract is written and what is allowable under the covenants specified. 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 $r' < \underline{r}$ allocates control to the lender, then

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

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.[5]

While derived from an admittedly stylized model– in particular, accounting ratios are likely to be generated by a more complicated, less accessible distribution than that of the multivariate normal– the resulting measure of contract strictness has a number of the desirable properties laid forth above.[6] It 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. Meanwhile, it provides for a natural economic interpretation as a stylized probability of lender control based on covenant violation, or more generally, the inverse of a borrower's distance to technical default.

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 logged financial ratios of levered Compustat firms.[7] To allow for variation in the correlation structure of ratios, both cross-sectionally and over time, I estimate a separate covariance matrices for each one-digit SIC industry, every year, such that $\Sigma_{I,Y}$ reflects the correlation structure in industry I estimated with data available at year Y. More will be said about the calculation of this measure of strictness in the forthcoming discussion of data used in the paper.

B. 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 to 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 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 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 and are defined in the appendix of this paper based on the most common constructions.[8]

I eliminate contracts which appear to be in violation within the first quarter. This leaves 2,642 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 levels would be inflated by using facility level observations rather than package level observations. Of the remaining contracts, 20.8% have multiple lead arrangers, each of which are matched to the contract. After matching loan packages to the relevant lead arrangers, I have 3,571 borrower-lender contracts available for analysis.

In order to generate the measure of contractual strictness defined in the prior section, I first estimate the variance-covariance matrix associated with the quarterly changes in logged financial ratios of levered firms using Compustat data. Looking at ratios in natural logs extends the support of otherwise constrained ratios (for example, leverage must be greater than zero) to more closely approximate a multivariate normal distribution for changes in the ratios. Meanwhile, 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 and over time, using rolling ten-year windows of backwards looking data to estimate Σ for each on an industry-by-industry basis. Although the results presented hereafter allow for this variation, they are substantially the same as results estimated using a single pooled 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, measurement error is likely be largely driven by borrower-specific components, which will be subsumed by borrower fixed effects 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.[9] 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.

Insert Figure 1 here

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 violations provided by Nini, Smith, and Sufi (2009b),[10] I estimate probit regressions of whether or not a violation occurred during the life of the loan on the proposed measure of contract strictness, the borrower's Altman Z-score, the natural log of tangible net worth, debt/tangible net worth, fixed-charge coverage, and current ratios, and as dummy variables for the borrower's S&P long-term debt rating.[11] I also include controls for loan characteristics, including the loan's maturity in months, amount, the presence of collateral, and number of participants, as well as time dummies. 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.[12] The results, presented in Table I, 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. Finally, 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. The significance of the proposed strictness measure gives comfort that the measure is in fact indicative of stricter contracts, but also presumably that stricter contracts are in fact predictive of violations.[13]

Insert Table I here

B.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 borrowers reported to be in default or selective default by Standard & Poor's (S&P) in Compustat's ratings database. This captures borrowers which have had a payment default on at least one obligation. This count may miss defaults by small, unrated borrowers, but will capture visible defaults likely to sway loan officer behavior.

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 contract in the period leading up to its issuance. Lenders with no record of a default at any point in the

20 year sample are excluded from the analysis. Finally, I demean default counts by lender, subtracting off the lead arranger's average default count in the sample. This removes the effect that lender size might have on default counts and contracting tendencies. Alternatively, we might have included lender fixed effects in the regression. These specifications are among the robustness checks included in the paper's Internet Appendix.

In defining lenders, I rely primarily on the lender names as reported in *DealScan*. In the event that a regional branch or office (e.g. Bank of America Arizona and Bank of America Oregon) is listed as the lender of record, I combine the regional offices under a single bank name (e.g. Bank of America). Similarly, broker-dealer or business banking segments (e.g. Bank of America Securities and Bank of America Business Capital) may also be aggregated under the parent's name. In dealing with bank mergers and acquisitions, I create a new institution if the merger results in both lenders changing their names under the assumption that such mergers are likely to result in a substantially different institution from either of its predecessors. However, in cases where lenders retain an independent brand and/or legal status after an acquisition, *DealScan* may continue to report lending activity separately (e.g. LaSalle Bank continues to appear in *DealScan* after its acquisition by ABN Amro). In these cases, I follow *DealScan* and treat the institutions separately as well, except that capital will be measured at the level of the ultimate parent (see below). Note that these choices are not critical to the main result of the paper, which can be reproduced either by treating each bank office as a separate lender, or alternatively, by aggregating all wholly-owned subsidiaries under the ultimate parent.

Finally, it is necessary to make mild assumptions about the timing of contracts. *DealScan* reports the facility start date as the legal effective date of the loan. However, the terms of a loan are negotiated well in advance of this date. Practitioner estimates suggest that the average syndicated transaction takes 2 months, 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)).[14] However, in addition to this, it may take as long as a month between the time a bank approves a term sheet and receives a mandate. It is during this pre-mandate phase when banks commit to loan 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 (1 month prior to receiving a mandate and 2 months in the syndication/documentation process). 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 (as opposed to contemporaneous versions of the same measures), 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.

Insert Table II here

Table II 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 \$3.10 billion and median total assets of \$818.30 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 47.64 (57) months, a mean (median) size of $411.35 million ($200 million), attract an average (median) of 9.25 (7) participant banks, and most importantly, have a mean (median) strictness of 22.51% (17.47%). Finally, I also report the characteristics of lead lenders for the sample loans. Lenders have average (median) total assets of $589.79 billion ($450.56 billion), mean (median) capitalization of 7.51% (7.77%) and experience an average (median) of 1.51 (0) defaults in the 90 days leading up to a loan contracting date. For the sample, the average ratio of defaults to total loans outstanding a bank has in *DealScan* is 0.1%, with a median of zero and a range of 0-4%.

II. 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 (Chava and Purnanandam (2009), Berger and Udell (2004), Gopalan, Nanda, and 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 falls 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 as an additional control to capture repayment risk for unrated firms and to allow for potentially lagged responses to distress by rating agencies, as well as debt/tangible net worth, fixed-charge coverage, current ratio, and logged tangible net worth. The latter controls cover leverage, cash flows, liquidity, and size and were chosen to reflect the accounting ratios which banks both are likely to use in their analysis of borrowers as well as in their contracts.

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 lenders are randomly assigned to a borrower, then pooled OLS is unbiased and efficient. If, however, lenders select borrowers 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 seek 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 the effects of selection on unobservables, the analysis depends on borrower fixed effects. Holding the borrower fixed, we ask, how does the contract that borrower A receives after its lender has suffered a relatively large (or small) number of defaults compare to its average contract. By focusing within borrower, we eliminate the possibility that default experience is correlated with unobservable borrower characteristics which are fixed over time.

Clear identification also requires that lender defaults do not proxy for unobservable macroeconomic risk which is neither captured in accounting controls, nor in the time-invariant fixed effects. In particular, time-series variation in contract strictness appears to have important business-cycle components which affect all banks and borrowers simultaneously. Time 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. I begin the analysis using year dummies– which placebo tests confirm are sufficient to isolate lender-specific effects from market effects– although the main results of the paper are unchanged using more granular time effects. I also pursue alternative specifications in which aggregate measures of macroeconomic risk, including economy-wide defaults, may substitute for time 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 IV.

Finally, equation (5) also includes controls for 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, although the exclusion of any or all of these transaction level controls does not alter the main findings of the paper. Insert Table III here

Panel A of Table III begins by estimating the fixed-effect regression of loan strictness on recent defaults and appropriate controls, as described above. Standard errors are double clustered at the level of the borrower and the lender. Clustering along the borrower's dimension allows for a possibly temporary firm effect, whereas clustering along the lender dimension accounts for the fact that lenders' default experiences and contracting tendencies may be correlated across different contracts. Clustering by year generates standard errors of roughly similar magnitudes, suggesting that time series variation is appropriately captured by the controls (Petersen, 2008). 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, demeaning counts by lender.

The results suggest a significant tightening by banks in response to recent defaults. The effects of defaults over the 360 days prior to contracting suggest a 0.12 increase in strictness for a given borrower for each incremental annual default to the lead lender (with strictness ranging from 0 to 100). This response is significant at the 5% level (and is robust to assuming a contracting date 30 or 60 days prior to closing). Columns (II)-(V) are consistent with a short-lived effect. The experience in the past 90 days is significant at the 5% level, whereas the effect steps down for less recent defaults.[15] Meanwhile, firm ratings dummies in the regression are jointly significant and confirm the findings of prior work, that observably riskier firms receive stricter contracts. The sign and significance of Altman's Z-score mirrors this. Of the loan controls, only loan amount is significant, and any or all can be removed from the regression without materially 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.

Were this 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) Altman's Z-scores after high 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, for example, have a correlation of 0.05 with their borrower's Altman's Z-scores (which increase as borrower risk is reduced), significant at the 1% level. Similarly, defaults have a -0.05 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, significant at the 5% level. Combined, 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 suggest 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. The immediacy of the effect observed, however, raises concerns that the annual time dummies are not fine enough to capture high frequency changes in macroeconomic risk. An obvious response is to increase the periodicity of time dummies. In fact, quarterly dummies produce a nearly equivalent coefficient on 90 day defaults (0.38 compared to 0.39), significant at the 5% level. These results are presented in the Internet Appendix along with other robustness tests. However, this fails to fully resolve the broader point. Moreover, the quarterly time dummies, in combination with borrower fixed effects, rating dummies, and clustering at the borrower and lender level, exhibit symptoms of over-fitting and are not feasible in smaller subsamples.[16]

I address this in a number of ways. First, the nature of the data and hypothesis being tested affords a unique opportunity to assess the actual size (that is, the frequency of type I errors or false positives) of the statistical test being performed, even under misspecification. I achieve this using placebo tests which substitute the default experience of the contracting lender with that of a rival bank which was active during the same year as a lead arranger, but not as an arranger in the current transaction. If, in fact, the coefficient on the lender's default experience is a spurious response to latent macroeconomic risk, then substituting the experience of a rival bank operating in the same environment will deliver equivalent results. If, instead, the model is well specified and time dummies adequately absorb latent macro factors, then the experience of rival banks will not load, except as as result of random variation in estimated coefficients. Using random reassignment of contracts to placebo lenders– again, lenders who were active in the contract year but not arrangers on the current transaction– and repeating the experiment 500 times, I find strong support for my model specification. Placebo banks fail to achieve positive and significant (at the 10% level) coefficients on their recent default experience for all but 6% of the simulations. This roughly coincides with the predicted size of the test and seems to strongly support the specification and its finding that it is the lender's *own* defaults that matter when contracting.

Panel B makes this point more explicit, 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 drive out the effects of a given lender's idiosyncratic experience. Moreover, unlike those reported in Panel A, the specifications in Panel B allow us to directly observe the effect of aggregate defaults on contract strictness.

The findings are consistent with earlier results. 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 suggests 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.

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

A valid concern with the estimates provided in Table III 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 will entirely capture the borrower risk that a given lender is facing. A similar story could be told for lenders which specialize in a particular industry– an oil and gas lender pays attention to their own default experience because it is more informative of oil and gas borrower risk than the aggregate. Whereas these problems may be insurmountable using lending and loan performance data which has been aggregated at the bank level in call reports or Compustat data, the availability of loan-by-loan performance and contract data affords us the opportunity to consider the effects of defaults on contracts in plausibly independent sectors of the economy.

To this end, Table IV 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.

Insert Table IV here

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 demeaned by lender and standard errors are clustered along borrower and lender dimensions. For defaults in different geographic regions, the estimated coefficient on recent defaults is significant at the 5% level, whereas the effects for defaults in different industries and different and geographic regions are significant at the 1% level. Coefficients are also of comparable magnitude to the estimates in Table III (even a bit larger, although after standardizing variables, the economic significance is comparable), reinforcing the theme that lender defaults are not a function of borrower risk, but a distinct lender effect.

How large are the effects of recent defaults on the contract the borrower receives? If we were to interpret the derived strictness measure as a true probability of contingent lender control within the quarter, then using the coefficient estimates presented in Table IV, a one standard deviation (2.5) increase in lender defaults for the median contract strictness (probability of violation of 17.5) increases the probability of lender control from approximately 53.6% to 56.3%.[17] The effects, however, are more dramatic if we consider firms for which violations are less common. A firm in the 10th percentile of contract strictness has close to zero probability of violation in the first year. After the effect of a one standard deviation increase in lender defaults, the probability of a violation increases to 5% in the first year and 35% over the facility's first three years. Given that covenant violations have been shown to reduce investment on the order of magnitude of 1% of capital (Roberts and Chava (2008) and reduce annual debt issuance by 2.5% of assets (Roberts and Sufi, 2009), the increased probability of a violation should be considered, in expectation at least, material to the firm's real and financing decisions.

Alternatively, in the context of prior studies linking borrower risk to covenant choice, 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 1.3, 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 equal to that of 2.7 additional defaults to the lender's loan book (just over a one standard deviation change in defaults). As later tables will show, the magnitude of this effect is even larger when we consider firms which have limited alternative sources of financing.

B. Distinguishing capital effects from other effects

So far, the motivation and presentation of results has remained atheoretical, eschewing the interesting question of why lenders tighten their contracts in response to their own default experience. In the following sections, I'll 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.

The effect of capitalization on bank behavior has been extensively studied as it pertains to the credit channel literature, although to my knowledge, this has not included any discussion of covenant strictness. Loan losses and other shocks to capital, for example, are thought to reduce the volume of credit supplied by affected banks. Peek and Rosengren (1997) show that depletions in regulatory capital of Japanese banks between 1989 and 1992 led to significant reduction in branch lending, whereas Houston, James, and Marcus (1997) demonstrate that capital shocks to subsidiaries of a bank holding company are transmitted to other subsidiaries in different markets (Kashyap, Stein, and Hanson (2010) provide a useful survey of this literature). Capital 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 balance sheet effects of recent defaults from alternative channels, Table V controls for bank capitalization ratios and changes to bank capitalization ratios around the time the loans were granted. From Compustat, I calculate the capitalization of the lead bank as Shareholder Equity/Total Assets as of the quarter the facility became active in *DealScan*, although I will include various leads and lags to allow variation in how long it takes for charge-offs to flow through the bank's balance sheet. If covenants respond to defaults only indirectly through changes in lender capital driven by large numbers of defaults, then we expect a significant and positive coefficient on capitalization, and more importantly, a diminished coefficient on recent defaults.

Insert Table V here

Using specifications otherwise identical to Panel A of Table III (only now considering contracts for which there are data on current and lagged lender capitalization) I find bank capital is strongly associated with contract strictness, again, conditional on borrower risk and economic conditions. The regression reported under Column (I) controls for the level of capitalization, whereas Column (II) considers the effects of changes to the lenders capitalization ratio. The level of capitalization is negative and significant at the 10% level, whereas changes to capitalization are negative and significant at the 1% level, suggesting that well-capitalized banks tend to write looser contracts and that, furthermore, changes in capitalization are similarly associated with changes in contracting. Using leads and lags for the change in capital to capture a lead-lag relationship between defaults and charge-offs, I find that the capitalization in the quarter of loan issuance continues to be negatively related to contract strictness, although neither the lead nor the lag is. 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. Meanwhile, and perhaps more importantly, in each of the specifications, the effect of recent default experience persists, with the coefficient on recent lender defaults again positive and significant at the 5% level.

The evidence in Table V is noteworthy in two respects. First, the implied effect of bank capital on contracting is a "supply-side" effect in its own right. This is consistent with the broader claim that contract formation is not independent of lender characteristics. Second, and more importantly however, the stability of the coefficient on loan defaults, even in the face of various controls for lender capitalization, suggests that the effect of defaults on contract strictness is not in fact driven by a contemporaneous deterioration in the lender's balance sheet. The similarity of estimates from specifications with and without capitalization controls and the evidence that capitalization and contracting co-vary together, combine to suggest a weak empirical relationship between corporate loan defaults and the capitalization levels of individual banks (this is perhaps not surprising, given the size of lenders in the sample). So while, in theory, defaults large enough to materially impact the capital of a large bank might plausibly drive contracting by way of a balance sheet effect, this is not the primary mechanism at work in our sample. Instead, the next section explores the possibility that lenders infer something from their defaults and adjust contracts to incorporate their new information.

C. Recent defaults and screening ability

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 III and IV, however, went to great lengths to rule out the possibility that lender defaults helped lenders learn about borrower risk.

So what information might banks glean from their recent default experience? One hypothesis is that banks interpret recent defaults as a reflection of their own screening technology.[18] A large number of defaults may lead bank managers to update their beliefs regarding the abilities of their loan officers, the adequacy of bank policies and procedures, or the effectiveness of credit scoring models at identifying borrower risk. Udell (1989), for example, finds evidence that banks monitor the continuing quality and performance of their loan portfolio (the so-called loan review function) as a means to monitor the performance of their loan officers. Conditional on believing that it does a poor job of screening, the bank may reasonably write stricter contracts which provide it the option to renegotiate with borrowers or to limit drawdowns as conditions change or new information is revealed, effectively substituting ex-post monitoring for ex-ante screening. Meanwhile, regular changes to lending practices and employee turnover suggest that constant reevaluation of screening quality may be necessary.

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 the aforementioned turnover in loan officers and credit policies, 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 or training of their current vintage 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.

Table VI carries out the test described above. Defaults to lender portfolios 90 days before contracting are counted as before, only sorted into bins based on the origination date of the defaulting loans. All bin counts are again demeaned by bank. Including controls for changes in bank capitalization ratios guards against differential effects on bank capital.

Insert Table VI here

Columns (I)-(IV) in Table VI report fixed effects regressions of contract strictness on defaults during the 90 days prior to contracting for loans originated in the year prior, between one and two years prior, two to 5 years prior, or more than 5 years prior to the current contract. Whereas all coefficients on recent defaults are positive, only defaults on the newest loans are significant at the 5% level. Meanwhile, coefficients appear to be monotonically decreasing as the vintage of the defaulting loan gets older, although this trend is purely suggestive and not statistically significant. Note that standard errors have more than tripled with respect to earlier tables, a result of reduced variation in the independent variable of interest.

To statistically test the differential effects of different default vintages, Column (V) estimates the effects of just the newest (loans less than one year old) and oldest vintage of defaults (loans greater than 5 years old) jointly. The bins have been selected to capture roughly the top and bottom deciles of loan age at the time of default, such that the default counts have comparable means (0.14 and 0.13 respectively) and standard deviations (0.45 and 0.44, respectively). This ensures the tests are not an artifact of scaling. In the joint estimation, I find a 1.64 difference in estimated coefficients. The differential effect is significant, albeit only at the 10% level.

An alternative hypothesis which is also consistent with Table VI 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 stake in the loan, and in fact report very similar findings that defaults that happen soon after loan origination are most damaging to lead arranger reputation. To compensate for their damaged reputation and as an alternative to taking larger shares of the transactions, lenders 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.[19] In order to test this, I create an indicator variable for bilateral loans based on *DealScan* information. *Bilateral* is set equal to one if *DealScan* reports the distribution method as either "Sole Lender" or "Bilateral", yielding 234 or 8% of all packages. Otherwise, *Bilateral* is equal to zero.

Insert Table VII here

Table VII 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 compensate for damage to their external reputations.

Combined, Tables III through VII 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.

Admittedly, the evidence presented is suggestive of, but not necessarily exclusive to one particular hypothesis. A natural alternative explanation might be that recent default experience drives time variation in risk aversion of loan officers with career concerns. If, for example, an individual loan officer is to be fired for her next defaulting loan, then her borrowers are likely to face tighter lending going forward. Meanwhile, given the natural turnover in loan officers over time, defaults on the older "legacy" loans analyzed in Table VI may be unrelated to the career concerns of the current vintage of loan officers, again consistent with the evidence presented.

However, also note that the results presented in Table IV suggest that default effects are transmitted across the bank and are not necessarily region or sector specific. This implies that, in the case of a large bank for which coverage areas are likely to be region or sector specific, the contracting loan officer and the loan officer tied to the default are unlikely to be the same. As a result, time variation in risk tolerance driven by past borrower defaults would need to occur at the level of a senior, centralized authority (e.g. the chief credit officer) and not at the level of individual loan officers.

Finally, it is worth challenging the notion that the large banks in the sample are sufficiently nimble to gather information about defaults in California and impose lending guide-lines on new business in West Virginia (to return to the example described in the introduction). This requires a central command which has authority in lending decisions across markets and also monitors the ex-post performance of loans for the entire portfolio. At the institutional level, we might think of a bank's central credit committees as performing this function. Credit committees are composed of control (risk) specialists and lending (client) specialists and serve as the final approval point before the loan officer can offer a commitment to lend to a borrower. These committees may also monitor the ongoing performance of the portfolio which they approve. As a result, recent defaults may be particularly salient to the terms of new approvals when discussed in the same meeting.[20] Meanwhile, unlike consumer or small business lending, the relatively small number of large transactions in the syndicated loan market makes it possible for all new loans to go through a single committee, even for large banks.

III. Lender effects and borrower outside options

Why do borrowers submit to stricter contracts when their own risk profile is unchanged? In a competitive funding market, borrowers would be expected to seek out looser contracts written by unaffected banks before accepting the tighter contracts written by troubled lenders. In equilibrium, we might therefore expect the observed contracts to be invariant to any lender-specific effects.

Prior work, however, suggests that opaque borrowers may be best served by a small group of relationship lenders and not a perfectly competitive mass of investors (Petersen and Rajan (1994, 1995), Cole (1998)). Repeated interactions between banks and their borrowers reduce information asymmetry over time and allow for upfront relationship-specific investments by lenders. At the same time, exclusive bank-borrower relationships may also present lenders with hold-up opportunities (Rajan (1992) and Sharpe (1990)) as borrowers become "locked-in" and, moreover, expose borrowers to the risk of lender distress or failure. Slovin, Sushka, and Polonchek (1993) for example, 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 connection 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, with broader bank groups hedging borrowers against contract tightening unrelated to changes in their own creditworthiness.

Insert Table VIII here

Table VIII begins by separating 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 ensures 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, clubbier bank groups. In particular, the coefficient on recent defaults increases by 0.89, with the difference significant at the 1% level. Linking to the earlier interpretation of economic significance in Section II.A in which I noted that a downgrade in the average rated borrower's long-term credit rating increased strictness by 1.3 units, we can see that for these borrowers, a one standard deviation in defaults (2.5) has nearly an equivalent effect as a two-notch downgrade in the borrower's own rating (more accurately, the effect of 1.86 ratings downgrades).

Meanwhile, Table IX considers how the availability of non-bank sources of funding impacts borrowers' contract sensitivity to their lenders' defaults. In particular, I exploit the strict ratings cut-off between commercial paper (CP) issuers and non-commercial paper issuers (borrowers with short-term ratings below A-2 are typically excluded from the CP market).[21]

Insert Table IX here

On one hand, borrowers with access to the CP market benefit directly from the outside option provided by the commercial paper issuance, and therefore are less likely to accept non-market terms from their lenders. This idea follows several papers which treat the commercial paper market as a natural substitute for bank borrowings (Kashyap, Stein, and Wilcox (1993), Bernanke, Gilchrist, and Gertler (1996), among others). Borrowers which can readily issue commercial paper, however, are also typically larger and more established firms. Diamond (1991), for example, describes a "life-cycle effect" in which borrowers establish their reputation with relationship banks before ultimately graduating to arm's length public markets. Commercial paper issuers therefore tend to be older and less opaque and are therefore less likely to be reliant on "relationship" lenders. As a result, these firms are also likely to benefit from the effects of increased competition for their business.

Without disaggregating the separate effects of characteristics correlated with CP issuance, Columns (I) and (II) of Table IX replicate the specification in Table VIII for borrowers which had short term rating equal to or better than A-2 in the year prior to loan contracting (CP issuers) and those which did not (Non-CP issuers). The evidence suggests that borrowers without access to CP as an alternative source of financing find that their contracts are considerably more dependent on time-varying lender attributes. The coefficient on recent defaults is 0.78 smaller for borrowers which have access to commercial paper issuance and is not distinct from zero. The difference is significant at the 5% level, in spite of the small sample of CP issuers.[22]

Taken together, the final tables suggest that perhaps lenders are less likely to adjust their contracts for borrowers with good outside options. Instead, evidence of lenders tightening contracts in response to their own defaults may be 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.

IV. 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 show that defaults on recently originated loans are more informative than older "legacy" loans held by banks, 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 the supply-side effects observed in covenants. Borrowers who are most dependent on the relationship aspect of the bank market are also most prone to receive stricter contracts from affected lenders.

Notes

[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.

[3]Bradley and Roberts (2004) find evidence of a trade-off between covenants and interest rates.

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

[5]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, $r_n - \underline{r}_n$. 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 $\mathbf{r} - \underline{\mathrm{r}}$.

[6]Indeed, the Doornik-Hansen test of joint normality for the Compustat sample of levered firms rejects the null hypothesis that the accounting ratios used in covenants are drawn from a perfectly multivariate normal distribution. However, given the size of the Compustat sample, such a test will be successful in detecting even minor deviations from a normal density.

[7]Note, Σ is estimated based on changes in and not levels of ratios, consistent with the stylized presentation in equation 3.

[8]For covenants which include measures of cash-flow or income, these are calculated on a rolling four-quarter basis. See appendix A for more details on variable construction.

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

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

[11]Borrower controls such as fixed-charge coverage and current ratio are chosen as controls as opposed to alternative accounting measures to most closely match the variables which banks are contracting on. The four ratios chosen are the most typical size, leverage, cash-flow, and liquidity ratios used in the loan contracts I observe, respectively.

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

[13]As an alternative, if firms only accept covenants with which they can easily comply (Demiroglu and James (2010)) or accounting ratios are sufficiently manipulable (Dichev and Skinner (2002)) the relationship between contract strictness and ex-post violations may be weak.

[14]For the subsample of *DealScan* loans reporting both mandate and closing dates, the timing is only

slightly longer, with a mean (median) time in market of 89 (63) days.

[15]The specification implicitly assumes a symmetric impact of defaults on covenants. Regressions which split the default count into two variables (tabulations available in the Internet Appendix to this paper)– one for when the lenders experiences above average default counts and one for when it experiences a below average default count– confirm that lenders both tighten as defaults increase and loosen as defaults decrease.

[16]For these reasons, both AIC and BIC model selection criteria favor specifications with year dummies.

[17]To see this, consider that the probability of a violation occurring at or prior to time T is $\sum_{i \in T} p(1-p)^t$, where p represents the quarterly probability of a violation.

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

[19]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.

[20]An extensive practitioner's guide to the design and implementation of the credit review process is available in Brian Ranson's Credit Risk Management (1995). Ranson recommends the credit committee constantly revisit past loan decisions, and even individual voting records, in light of ex-post loan outcomes in order to obtain a "frank appraisal of the properties of the committee".

[21]Faulkender and Petersen (2006) use the presence of a commercial paper and/or bond rating to capture access to external debt markets. However, sorting only on commercial paper ratings is of practical value in that it allows me to continue to include long-term ratings dummies as controls for borrower risk in both specifications, whereas sorting on the presence of a long-term debt rating forces me to exclude those controls from one or both groups.

[22]The evidence that bank capital effects vary across the subsamples in Tables VIII and IX is mixed. The effect of changes in capital is larger, but not significantly so when comparing non-CP issuers to issuers, whereas the effects of defaults and capital are jointly significantly different across subsamples. In Table VIII, however, coefficients on changes in lender capitalization reported are nearly identical across the subsamples, although when capitalization is measured in levels, as in Column (I) of Table V, the effects are larger in magnitude for tight-knit bank groups.

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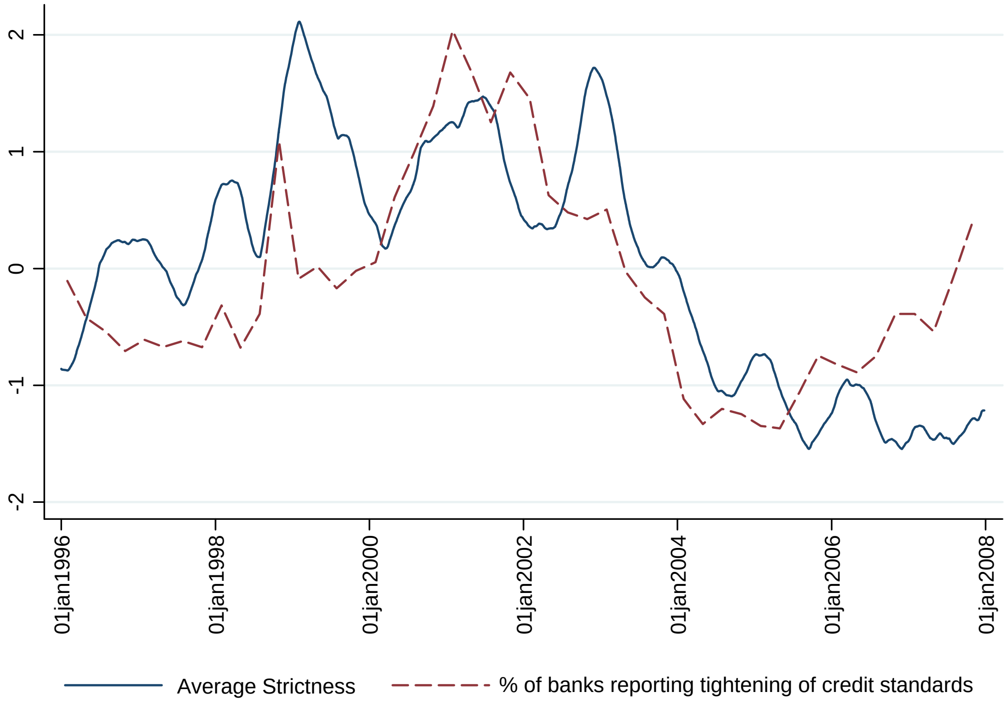

Figure 1: Average contract strictness over time, plotted against the Federal Reserve survey of senior loan officers, percentage of respondents reporting tightening credit standards. The moving average is calculated using a tent shaped kernel over 180 day bandwidth, such that $\overline{STRICTNESS}_t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

Table I: 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 measure described in the methodology section, ranging 0-1), 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. Industry dummies are calculated at the one-digit SIC level. Ratings dummies are based on S&P long term debt ratings (no rating is the base category). Covenant controls include the borrowers debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are defined in Appendix A. Results are reported in terms of marginal effects evaluated at the mean of each variable. 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	0.15***			0.22***	0.15***	0.20***
	(0.05)			(0.07)	(0.05)	(0.08)
Number of Financial Covenants		0.01			0.00	0.02
		(0.01)			(0.01)	(0.01)
Slack Net Worth Covenant			-0.31**	-0.11		-0.13
			(0.14)	(0.15)		(0.15)
ln(Maturity)	0.12***	0.12***	0.16***	0.15***	0.12***	0.15***
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
ln(Amount)	-0.03**	-0.03**	-0.02	-0.02	-0.03**	-0.02
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
Secured	0.05***	0.06***	0.07**	0.06**	0.05***	0.06**
	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
ln(# of participants)	0.00	0.01	0.00	0.00	0.00	-0.00
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)
Borrower Z-score	-0.01**	-0.01***	-0.01**	-0.01**	-0.01**	-0.01**
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.01)
Observations	2,548	2,552	1,283	1,280	2,548	1,280
Log likelihood	-1203	-1211	-640	-634	-1203	-633
Covenant Controls	YES	YES	YES	YES	YES	YES
Ratings Dummies	YES	YES	YES	YES	YES	YES
Year Dummies	YES	YES	YES	YES	YES	YES
Industry Dummies	YES	YES	YES	YES	YES	YES

Table II: 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					
	N	Mean	SD	10th	50th	90th
Firm Characteristics						
Total Assets ($M)	22,020	3,515.27	11,993.31	51.71	599.86	8,268.60
EBITDA/Assets	18,305	0.12	0.13	0.03	0.12	0.23
Market Value of Equity/Book Liabilities	20,091	3.07	10.77	0.28	1.28	6.04
Has S&P long-term debt rating	22,789	0.43	0.50	-	-	1.00
S&P long-term debt rating	9,813	12.87	3.67	8.00	13.00	17.00
Altman Z-score	15,055	3.62	4.67	0.97	2.83	6.55
Loan Characteristics						
Maturity (months)	20,942	49.06	289.52	12.00	42.00	82.00
Amount ($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					
	N	Mean	SD	10th	50th	90th
Firm Characteristics						
Total Assets ($M)	2,642	3,103.93	8,158.32	105.96	818.30	6,404.80
EBITDA/Assets	2,642	0.15	0.07	0.07	0.14	0.24
Market Value of Equity/Book Liabilities	2,583	1.20	1.10	0.36	0.89	2.29
Has S&P long-term debt rating	2,642	0.49	0.50	-	-	1.00
S&P long-term debt rating	1,285	12.34	2.73	9.00	12.00	16.00
Altman Z-score	2,474	3.92	2.88	1.66	3.38	6.49

Table II: Summary Statistics and Sample Selection (cont)

	DealScan-Compustat Covenant Sample					
	N	Mean	SD	10th	50th	90th
Loan Characteristics						
Contract Strictness	2,642	22.51	20.30	0.002	17.47	52.01
Maturity (months)	2,623	47.64	20.53	12.00	57.00	62.00
Amount ($M)	2,642	411.35	773.46	27.50	200.00	950.00
Secured	2,642	0.51	0.50	-	1.00	1.00
No. of participants	2,642	9.25	8.83	1.00	7.00	20.00
No. of lead arrangers	2,642	1.23	0.43	1.00	1.00	2.00
Bank Characteristics						
Lender Total Assets ($BN)	2,504	589.79	475.89	83.58	450.56	1,337.91
Lender capitalization	2,481	7.51%	1.80%	5.29%	7.77%	9.31%
Defaults on lender portfolio-past 90 days	2,462	1.51	2.42	-	-	5.00
% Loans Arranged by Top 3 banks		37.52%				
% Loans Arranged by Top 5 banks		48.52%				

Table III: Contract Strictness and Recent Defaults. Panels A and B present borrower fixed-effects regressions of loan strictness as described in the methodology section (ranging 0-100), on the number of defaults in the 90-days prior to contracting and controls. Defaults on the lender's portfolio are calculated as the number of outstanding DealScan loan packages in which the lead arranger participated and for which the borrower's rating was changed to *Default* by the S&P ratings database during the period of interest. Covenant controls include the borrowers debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are defined in Appendix A. Standard errors are clustered by borrower and by lender, are 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	0.12**				
	(0.05)				
Defaults on lender portfolio- past 90 days		0.38**	0.38**	0.36**	0.39**
		(0.18)	(0.18)	(0.18)	(0.17)
Defaults on lender portfolio- 90-180 days		0.13	0.14	0.12	
		(0.14)	(0.15)	(0.14)	
Defaults on lender portfolio- 180-270 days		-0.09	-0.08		
		(0.15)	(0.15)		
Defaults on lender portfolio- 270-360 days		0.07			
		(0.15)			
ln(Maturity)	-0.88	-0.88	-0.89	-0.89	-0.89
	(0.92)	(0.93)	(0.92)	(0.92)	(0.92)
ln(Amount)	2.32**	2.38**	2.39**	2.37**	2.35**
	(1.05)	(1.04)	(1.04)	(1.04)	(1.05)
Secured	0.82	0.89	0.89	0.87	0.85
	(1.87)	(1.87)	(1.87)	(1.87)	(1.86)
ln(# of participants)	1.28	1.30	1.27	1.28	1.29
	(0.86)	(0.87)	(0.87)	(0.86)	(0.86)
Borrower Z-score	-1.39***	-1.39***	-1.39***	-1.39***	-1.40***
	(0.48)	(0.48)	(0.48)	(0.48)	(0.48)
Observations	2,289	2,289	2,289	2,289	2,289
R-squared (partial, excluding unreported fixed effects)	0.160	0.162	0.162	0.161	0.161
Ratings Dummies	YES	YES	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES	YES	YES
Covenant Controls	YES	YES	YES	YES	YES
Loan Year Dummies	YES	YES	YES	YES	YES
Loan Type Dummies	YES	YES	YES	YES	YES

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

	Panel B			
Loan Strictness	I	II	III	IV
Defaults on lender portfolio- past 90 days	0.34**	0.32*	0.33**	0.35**
	(0.17)	(0.17)	(0.17)	(0.16)
ln(Maturity)	-0.87	-0.92	-0.86	-0.89
	(0.98)	(0.97)	(0.98)	(0.97)
ln(Amount)	1.71	1.74*	1.71	1.75*
	(1.07)	(1.05)	(1.05)	(1.05)
Secured	1.34	1.39	1.35	1.36
	(1.87)	(1.85)	(1.89)	(1.86)
ln(# of participants)	1.52*	1.53*	1.51*	1.48*
	(0.87)	(0.87)	(0.87)	(0.87)
Borrower Z-score	-1.45***	-1.44***	-1.45***	-1.45***
	(0.48)	(0.48)	(0.48)	(0.48)
Aggregate defaults - past 90 days	0.29***	0.31***	0.29***	0.29***
	(0.06)	(0.07)	(0.06)	(0.06)
Baa-Aaa credit spreads		-1.40		
		(2.49)		
S&P 500 return - past 90 days			1.03	
			(7.15)	
Quarterly GDP growth				39.46
				(59.19)
Observations	2,289	2,289	2,289	2,289
R-squared (partial, excluding unreported fixed effects)	0.138	0.139	0.138	0.138
Ratings Dummies	YES	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES	YES
Covenant Controls	YES	YES	YES	YES
Loan Year Dummies	NO	NO	NO	NO
Loan Type Dummies	YES	YES	YES	YES

Table IV: The Effects of Geographically and Industrially Distinct Defaults. Table IV presents borrower fixed-effects regressions. Recent default counts in columns I, II and III exclude defaults in the same state (or country for non-US borrowers) as the contracting borrower, the same 1-digit SIC code , or both, respectively. Covenant controls include the borrowers debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are as defined in Table III. Standard errors are clustered by borrower and by lender, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

Loan Strictness	Different State/Country I	Different Industry II	Different Industry & State/Country III
Defaults on lender portfolio- past 90 days	0.42**	0.47***	0.49***
	(0.18)	(0.17)	(0.18)
ln(Maturity)	-0.95	-0.97	-0.98
	(0.93)	(0.93)	(0.93)
ln(Amount)	2.39**	2.42**	2.36**
	(1.06)	(1.04)	(1.05)
Secured	0.83	0.88	0.81
	(1.85)	(1.89)	(1.88)
ln(# of participants)	1.21	1.19	1.23
	(0.86)	(0.84)	(0.85)
Borrower Z-score	-1.40***	-1.40***	-1.41***
	(0.48)	(0.48)	(0.48)
Observations	2,275	2,275	2,275
R-squared (partial, excluding unreported fixed effects)	0.169	0.170	0.172
Ratings Dummies	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES
Covenant Controls	YES	YES	YES
Loan Year Dummies	YES	YES	YES
Loan Type Dummies	YES	YES	YES

Table V: Capital Effects and Recent Defaults. Table V presents borrower fixed-effects regressions. The dependent variable is loan strictness, as described in the methodology section. Lender capitalization is the ratio of shareholder equity to total assets held by the lender in the quarter in which the the loan is originated. Covenant controls include the borrowers debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are as defined in Table III. Standard errors are clustered by borrower and by lender, are 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
Defaults on lender portfolio- past 90 days	0.44**	0.43**	0.41**
	(0.19)	(0.18)	(0.18)
Lender capitalization (t)	-0.40*		
	(0.22)		
ΔLender capitalization (t+1)			0.46
			(0.40)
ΔLender capitalization (t)		-1.18***	-1.21***
		(0.24)	(0.28)
ΔLender capitalization (t-1)			-0.62
			(0.41)
ln(Maturity)	-1.00	-0.98	-1.10
	(0.93)	(0.96)	(0.95)
ln(Amount)	2.23**	2.34**	2.48**
	(1.13)	(1.14)	(1.18)
Secured	0.03	0.13	0.15
	(1.93)	(1.93)	(1.98)
ln(# of participants)	1.28	1.26	1.10
	(0.88)	(0.88)	(0.89)
Borrower Z-score	-1.81***	-1.82***	-1.84***
	(0.65)	(0.65)	(0.65)
Observations	2,059	2,059	2,022
R-squared (partial, excluding unreported fixed effects)	0.178	0.181	0.186
Ratings Dummies	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES
Covenant Controls	YES	YES	YES
Loan Year Dummies	YES	YES	YES
Loan Type Dummies	YES	YES	YES

Table VI: Contract Strictness and Legacy Defaults. Table VI presents borrower fixed-effects regressions. Defaults are sorted based on the whether the defaulting loans were originated in the year prior to, between one and two years prior to, two to five years prior to, or more than 5 years prior to the current contract. Column V tests the differential effects of defaults on recently originated loans (<1 year) and defaults on "legacy loans" (>five years), each comprising roughly 10% of all defaults. Covenant controls include the borrowers debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are as defined in Table III. Standard errors are clustered by borrower and by lender, are 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
(i)	Lender defaults (loans<1 year old)	1.85***				1.85***
		(0.63)				(0.63)
(ii)	Lender defaults (1 year old<loans<2 years old)		0.51			
			(0.41)			
(iii)	Lender defaults (2 years old<loans<5 years old)			0.49*		
				(0.26)		
(iv)	Lender defaults (loans>5 years old)				0.19	0.21
					(0.53)	(0.55)
	(i)-(iv)					1.64*
	ΔLender capitalization	-1.26***	-1.24***	-1.15***	-1.21***	-1.25***
		(0.26)	(0.25)	(0.24)	(0.25)	(0.25)
	ln(Maturity)	-0.88	-0.96	-0.94	-0.95	-0.90
		(0.95)	(0.95)	(0.96)	(0.98)	(0.97)
	ln(Amount)	2.20*	2.37**	2.30**	2.33**	2.21*
		(1.13)	(1.14)	(1.14)	(1.16)	(1.14)
	Secured	0.08	0.04	0.15	0.09	0.09
		(1.96)	(1.93)	(1.92)	(1.92)	(1.95)
	ln(# of participants)	1.30	1.21	1.25	1.20	1.29
		(0.86)	(0.87)	(0.88)	(0.87)	(0.86)
	Borrower Z-score	-1.77***	-1.79***	-1.81***	-1.78***	-1.78***
		(0.66)	(0.67)	(0.65)	(0.67)	(0.66)
	Observations	2,059	2,059	2,059	2,059	2,059
	R-squared (partial, excluding unreported fixed effects)	0.182	0.179	0.180	0.178	0.182
	Ratings Dummies	YES	YES	YES	YES	YES
	Borrower Fixed Effects	YES	YES	YES	YES	YES
	Covenant Controls	YES	YES	YES	YES	YES
	Loan Year Dummies	YES	YES	YES	YES	YES
	Loan Type Dummies	YES	YES	YES	YES	YES

Table VII: Effects of defaults on internal and external reputation. Table VII presents borrower fixed-effects regressions. The interaction of interest is 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". Covenant controls include the borrowers debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are as defined in Table III. Standard errors are clustered by borrower and by lender, are 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	0.42**
	(0.18)
Defaults on lender portfolio- past 90 days X Bilateral	0.92
	(1.06)
Bilateral	-4.49
	(4.13)
ΔLender capitalization	-1.16***
	(0.25)
ln(Maturity)	-1.01
	(0.95)
ln(Amount)	2.30**
	(1.13)
Secured	0.20
	(1.92)
ln(# of participants)	1.21
	(0.87)
Borrower Z-score	-1.82***
	(0.65)
Observations	2,059
R-squared (partial, excluding unreported fixed effects)	0.183
Ratings Dummies	YES
Borrower Fixed Effects	YES
Covenant Controls	YES
Loan Year Dummies	YES
Loan Type Dummies	YES

Table VIII: Contract Sensitivity and Lender Relationships. Table VIII presents borrower fixed-effects regressions. 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. For borrowers with 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. Covenant controls include the borrowers debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are as defined in Table III. Standard errors are clustered by borrower and by lender, are 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	0.97***	0.08	0.89***
	(0.27)	(0.21)	
ΔLender capitalization	-1.20*	-1.30***	
	(0.61)	(0.36)	
ln(Maturity)	-1.74	0.27	
	(1.44)	(0.81)	
ln(Amount)	2.00	-0.01	
	(1.50)	(1.42)	
Secured	1.98	0.63	
	(2.65)	(2.09)	
ln(# of participants)	1.91	0.79	
	(1.41)	(1.28)	
Borrower Z-score	-1.48**	-4.62***	
	(0.66)	(0.84)	
Observations	926	864	
R-squared (partial, excluding unreported fixed effects)	0.233	0.320	
Ratings Dummies	YES	YES	
Borrower Fixed Effects	YES	YES	
Covenant Controls	YES	YES	
Loan Year Dummies	YES	YES	
Loan Type Dummies	YES	YES	

Table IX: Contract Sensitivity and Alternative Financing. Table IX presents borrower fixed-effects regressions. Column II 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 I examines borrowers without short-term ratings and those rated weaker than A-2. Covenant controls include the borrower's debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are as defined in Table III. Standard errors are clustered by borrower and by lender, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

Loan Strictness	Non-CP Issuer I	CP Issuer II	I-II
Defaults on lender portfolio- past 90 days	0.50**	-0.28	0.78**
	(0.25)	(0.22)	
ΔLender capitalization	-1.41***	-0.83***	
	(0.36)	(0.31)	
ln(Maturity)	-0.50	0.00	
	(1.23)	(1.03)	
ln(Amount)	2.80**	3.14**	
	(1.37)	(1.44)	
Secured	-0.25	1.96	
	(1.95)	(3.05)	
ln(# of participants)	1.50	-1.77	
	(1.04)	(1.58)	
Borrower Z-score	-1.87***	0.49	
	(0.71)	(0.83)	
Observations	1,674	366	
R-squared (partial, excluding unreported fixed effects)	0.175	0.526	
Ratings Dummies	YES	YES	
Borrower Fixed Effects	YES	YES	
Covenant Controls	YES	YES	
Loan Year Dummies	YES	YES	
Loan Type Dummies	YES	YES	

Appendix

Amount = The total amount of the loan package

Capital Expenditure = Sum of rolling four quarter capital expenditures

Contracting date = The date which is 90 days prior to the facility start date defined in DealScan

Credit Rating = S&P senior long-term debt rating

Credit Spread = Yield spread between Baa and Aaa Moody's rated corporate bonds

Current Ratio = Total current assets / total current liabilities

Debt/Equity = (Long term debt + debt in current liabilities) / shareholder equity

Debt/Tangible Net Worth=(Long term debt + debt in current liabilities) / (total assets-total liabilities-intangible assets)

Defaults on lender portfolio = The number of outstanding DealScan loan packages in which the lead arranger participated and for which the borrower's rating was changed to *Default* by the S&P ratings database during the period of interest

EBITDA = Sum of rolling four quarter operating income before depreciation

EBITDA/Debt = (Sum of rolling four quarter operating income before depreciation) /(long term debt + debt in current liabilities)

Fixed Charge Coverage = (Sum of rolling four quarter operating income before depreciation) / (sum of rolling four quarter interest expenses + debt in current liabilities one year prior)

Lender Capitalization = Lender shareholder equity /total assets

Loan Type = Indicator variables for the following categories reported in DealScan: corporate purposes, debt repayment, working capital, takeover, CP backup, or other

Maturity = The maximum stated maturity of a package in months

Interest Coverage = (Sum of rolling four quarter operating income before depreciation) / (sum of rolling four quarter interest expenses)

Quick Ratio = (Total current assets - inventories) / total current liabilities

S&P 500 Returns = 90 day holding period return for the S&P 500 index

Tangible Net Worth = Total assets-total liabilities-intangible assets

Z-Score= 3.3 Pre-tax operating income / total assets + sales / total assets+ 1.4 retained earnings/ total assets + 1.2(current assets - current liabilities)/total assets+.6 market value of equity / total liabilities

Internet Appendix: The Supply-Side Determinants of Loan Contract Strictness*

Table AI-AIII provide additional summary statistics regarding the measure of covenant strictness and default frequencies, as well as several alternative specifications for the main results of the paper. Table AI begins by detailing the distribution of contract strictness by firm rating. Consistent with much of the prior literature linking tighter covenants to riskier firms, highly rated borrowers receive much looser contracts than poorly rated borrowers. Unrated borrowers receive contracts roughly equivalent to those of firms rated between the investment grade and junk cutoff.

Table AI also documents the average strictness by year to supplement Figure 1, for which values were smoothed via a moving average filter and then standardized to compare with the senior loan officer survey. Consistent with Figure 1, contract strictness peaked in 1999 and 2001 before a decline in lending standards in 2003 through 2007. This lines up intuitively with default counts by year, also reported in AI. As described in main text, defaults are reported by Standard and Poor's and only include borrowers in emphCompustat and emphDealScan.

Table AII considers a number of alternative specifications to those in Table III and IV in the main paper. Column 1 allows for lender fixed effects, where lenders which acted as lead arrangers in less than 50 loans are aggregated into a single group. Column 2 repeats the lender fixed effects regression, eliminating the top three banks in the sample (37% of all transactions). Meanwhile columns 3 through 6 replace annual time effects with quarterly time effects. The results are quantitatively similar to those of specifications for which only annual dummies are included.

Finally, column 7 of Table AII re-estimates the paper's main result replacing the proposed measure of contract strictness with the slack of the net worth covenant, as defined in Table I of the main paper. The effect of defaults on the slackness of the net worth covenant is negative (more defaults begets tighter covenants). However, the number of observations declines as many firms do not receive a net worth covenant.

Table AIII documents the symmetry of lenders' response to default news, using both the prior year's and prior quarter's default count. In each case, Ive replaced the default count variable with two variablesone which captures a relatively low number of defaults (those

*Citation format: Murfin, Justin, [year], Internet Appendix to The Supply Side Determinants of Loan Contract Strictness, Journal of Finance [vol ?], [pages], http://www.afajof.org/IA/[year].asp. Please note: Wiley-Blackwell is not responsible for the content or functionality of any supporting information supplied by the authors. Any queries (other than missing material) should be directed to the authors of the article.

default counts which are below the lenders average) and one which captures a relatively high number of defaults (those default counts which are above the lenders average). For the variable capturing a relatively high number of defaults, it is zero when the default count is smaller than the lenders average. Otherwise it is the observed default count (a variable for below average defaults is similarly constructed). Note, Ive also standardized both variables so that they are mean zero, with a standard deviation of zero. Otherwise, the right skewed distribution of defaults muddles the interpretation. The coefficients for both worse-than-average and better-than-average default outcomes are surprisingly close (and not statistically distinct). Included in the same specification, neither variable is significant by itself, although a test of joint significance rejects the null that both coefficients are zero. In both columns 3 and 6 the coefficients are jointly significant at the 10% level.

Table AI: Additional Summary Statistics

Strictness by S&P LT Debt Rating

	Unrated	AAA-AA(+/-)	A(+/-)	BBB(+/-)	BB(+/-)	B(+/-)	CCC(+/-)
Mean	22.90	0.64	7.17	18.16	29.91	30.99	36.38
N	1357	15	175	497	425	168	5
S.D.	19.90	1.91	12.25	17.77	20.80	22.86	29.52

Strictness by Year

	1994	1995	1996	1997	1998	1999	2000
Mean	15.73	23.29	20.83	23.01	23.71	29.89	23.94
N	25	67	174	236	142	190	216
S.D.	16.17	23.07	18.79	19.95	19.02	22.31	21.42

	2001	2002	2003	2004	2005	2006	2007
Mean	27.95	23.28	25.59	19.95	18.54	17.29	17.13
N	218	212	254	276	254	189	140
S.D.	22.09	20.02	20.66	19.28	19.08	17.92	17.77

DealScan Borrower Defaults by Year

1994	1995	1996	1997	1998	1999	2000
1	10	2	1	1	40	49

2001	2002	2003	2004	2005	2006	2007
75	52	31	15	11	7	4

Table AII. Alternative Specifications I. Table AII presents borrower fixed-effects regressions with bank fixed effects and quarterly time dummies. Bank fixed effects regressions in 1 and 2 group lenders with less than 50 observations under a single dummy. Column 2 excludes transactions led by the top 3 banks by number of transactions over the sample. Columns 3-6 re-estimate the main tables in the paper with quarterly fixed effects. Recent default counts in columns 4, 5 and 6 exclude defaults in the same state (or country for non-US borrowers) as the contracting borrower, the same 1-digit SIC code , or both, respectively. Finally, column 7 (next page) re-estimates the paper's main result replacing the proposed measure of contract strictness with the slack of the Net Worth covenant, as defined in Table I of the main paper. In each column, covenant controls include the borrower's debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are as defined in Table III of the paper. Standard errors are clustered by borrower and by lender, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

	Bank Fixed Effects		Quarterly Fixed Effects			
		Excl. top 3 banks	All Defaults	Different State/Country	Different Industry	Different Industry & State/Country
Loan Strictness	1	2	3	4	5	6
Defaults on lender portfolio- past 90 days	0.43***	0.77***	0.38**	0.40**	0.46***	0.48**
	(0.14)	(0.20)	(0.18)	(0.19)	(0.18)	(0.19)
ln(Maturity)	-0.82	1.61**	-0.74	-0.82	-0.85	-0.84
	(0.87)	(0.77)	(0.93)	(0.94)	(0.94)	(0.94)
ln(Amount)	2.28**	1.92	2.14**	2.17**	2.20**	2.14**
	(0.97)	(1.28)	(1.00)	(1.01)	(1.01)	(1.00)
Secured	0.62	0.55	0.96	1.00	1.04	1.00
	(2.00)	(1.79)	(1.86)	(1.86)	(1.88)	(1.88)
ln(# of participants)	1.22*	0.83	1.34	1.26	1.24	1.27
	(0.68)	(0.90)	(0.87)	(0.87)	(0.86)	(0.86)
Borrower Z-score	-1.39***	-1.80**	-1.38***	-1.40***	-1.39***	-1.40***
	(0.37)	(0.71)	(0.43)	(0.43)	(0.42)	(0.43)
Observations	2,289	942	2,289	2,275	2,275	2,275
R-squared (partial, excluding unreported fixed effects)	0.17	0.22	0.19	0.20	0.20	0.20
Ratings Dummies	YES	YES	YES	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES	YES	YES	YES
Bank Fixed Effects	YES	YES	NO	NO	NO	NO
Covenant Controls	YES	YES	YES	YES	YES	YES
Loan Year Dummies	YES	YES	NO	NO	NO	NO
Loan Quarter Dummies	NO	NO	YES	YES	YES	YES
Loan Type Dummies	YES	YES	YES	YES	YES	YES

Table AII: Alternative Specifications (cont).

Slack Net Worth Covenant*100	7
Defaults on lender portfolio- past 360 days	-0.08**
	(0.04)
ln(Maturity)	-1.01
	(0.70)
ln(Amount)	0.13
	(0.72)
Secured	-0.42
	(1.05)
ln(# of participants)	-0.72
	(0.85)
Borrower Z-score	0.93*
	(0.48)
Debt/Tangible Net Worth	0.48***
	(0.15)
Fixed Charge Coverage	-0.00
	(0.04)
Current Ratio	1.86***
	(0.70)
ln(Tangible Net Worth)	5.81***
	(1.36)
Observations	914
R-squared (partial, excluding unreported fixed effects)	0.28
Ratings Dummies	YES
Borrower Fixed Effects	YES
Covenant Controls	YES
Loan Year Dummies	YES
Loan Type Dummies	YES

Table AIII: Symmetry of Response. Table AIII considers the symmetry of response to lender defaults. Annual and quarterly lender defaults prior to contracting are separated into above average and below average default counts. When default counts are above the lender's average count, the above average variable reflects the default count, otherwise it is zero. Below average default counts are constructed similarly. Both are standardized so the coefficient magnitude reflects the impact of a one standard deviation change in defaults. Column 7 excludes defaults in the same state (or country for non-US borrowers)/1-digit SIC code as the contracting borrower. Covenant controls include the borrower's debt/tangible net worth, fixed charge coverage, current ratio, and ln(tangible net worth). Other variables are as defined in Table III of the paper. Standard errors are clustered by borrower and by lender, are robust to heteroskedasticity, and are reported in parentheses. ***, **, and * signify results significant at the 1, 5, and 10% levels, respectively.

							Different Industry & State/Country
Loan Strictness	1	2	3	4	5	6	7
Defaults on lender portfolio- past 360 days (above ave. #)	0.59		0.49†				
	(0.39)		(0.42)				
Defaults on lender portfolio- past 360 days (below ave. #)		0.61	0.51†				
		(0.43)	(0.47)				
Defaults on lender portfolio- past 90 days (above ave. #)				0.72**		0.58†	0.60*
				(0.36)		(0.39)	(0.36)
Defaults on lender portfolio- past 90 days (below ave. #)					0.67*	0.52†	0.58*
					(0.39)	(0.42)	(0.33)
ln(Maturity)	-0.87	-0.87	-0.88	-0.89	-0.86	-0.88	-0.92
	(0.92)	(0.92)	(0.92)	(0.92)	(0.92)	(0.92)	(0.91)
ln(Amount)	2.32**	2.34**	2.32**	2.35**	2.33**	2.35**	2.26**
	(1.05)	(1.05)	(1.05)	(1.05)	(1.05)	(1.04)	(1.04)
Secured	0.81	0.83	0.82	0.83	0.85	0.85	0.76
	(1.88)	(1.85)	(1.86)	(1.86)	(1.86)	(1.86)	(1.89)
ln(# of participants)	1.27	1.26	1.29	1.26	1.30	1.31	1.36
	(0.86)	(0.86)	(0.86)	(0.86)	(0.86)	(0.86)	(0.84)
Borrower Z-score	-1.39***	-1.37***	-1.39***	-1.40***	-1.36***	-1.39***	-1.40***
	(0.48)	(0.49)	(0.48)	(0.48)	(0.49)	(0.48)	(0.48)
Observations	2,289	2,289	2,289	2,289	2,289	2,289	2,275
R-squared (partial, excluding unreported fixed effects)	0.16	0.16	0.16	0.16	0.16	0.16	0.16
Ratings Dummies	YES	YES	YES	YES	YES	YES	YES
Borrower Fixed Effects	YES	YES	YES	YES	YES	YES	YES
Loan Year Dummies	YES	YES	YES	YES	YES	YES	YES
Loan Type Dummies	YES	YES	YES	YES	YES	YES	YES
Covenant Controls	YES	YES	YES	YES	YES	YES	YES

† Implies lender above average and below average defaults are jointly significant at the 10% level.