Friends with Money*

Joseph Engelberg

Pengjie Gao

Christopher A. Parsons‡

This Draft: April 2010
First Draft: December 2009

Abstract: When banks and firms are connected through interpersonal linkages – such as their respective management having attended college or previously worked together – interest rates are markedly reduced, comparable with single shifts in credit ratings. We find no evidence that such rate concessions reflect "sweetheart deals." Subsequent firm performance, such as future credit ratings or stock returns, improves following a connected deal, suggesting that social networks lead to either better information flow or better monitoring.

* We have benefited from helpful discussions with Aydoğan Altı, Dan Hamermesh, Jay Hartzell, Tim Loughran, Paul Schultz, Jeremy Stein, Sheridan Titman, and seminar participants at the University of Houston and University of Washington. We wish to thank Jacqueline Higgins and Shoshana Zysberg at Management Diagnostic Limited for assistance with the BoardEx database, and Jing Zhang at Moody's-KMV for assistance with the expected default frequencies (EDF®) and EDF implied spreads (EIS®) database. Xian Cai and Mei Zhao provided superb research assistance.

‡ Joseph Engelberg, Kenan-Flagler Business School, University of North Carolina at Chapel Hill, (Email) joseph_engelberg@kenan-flagler.unc.edu, (Tel) 919-962-6889; Pengjie Gao, Mendoza College of Business, University of Notre Dame, (Email) pgao@nd.edu, (Tel) 574-631-8048; and Christopher Parsons, Kenan-Flagler Business School, University of North Carolina at Chapel Hill, (Email) chris_parsons@kenan-flagler.unc.edu, (Tel) 919-962-4132.

I. Introduction

Stein (2003) characterizes information and agency problems as the "most pervasive and important" violations of Modigliani and Miller's (1958) perfect capital market assumptions. Because reliance on external finance depends to a large extent on these frictions, technologies that ameliorate their effects have important implications for a firm's financing cost, capital structure and investment policy. In this paper, we study whether *personal relationships* between the respective employees of borrowers and lenders represent such a mechanism.

The expected effect of personal relationships in credit markets is not obvious. On one hand, a lender personally beholden to a borrower may overlook its flaws, thereby putting his or her own shareholders' capital at undue risk. On the other hand, such relationships may catalyze information flow or reduce monitoring costs, placing the connected bank at an advantage relative to competing lenders. Here, both parties stand to benefit – banks make better lending decisions, and assuming the associated surplus is shared, firms lower their costs of capital.

The goals of this paper are twofold. First, we aim to establish a causal link between borrower-lender personal relationships and lending market outcomes. Second, we explore whether such relationships lead banks to make choices that harm their own shareholders, or whether they improve their capital allocation decisions.

To address these questions, we assemble a dataset of roughly 20,000 commercial loans made to U.S. companies between 2000 and 2007. The set of borrowers involves over 5,000 public firms, and the set of lenders over 1,900 commercial banks. Next, we obtain a list of common organizations where each of the 65,000 unique directors and executives in our universe of firms and banks may have fostered personal relationships. This tells us, for instance, if the President of Wachovia Bank and the Chief Executive Office of Pepsi Co. attended college together, or if they overlapped in their first job after graduate school. The main question: Do personal relationships such as these influence lending terms?

Establishing a causal relation requires a careful account of the endogeneity of personal relationships. A serious concern is reverse causality, whereby lending interactions lead to the formation of social relationships. As an illustration, suppose a banker provides financing to a firm at below market rates, and is subsequently invited to join the board of the CEO's favorite charity, or perhaps even the board of the borrowing firm itself. Such an example is typical of several that could potentially generate correlation between lending terms and firm-bank personal relationships, but not for causal reasons.

Perhaps the most significant advantage of our data is that they allow us to infer connections whose formation predates, by several years or decades, the lending relationships we analyze. If Pepsi borrows from a Wachovia-led syndicate in 2004, we take as exogenous that their respective top executives may have both received MBAs from Stanford in 1974, or both worked for Xerox in 1982. Such a long lag between relationship formation and lending transactions poses an insurmountable obstacle to reverse causality, and nearly as big a challenge to omitted variable critiques.

In pooled cross-sectional regressions of interest rates charged by syndicates, we find that the presence of at least one pre-existing, personal relationship between the firm and lender – removed by at least five years relative to the date of the lending transaction – markedly reduces borrowing costs. For firms with very good credit (A or better), the effect is only 8 basis points (because spreads are bound at zero, the effect for highly rated firms cannot be large), steadily climbing as credit quality deteriorates. Firms with ratings in the BBB-B range can expect interest rate concessions of about 20 basis points; the magnitude more than doubles again for firms rated even worse, or that lack a rating altogether (45-50 bp). We expect the result to strengthen not only because default risk increases borrowing costs, but also because adverse selection and monitoring problems are most severe for these firms. In models controlling for a variety of firm, industry, loan, and macroeconomic characteristics (and even bank and firm fixed effects), we observe similar magnitudes, averaging between 15 and 20 basis points across all credit categories, or about 10 percent of the average charged spread. For comparison, the average spread between A and AA ratings is 16 basis points.

It is noteworthy that the effects we document are not simply a repackaging of the familiar result that lending terms can change when a firm and bank do repeated business with each other.[1] Our findings hold strongly for both a firm's historical banking partners, as well as for banks with which it has no prior lending experience. This finding underscores that in relationship banking, it appears to be the *human touch* that makes the difference, not necessarily familiarity with a firm's physical assets.

With regard to other lending terms, we find no evidence that creditors personally connected to their borrowers seek to protect themselves in other ways, such as loaning smaller amounts or using more covenants to restrict the firm's behavior. In fact, the opposite pattern emerges. With the same set of controls employed in the spread regressions (e.g., size and prior activity of syndicate banks, firm characteristics, macroeconomic controls, etc.), we find that personally connected syndicates lend somewhat *more* on average. Moreover, covenants are less likely to be required between personally connected firms and syndicate banks, and when they are used, are fewer in number.

The remainder of our analysis takes as given that firm-bank personal connections alter the terms of lending in the firm's favor, and asks whether these are good or bad decisions. Although the source of our banking data (Loan Pricing Corporation's *Dealscan*) does not provide data on specific loan performance, we gain insight by examining the evolution of each borrower's credit rating subsequent to initiating a bank deal. Although not specifically related to a given transaction, these summary statistics measure a firm's ability to meet its outstanding debt obligations, part of which includes the bank transactions we analyze. Furthermore, because credit ratings pertain to a firm's public debt, analyzing them represents a conservative

[1] See Peterson and Rajan (1994), Berger and Udell (1995), Degryse and Van Cayseele (2000), and Bharath, Dahiya, Saunders, and Srinivasan (2009).

way of measuring a firm's likelihood of defaulting on more senior claims, such as syndicated bank loans.

We consistently find that after borrowing, the credit ratings of personally connected firms improve compared to their un-connected counterpart borrowers. As a typical example, of the 1,290 BB-rated firms that initiate syndicated bank deals with at least one connected bank, 63% maintain the same credit rating in the years immediately following, 22% improve and 15% worsen. In contrast, the comparable distribution for the 1,880 BB-rated firms completing deals with unconnected banks is 64%, 11%, and 25%. Remarkably, such a pattern holds across *every* credit rating category (AAA, AA, A, etc.), as well as for alternative measures of risk (e.g., Moody's *Expected Default Frequencies*, Moody's *EDF Implied Spreads*).

Analysis of subsequent stock returns confirms that such improvements were not foreseen by the market. Pooled time-series cross-sectional regressions of characteristic risk-adjusted stock returns (following Daniel, Grinblatt, Titman, and Wermers (1997)) indicates one, two, and three-year excess returns of 3%, 10%, and 17%; in other words, firms completing deals to connected banks experience substantially higher stock returns than those borrowing from unconnected syndicates. Fama-MacBeth (1972) regressions indicate even stronger effects. A calendar-time portfolio approach, whereby we finance long positions in the stock of connected borrowers with short positions in unconnected ones, paints a similar picture, although much weaker statistically given the very short time period.

Together, the evidence from lending terms and *ex post* firm performance suggests an intuitive story – a firm's managers and directors have time varying, private information about future fundamentals, and personal relationships allow this information to be credibly conveyed to lenders.

Strictly interpreted, because personal relationships are among the strongest predictors of borrowing costs, our results are directly relevant for understanding cross-sectional differences in firms' costs of capital, and indirectly for capital structure and investment policy. We explore neither of the latter here, but because of the strong link between external financing and investment, note an immediate implication. More generally, the evidence identifies a specific technology that allows banks – and some more than others – to excel in problems situations, where a borrower's creditworthiness is difficult to evaluate or when active monitoring is required (Diamond (1984, 1991), Fama (1985)).

A number of papers, many in international contexts, have explored whether lending decisions improve or worsen when firms and banks are linked in some way that compromises the latter's objectivity. Generally, the evidence suggests that such situations lead to wealth transfers from lenders to borrowers, a perhaps unsurprising conclusion given the (often extraordinary) conflicts of interest imposed on the lending bank.[2] Our study is related to the extent that personal relationships also present an opportunity for a bank to have more intimate knowledge of a borrower; however, the lack of incentive conflicts is an important difference, and undoubtedly contributes to why we find such a positive effect of personal connections on lending decisions. Additionally, the exogeneity of relationship formation allows for a causal interpretation often made difficult in other settings.

Finally, our study also contributes to a growing literature that explores the impact of personal networks on business and investment decisions. See Cohen, Frazzini, and Malloy (2008) for evidence that personal connections enhance information flow among investment professionals, Schmidt (2008) for evidence that information about mergers travels across personal networks, and Fracassi (2008) for evidence that social relationships among executives and board members influence investment policy.

We organize the paper as follows. In the next section, we describe the lending and connections data, and outline our empirical strategies. We begin our formal analysis in Section III, where we explore the extent to which firm-bank connections influence lending terms

[2] Domestic studies include Krozner and Strahan (2001) and Güner, Malmendier and Tate (2008). Rajan and Zingales (1998) and Chutatong, Raja and Wiwattanakantang (2005), Morck and Nakamura (1999) and Hoshi, Kashyap and Sharfstein (1991), Laeven (2001), La Porta, Lopez-de-Silanes, and Zamarripa (2003) study connected lending in Asia, Japan, Russia, and Mexico respectively.

including interest rates, covenants, and loan amounts. Section IV is dedicated to answering the question of whether or not personal connections are associated with better or worse future firm performance. We consider robustness and some extensions to our basic results in Section V, and then conclude.

II. Data and identification strategy

Management Diagnostic Limited (MDL) is a data purveyor that collects biographical information on executives and board members of public companies. Its main product, BoardEx, reports work histories, educational backgrounds, and current participation in social organizations for CEOs, CFOs, other executives, and current directors. BoardEx has been used to examine the role of social networks in a variety of corporate finance settings (e.g., Schmidt (2008), Cohen, Frazzini and Malloy (2008) and Fracassi and Tate (2008)).

We supplement BoardEx with biographical information on personnel from a large number of public and private commercial banks, made generously available after a custom data request to MDL. The union of these data results in a universe of 5,057 public U.S. firms, 1,924 commercial banks, and 65,074 different individuals (either directors or executives at their respective institutions). From these we infer interpersonal linkages between bankers and borrowers, and test whether these linkages appear to facilitate information flow, or whether they adversely affect lender incentives.

Interpersonal relationships are endogenous, a recognition that plays an important role in how we construct our network variables. In particular, because we intend to explain corporate lending behavior with *pre-existing* personal connections between lenders and borrowers, it is crucial that we eliminate reverse causation, e.g., a commercial banker undercutting her competition by a few basis points, expecting to be rewarded with a seat on the borrower's board.

Instead, we wish to identify examples where social connections are plainly exogenous to the lending deals we analyze. Consequently, we focus on two specific types of connections that meet this criterion: 1) *school connections,* formed when two people graduate from the same educational institution within two years of one another (e.g., Stanford Class of 1984 or 1985), and 2) *third-party past professional connections,* formed when two people overlap through either a common past job (e.g., both having worked for IBM in their first job after graduation) or past board membership (e.g., both having served on Coca Cola's board). As suggested by its name, *third-party past professional* connections must predate the lending deal by more than five years, and can involve neither the borrowing firm or lending institution in any way. This requirement ensures that connections inferred between a banker at bank *X* and manager at firm *Y* are formed at a *distant place* (say, at firm *Z*, or during college) and *time* (at least five years ago.)

As a practical matter, this eliminates most of the connections we can infer, including those that arise from *current* common social organizations including charities, volunteer groups, museum boards, and others venues. To distinguish them from their *school* and *third-party past professional* analogs, we refer to these as *social connections* (admitting a slight abuse of language given that all the connections we analyze are ultimately "social"). Although *social connections* may also have a causal influence on lending behavior, BoardEx does not list the start and end dates for most of them – e.g., we cannot tell how long a CFO has served on the board of the Bronx Zoo (see also Schmidt and (2008) and Fracassi and Tate (2008)). Consequently, we would not be able to tell whether this seat came after a banking transaction with another Bronx Zoo director, or vice versa. For this reason, we ignore *social connections* entirely in our main analysis. What we lose in statistical power however, we gain in the ability to make precise, causal inferences insofar as personal connections influence lending outcomes.

In Panel B of Table 1, we list summary statistics for all three possible types of connections: *school*, *third-party past professional,* and *social*. The connection measures are calculated at the syndicate level; for example, the mean value of *third-party past professional connections* is 1.28, indicating that executives or directors of the average borrower share roughly two past jobs (since removed by five years or more) with executives or directors at any of the syndicate banks. *School connections* are far less common (mean 0.26), no doubt because of the restriction we impose that two individuals must have attended the same educational institution, but no more than two years apart. *Social connections*, which we consider only for robustness, are the most common.

Our analysis involves bank loans made to publicly traded companies within the U.S., the majority of which are syndicated between multiple banks that share lending risk. The source for these data is Dealscan, a proprietary product from Loan Pricing Corporation (LPC). This is by now a standard data source, and because a number of other papers provide excellent descriptions of its features, we refer the reader interested in more detail than we provide to these.[3]

The unit of observation in Dealscan is a credit facility, which can be either a loan with a specific maturity or a revolving line of credit.[4] For each facility, Dealscan lists a number of relevant firm and borrower characteristics including the amount loaned (or available as a line of credit), the identity of the firm and participant banks, the stated purpose of the loan, information about covenants, interest rate, maturity, and presence or absence of securitized collateral. Our main variables of interest are the rate charged (the "all-in drawn spread"), covenant variables and deal size, which we analyze as functions of the pre-existing personal connections between personnel at firm and syndicate banks. However, we employ the majority of the other available variables as controls. In Panel A of Table 1, we list a number of relevant summary statistics.

---

[3] For recent examples, see Bharatha, Dahiyab, Saunders, and Srinivasan (2007) and Qian and Strahan (2007).

[4] About 20 percent of our observations correspond to separate tranches within a lending "package." We consider each such tranche a separate observation (e.g., as does Bharath, Sunder, and Sunder (2008)), but note nearly identical results if aggregated to the package level.

A considerable part of our analysis concerns the *ex post* performance of borrowers after initiating a syndicated loan, specifically as it relates to firm-bank personal connections. Ideally, we would examine how individual loans perform, but because such data are generally not available, we examine various firm-level proxies instead. Two of these are very familiar: changes in public credit ratings and risk-adjusted stock returns, the former from Dealscan (COMPUSTAT also lists these), and the latter from CRSP. Our distribution of credit ratings (not reported) is standard, with a modal value (BB) just below the investment grade threshold. Hovakimian, Kayhan, and Titman's comprehensive study of credit rating targets (2009, Table 1) finds a very similar distribution.

Shown also in Table 1, Panel C are summary statistics for two proprietary credit risk measures made available to us from Moody's-KMV: *Expected Default Frequencies (EDFs)* and *EDF Implied Spreads (EISs).*[5] These provide alternative ways of measuring changes in default risk subsequent to a syndicated loan deal, and relative to ratings, offer broader and timelier coverage. The first is a numerical analog to a firm's credit rating, while the second is a "synthetic" spread based upon the firm's *EDF*. Importantly, *EIS* is intended to predict spreads on bonds, rather than on senior bank debt. Thus, *EIS* and *All-in Drawn Spreads* on bank debt are not directly comparable.

III. Personal connections and lending terms

We begin our analysis with a simple question: do lenders personally connected to their borrowers cut them better deals? We focus primarily on three terms easily available from Dealscan: credit spreads, deal size, and restrictive covenants.

[5] See Bohn and Crosby (2003) for an overview of the methodology behind the EDF, Agrawal, Arora and Bohn (2004) for a summary of the methodology behind EISs, and Dvorak (2008) for discussion of the adoption of these credit risk measures in practice.

A. Credit Spreads

Unless a firm can issue riskless debt, the interest rate it pays will include a "spread" above the risk-free rate, usually quoted in basis points (bp) above LIBOR or 10-yr U.S. Treasury yields. Dealscan employs the former benchmark. In our sample of syndicated bank deals, the average (median) spread is 206 (188) bp, indicating that if the government can borrow at 5%, then over the same horizon, the average (median) firm can borrow at a statutory rate of 7.06% (6.88%).

The credit spread is designed to compensate investors (here syndicate banks) for the risk of lost cash flows relative to default-free securities issued by the government. Generally, spreads take into account: 1) taxes – unlike corporate debt, U.S. treasuries are taxed at neither the local or state level, 2) liquidity – the secondary market for corporate debt is considerably thinner than that for treasuries,[6] and 3) default losses – should a firm default on its debt, lenders can anticipate only partial recovery of principal and interest.[7]

Clearly, connections between lenders and borrowers will not affect tax treatment, but may affect either of the other components. Perhaps the most obvious mechanism is that personal connections enhance information flow, and therefore, reduce the adverse selection problem faced by lenders when setting interest rates. This is particularly relevant in situations where information is difficult to describe (i.e., "soft" or intangible signals) or sensitive (e.g.,

---

[6] According to estimates in Basta, Price and Cho (2006, p.399), Marsh and Basta (2008), and Basta et al. (2009), the average proportional quoted spread – defined as the bid-ask spread divided by trade price – of syndicated loans traded on the secondary market averaged 50-65 basis points from January 2002 to July 2007. Spreads reached 136 basis points by December 2007, and widened to 325 basis points after the Lehman Brothers bankruptcy. In sharp contrast, the average bid-ask spread for Treasuries are usually less than 1 basis point (Fleming (2003)).

[7] Although both corporate bonds and bank loans are both less liquid and tax-disadvantaged with respect to U.S. Treasuries, these differences are mostly between corporate and government-issued notes, not across different corporate securities. A number of studies including Longstaff, Mittal, and Neis, (2005), Chen, Collin-Dufresne, and Goldstein (2009), and Cremers, Driessen, Maenhout, and Weinbaum (2004) have found that, despite differences in methodology, the non-default component in credit spreads is roughly 50-80 bp, and is relatively invariant to default risk. For more detailed discussion of the decomposition of credit spreads, see Elton, Gruber, Deepak, and Mann (2001), Huang and Huang (2003), Chen, Lesmond, and Wei (2007), and Ericsson and Elkhami (2009).

news about an upcoming patent becoming known by competitors). Similarly, it is possible that personal connections impose a personal cost on the firm's management should it strategically default on its debt obligations. Whether by allowing a syndicate to select better deals, or to actually make deals better, personal connections have the possibility of reducing default risk, and therefore, should reduce the firm's borrowing cost.

It follows directly that the secondary market for syndicated loans – already illiquid compared to that for other debt instruments – could be influenced by relationships as well. Although they do not focus explicitly on personal relationships, Drucker and Puri (2009) show that banking relationships (estimated by repeated transactions between a given firm-bank pairing) and loan sales are reinforcing. That is, rather than predicting the termination of a banking relationship, secondary market transactions are associated with *more* future business. If such banking relationship loans predict a more liquid secondary market, and if this liquidity is priced when interest rates are set *ex ante*, then this provides a second channel through which spreads may be affected by relationships.

To get a sense of the magnitudes involved, we focus first on simple, univariate comparisons. We are able to construct firm-syndicate personal relationship measures for almost 20,000 deals, although this number is trimmed substantially in regressions that require data availability for the large number of firm and industry characteristics we employ. For the time being, we consider this larger set, but keep in mind that we are not controlling for other important determinants of interest rates. Of the 19,554 deals matched with our connections database, at least one *school* or *third-party past professional connection* between the borrowing firm and a syndicate bank exists among 5,721 deals (29%). In such cases, the average (median) credit spread is 127 (88 bp). In the remaining 13,833 cases, the average spread is considerably higher, with an average (mean) of 239 (225) basis points.

However, in a regression that controls for other determinants of credit risk, this difference settles to approximately 28 basis points (Table 2, column 1). As seen, this is comparable to shifts in credit ratings. For example, an improvement of two rating categories from A to AAA decreases borrowing costs by 174-144 = 30 bp, whereas a single upgrade from BBB to A reduces the interest rate by 144-102=42 bp.[8]

An important set of controls is the set of indicators for previous *banking*, but not *personal*, relationships between the borrower and syndicate banks. Theories of financial intermediation have been advanced to predict both positive and negative effects on spreads for repeated firm-bank interactions. Boot and Thakor (1994) argue that when reusable information is generated in the process of originating a bank loan, subsequent spreads are lower because (part of) the fixed costs of information production are passed on to the lender. On the other hand, if the borrower has few other financing options, or if the information is sensitive (for example, to competitors), existing banks may reap monopoly rents, leading spreads to increase over time. Bharath, Dahiya, Saunders, and Srinivasan (2009) explore this dichotomous prediction, and find that repeated transactions are generally associated with reduced borrowing costs.

Following these authors, we include dummy variables for whether the borrower has transacted with at least one of the syndicate members in the last three years (t-3 through present), in the previous three years (t-6 through t-4), or even further back (t-9 through t-7). Confirming the findings of Bharath et al., Column 1 indicates that previous banking relationships are in fact associated with lower spreads, and intuitively, that this declines as the

[8] The notable increase in spreads between BB and BBB ratings corresponds to the investment grade threshold. Several important investor groups are restricted from holding non-investment grade debt securities, which can include corporate bonds and syndicated loans (a ruling by the U.S. Treasury Department in 1936; Financial Institutions Reform, Recovery, and Enforcement Act (FIRREA) of 1989). See Kisgen and Strahan (2009) for a summary of the historical development of regulations on credit ratings for bond market participants.

relationship becomes stale. However, even the largest banking relationship indicator has a magnitude (-13 bp) less than half that of the firm-bank personal relationship indicator.[9]

Also included is the number of lenders in the syndicate, as well as the number of aggregate deals completed by the syndicate members in the previous year. With these variables, we wish to model any size effects that may lead larger and/or more active syndicate banks to charge different rates of their borrowers.[10] As seen, the number of lenders does not appear significant, whereas more active banks charge somewhat lower spreads.

Additionally, we collect for each borrower and syndicate bank their zip codes and, when available, calculate the distance between their respective headquarters. If located less than 100 km apart, the *Local Bank* indicator takes a value of one, and zero otherwise. We include this variable for two reasons. The first is that if information collection or monitoring costs depend on proximity, then we want to account for these cost differences in our regressions. The second is that because the main variables of interest, those relating to personal connections, may be highly correlated with the proximity between a bank and borrower. To make sure that firm-bank personal connections are not simply picking up common location, we model the latter explicitly. As seen however, the *Local Bank* indicator has only a small, positive, and insignificant coefficient.

Finally, we include a number of macroeconomic controls. Motivated by Fama and French (1989) and Collin-Dufresne, Goldstein, and Martin (2001), we include the following five variables: the *level of the term spread* (the difference between 10-year treasury yield and 3-month treasury yield), the *one-year change in the term spread*, the *default spread* (the difference between the Moody's Baa corporate bond index yield and the Moody's Aaa corporate bond index yield), the *one-year change in the default spread*, and the *one-year value-weighted*

[9] Alternatively, we have split the sample into two groups: those in which the firm has conducted a prior deal with a current syndicate partner, and those in which it has not. The effect of personal connections of credit spreads is nearly identical in both groups.

[10] We have also estimated each of our models with indicators for individual banks, with little change in the results. See Section V for these and other issues related to robustness.

return on the S&P 500 Index. Generally, none of these provide any significant explanatory power. We also include year dummies, the logarithm of the loan or credit line's maturity (in months), and indicators for whether or not the facility is secured with collateral.

The second through fifth columns break up this regression by credit rating groups. Deals where the borrower's credit rating is A or better (A, AA, or AAA) are shown in column 2, which indicates that on average, personally connected deals are perceived by syndicates as being less risky. The point estimate on the personal connections indicator is -8 bp, which although small in an absolute sense, is almost 20 percent of the average spread for this group (mean 43 bp).

The same analysis is repeated for credit rating groups BBB-B and CCC-C respectively in subsequent columns. Results from the BBB-B group indicate substantial variation in credit quality, with spreads ranging 110 basis points on average between categories. Moreover, the effect of firm-bank personal relationships is over twice as strong, leading to an average reduction in the spread of 20 bp with personal relationships present. The fourth column contains only 359 observations, but because the magnitude on the relationship indicator is so high (-51 bp), it nevertheless yields a statistically significant estimate for this sample. Perhaps the most immediate takeaway from Table 2 is that personal relationships are a robust determinant of borrowing costs, but most so for firms with poor credit.

The final column shows the results for the roughly 45% of firms lacking a public credit rating at the time the syndicated deal is initiated. Interestingly, the effect of personal relationships for these unrated firms are similar to those observed for low credit rating firms (particularly those with CCC credit or worse), with a magnitude of -47 bp. Because we know relatively little about the credit characteristics of these firms, we do not emphasize these results. We do note however that, as pointed out by Faulkender and Peterson (2006), the decision to secure a public debt rating is endogenous, and is correlated with the firm's information environment. Specifically, firms with sensitive information may find the scrutiny associated with a credit rating agency's evaluation undesirable. In such situations, personal connections that confer trust are likely to be of particular value.

A potential criticism of the results of Table 2 is that although we have controlled for the probability of default with credit ratings, we have not accounted for differential recoveries given default. Because recoveries depend on industry and firm characteristics (for evidence, see Altman and Kishore (1996), Acharya, Bharath, and Srinivasan (2007)), we include in Table 3 a number of firm and industry-specific control variables likely to affect asset recoveries in liquidation. Along with dummies for each of the Fama and French 30 industry classifications, we also include each firm's *lagged total assets* (in logarithms), *market-to-book ratio*, *capital expenditures* (scaled by assets), *percentage of assets that are tangible*, and *profitability* (EBITDA scaled by assets), and *CAPM Beta*. If creditors account for the expected correlation of default losses with the aggregate market (Ross (1985), Almeida and Philippon (2007)), we should expect a positive coefficient on the latter.

Requiring data availability for all of these variables substantially reduces the size of our sample, to just over eleven thousand firms. Because summary risk measures are so important for predicting spreads, but because so many firms are not publicly rated, in Table 3 we account for default risk with Moody-KMV *EDF* implied ratings, for which we have more extensive coverage. We group firms into deciles of *EDF*, and then include dummies for nine of these in the regressions. Including the numerical value of *EDF* makes almost no difference.

The first column of Table 3 shows the results. Although the coefficient on the personal connections indicator drops somewhat, it remains highly significant, both statistically ($p<0.001$) and economically (-18 bp).[11] As before, this coefficient becomes more negative for firms with worse credit ratings, although to save space, we do not repeat this disaggregation. Most of the

[11] The reduction in magnitude on the firm-bank *personal connections* indicator coefficient is primarily due to the changing of the sample (firms without COMPUSTAT data are more likely to be young, small, growth firms), rather than to the addition of new control variables.

firm-level variables either are, or border on being, statistically significant, with size, profitability, and market-to-book having the most predictive power.

For making causal inferences, it is important that personal connections are not simply capturing other firm attributes that may affect borrowing costs. In the second column, we include firm fixed effects, and thus, hold the borrower constant but vary the lending syndicate. Of course, this procedure admits only the set of firms that complete at least one deal with a connected syndicate, and at least one with a non-connected syndicate.

The marked increase in R^2 (from 0.50 to 0.75) makes clear that despite our attempts to control for the probability of and losses given default in column 1, latent firm characteristics play an important role in lenders' risk assessments. Nonetheless, the inclusion of firm dummies leaves the personal connections indicator nearly unchanged. Holding the borrowing firm constant, column 2 indicates that the presence of at least one *school* or *third-party past professional* connection reduces the charged interest rate by 17 basis points ($p<0.001$).

Because the sample period is so short (2000-2007), any remaining concern over omitted variables can pertain only to unobservable, firm-specific attributes that: 1) vary at high frequency, and 2) are correlated with the personal connections variables. In unreported results, we have re-run the regression with firm-year fixed effects, which limits the relevant sample to firms that conduct both connected and unconnected deals within the same year. The point estimate on personal connections is similar to that found in the first two columns of Table 3, but is estimated very imprecisely ($p>0.10$). Likewise, we note (but do not report) that a firm's characteristics are not significantly different when it participates in a connected versus an unconnected deal: size, market-to-book, profitability, asset tangibility, and investment rates are nearly identical across deal types, suggesting that time-varying, firm-specific differences in risk factors are unlikely to explain the results.

The third and fourth columns of Table 3 show the results when we model the personal connection-credit spread relationship with logarithms. Comparing columns 1 and 3, we see that a logarithmic specification not only provides a substantially better fit (R^2=0.615), but also strengthens the statistical significance of firm-bank personal connections. The coefficient on the log of connections indicates that by doubling the number of personal connections between a firm and its syndicate partners, the firm pays a spread over 13 percent less. On average, this means that 1.5 additional connections (the mean of this variable) are associated with a spread reduction of approximately 179*0.134 = 24 bp. The final column shows that although including firm fixed effects substantially decreases the magnitude of the spread-connection elasticity (point estimate of -0.048), it remains highly significant ($p<0.001$).

Before proceeding, we briefly note that the non-linear relationship between spreads and firm-bank personal connections indicated in the log-log specification is confirmed in a number of unreported specifications (e.g., quadratic, non-parametric regressions). Regardless of the empirical model, we consistently find that the value of each connection diminishes as the aggregate number of firm-bank connections within the syndicate increases. Given that spreads are bound from below at zero, this result may not be particularly surprising. On the other hand, this constraint binds for only firms of the highest credit quality, and as we have already seen, these are exceptional cases.

B. Covenants

Interest rates are but one mechanism by which syndicate banks can protect themselves *ex ante* from the risk of having financed a poor project, or from *ex post* risk shifting by management. The state-dependent transfer of control rights via covenants is another. Here, we explore whether personally connected lenders substitute interest rate concessions for tighter and/or more restrictive covenants that constrain the firm's behavior.

Essentially, covenants are provisions in a debt contract that specify technical default. Even if a firm has not missed an interest or principal payment, violation of a covenant shifts control rights to the lender(s), requiring the borrower, for example, to accelerate principal repayment or post additional collateral. Covenants are discretionary features in credit agreements, and often pertain to operating performance or debt coverage ratios. A number of recent papers have investigated the role of covenants insofar as they relate to creditor intervention (Chava and Roberts, 2008; Nini, Smith, and Sufi, 2009; Roberts and Sufi, 2009a), renegotiation (Roberts and Sufi, 2009b), and the sales of syndicates loans (Drucker and Puri (2008), Gupta, Singh, and Zebedee (2008)).

We take a reduced form approach, and simply sum the number of covenants (if any) listed for each credit facility. Besides that reflected by their prevalence, our analysis ignores any information reflected in the covenants themselves, e.g., whether they are "strict" or "slack," or whether certain types of provisions are more or less common in connected deals. For about one-third of the deals, no covenant is listed in Dealscan; for the remaining two-thirds, the average number of covenants is 4.7, with a standard deviation of 3.1.

Table 4, Panel A presents the results of analyzing loan covenants as a function of our personal connections variables. We employ the same set of firm, loan, bank, and macroeconomic controls as in Table 3. In the first two columns, the dependent variable is discrete, taking a value of one if any covenants are listed by Dealscan, and zero otherwise. The marginal effects shown in these columns indicates only suggestive evidence for the indicator connections variable (column 2), but a stronger result for the more continuous connections variable (column 1). By doubling the number of personal connections, the probability of covenants being required decreases by 2.3 percent, a result significant at the one percent level. In unreported results, we find that this result – like all others in the paper – is considerably stronger for firms with poor credit ratings.

For robustness, shown in the next columns are results from linear regressions, where the dependent variable is the number of covenants required (possibly zero). We conduct this exercise to allow firm fixed effects. As in the previous columns, the logarithmic specification indicates a negative relation between firm-bank personal connections and covenants; the discrete specification for the full sample does not.

C. Deal Size

The results so far indicate that firm-bank personal connections lead to less stringent lending terms, and that firms with the worst credit – for whom adverse selection and managerial incentive problems are likely the greatest – benefit the most. Here, we consider whether the effects we document apply only to small loans, or whether they generalize to larger stakes.

In Table 4, Panel B, we consider as the dependent variable the natural logarithm of the deal size, or tranche amount. All columns employ the same set of control variables employed in previous tables including firm size (lag of total assets, volatility, Fama-French 30 industry classification, etc).

Estimates in the first and second columns suggest that increasing the number of firm-bank personal connections increases the amount lent. The log-log specification indicates an elasticity of roughly 3.5 (column 1); the discrete specification (column 2) shows that compared to deals lacking personal connections, syndicated deals among personally connected members are over 13 percent larger, translating to roughly $45 million on average. In the final two columns of Panel B with firm fixed effects, both specifications indicate a strong, positive relation. Compared to the specification in column 1, the inclusion of firm fixed effects slightly strengthens the result. The elasticity is a precisely estimated 0.076, indicating that 1.5 additional connections increase the average loan balance by over $40 million. The discrete model shown in the final column indicates a slightly strengthened effect for personal connections on loan balances, compared to the model without firm effects.

IV. Ex-post performance

The results of Section III indicate that firm-bank personal connections shift lending terms in the borrower's favor, but are silent with respect to the reasons why. Holding risk constant, more lenient terms would result in a wealth transfer from the bank to the firm's shareholders. However, if firm-bank connections alter the risk profile of the borrower – either by mitigating adverse selection problems or improving the bank's ability to monitor and alleviate borrower's moral hazard incentives – then the concessions documented in Tables 2 through 4 may be warranted.

The ideal test would be to compare default rates between connected and unconnected syndicates. Unfortunately, Dealscan does not provide data on the performance of individual loans, and because the secondary market for such securities is extremely illiquid, examining prices is not feasible. Absent performance data on specific loans, we examine various firm-level performance metrics that, while noisy, nevertheless provide information about the firm's ability to service its debt obligations: credit ratings, *EDFs*, *EDF* implied spreads (*EIS*), and stock returns. All of these are benchmarked to the date of the syndicated deal, and tracked forward.

A. Future Credit Ratings

If a firm's fundamentals deteriorate after securing a loan or line of credit, this should be captured by changes in its future credit ratings. Dealscan provides, for every firm with publicly rated debt, the long-term rating at the time the syndicated deal is initiated. From Moody's (and then cross-checked with COMPUSTAT), we obtain each borrower's future credit rating 12, 24, and 36 months subsequent to the deal of interest. Additionally, we collect ratings as of July 2009, the date the data were assembled.

Before proceeding, we note one important change to the sample. In Section III, the unit of observation was the individual credit facility, which occasionally included multiple tranches within a loan package defined by firm, syndicate group, and origination date. In other words, a syndicate might (for example) simultaneously provide a $500 million line of credit at 7%, as well as a subordinated $300 million line of credit at 8%. Following Bharath, Sunder, and Sunder (2008), we treated these as independent observations in our previous analysis. However, while the fact that loan characteristics vary across tranches justifies their inclusion in the previous application, this is clearly inappropriate when examining firm-level performance. Even if a firm borrows against multiple lines of credit within the same loan package, this clearly constitutes only one independent observation for the firm's *ex post* performance. Collapsing at the package level reduces the sample by about 20 percent, relative to that analyzed in Section III.[12]

In Figure 1, we compare the evolution of future credit ratings following personally connected deals (gray bars), to that following unconnected deals (black bars). Initial credit ratings are shown in each panel, starting with rating category AA; final ratings are those as of July 2009. In this exercise, we wish to track future ratings changes for as long a window as possible, and so impose the same calendar ending date for all firms. Later analysis varies the end dates, but standardizes the window – e.g., 12 months after a deal, 24 months afterward, etc.

The striking differences between the black and gray bars in Figure 1 underscore the importance of personal connections as an *ex ante* indicator of deal quality. As seen, the credit ratings of connected firms tend to drift upward or remain the same, whereas the ratings of firms lacking personal connections to their syndicate partners are more likely to worsen.

Without exception, this pattern holds for every initial rating category, a remarkable finding given we are analyzing *changes* in ratings, not levels. The probability of being downgraded following a connected deal, by rating category is AAA: 4.7%, AA: 5.8%, A: 9.7%, BBB: 6.2%, BB: 14.4%, B: 5.0%, CCC: 0%. By comparison, the comparable list for firms that borrow from unconnected syndicates: AAA: 10%, AA: 44.2%, A: 15.6%, BBB: 10.5%, BB: 23.6%, B: 7.0%, CCC: 0%. The mirror pattern is seen for upgrades.

[12] The results in Section III are nearly identical in each specification.

Table 5 puts these univariate patterns in a regression framework. The first, second, and third pairs of columns respectively, track credit ratings changes at the 12, 24, and 36-month interval after the initiation of a syndicated bank deal. In each case, the dependent variable is a discrete indicator *Downgrade*, taking a value of one if the firm is subsequently downgraded (e.g., BBB to BB or below), and zero otherwise. Note that for firms completing deals in the latter part of our sample, not enough time has passed for their future credit ratings to be analyzed (e.g., a firm borrowing in Dec. 2007 does not, at the time of writing, have a 36-month ahead rating).

As seen in columns 1, 3, and 5, the presence of at least one personal connection has a dramatic effect on the future trajectory of credit rating changes. With each passing year, connected firms are about 2.5 percent less likely to be downgraded than their unconnected counterpart borrowers. By the third year, the effect is over 7 percent, and is significant at far better than the one percent level. In the second, fourth, and sixth columns, we see that the logarithmic specification also significantly predicts downgrades, and more so at longer horizons. In unreported results, we have estimated the same regressions, but exclude firms whose future credit ratings do not change over the relevant interval. This restriction magnifies further the distinction between connected and unconnected deals. Likewise, regressions of credit rating upgrades produce similar results, although the signs are predictably reversed.

When analyzing credit ratings, it is important to note that there is some evidence of serial correlation in rating changes, particularly for highly rated firms (e.g., Altman and Kao (1992)). While we do not expect this to have a differential effect between connected and unconnected deals – and thus, would not expect our connection variables to be biased – in unreported results we have conducted a number of robustness checks, e.g., by including prior ratings changes, prior stock returns, and other measures of pre-existing trends in default risk. None materially change the reported estimates. Moreover, as we later show, stock returns of connected borrowers are also higher following syndicated deals, suggesting that the rating changes we document are not simply continuations of existing trends in firm risk.

B. *EDF* and *EDF*-Implied Spreads

The preceding exercise is possible only for firms with public debt ratings. Here, we gain roughly 3,000 observations by regressing future *EDF*s (Panel A) and *EIS* (Panel B), both firm-level credit risk estimates provided to us by Moody's, on the firm-bank personal connections variables used in our previous tests. Although we include the same set of firm and industry characteristics as in previous regressions, the key control variable is the value of either *EDF* or *EIS* when the loan originates.

Comparing Panel A, columns one (12 months), three (24 months), and five (36 months), we see that the presence of firm-bank personal connections remains an important predictor of *EDF* over each window. As in Table 5, the effect pronounces at longer horizons. For example, in the 36-month period shown in column 5, firm-bank personal connections are associated with almost a three-fourths unit decrease in *EDF*. To put this in perspective, the average firm has an *EDF* of 2.71, which would correspond roughly to a BB rating. A unit shift of *EDF* in either direction would move the corresponding credit rating approximately one-half a rating category. The logarithmic specification for connections is somewhat weaker from a statistical significance perspective; however, all the point estimates are negative, and the final column is significant at the 5 percent level.

A similar picture emerges in Panel B, where each firm's future EDF Implied Spread (EIS) is modeled as a function of firm-bank personal connections, along with the usual set of control variables. The first column indicates that even controlling for the firm's initial *EIS*, the presence of personal connections to syndicate members reduces its future, expected borrowing cost by 49 basis points twelve months in advance. By 24 months, the expected reduction is 77 basis points, in the neighborhood of being upgraded from junk (BB or worse) to investment grade (BBB or worse). At three years, the marginal effect is 80 basis points. As in the *EDF* regressions, the log specification (columns 2, 4, and 6) is not as strong, but paints largely the same picture.

Of course, because *EIS* is designed to measure spreads for public debt, the magnitudes we observe in Table 6 are substantially higher than what we observe in Tables 2 and 3. As mentioned previously, bank debt is almost always written senior to bonds, a priority structure that inherently places the latter at higher risk. We present the *EIS* results to emphasize exactly this distinction. Table 2 already shows that that the impact of personal connections on borrowing costs is decreasing in default probability; if the same dynamics apply to more junior claims (e.g., bond placements with institutional investors), then the magnitudes we document for bank loans are likely a lower bound on the more general effects in debt markets.

C. Stock Returns

The three dependent variables we have considered so far – future credit ratings, *EDFs*, and *EISs* – are all explicitly designed to evaluate the firm's ability to service its debt obligations. Stock returns are also useful in this regard, and importantly, are immune from the criticism that credit rating changes are serially correlated, or are predictably from other information not captured in our regressions.

In general, stock returns are a better predictor of default as credit quality worsens. Obviously, a firm with minimal leverage will most likely be able to make interest payments, even after a substantial decline in its equity value. Thus, the evidence in this section, insofar as it is used to infer the performance of the underlying loan, should apply mostly to firms with modest to poor credit ratings.

Table 7 contains three panels. Compared to Table 6, each panel considers the same horizons, sample, and control variables. However, in Panel A, the dependent variable is each stock's size, book-to-market ratio and price momentum *characteristic-adjusted return*, following Daniel, Grinblatt, Titman, Wermers (1997). Essentially, this approach adjusts individual stock returns by subtracting the returns from a portfolio with similar size, book-to-market ratio and price momentum. As before, we allow borrower-syndicate personal connections to enter in both a discrete and logarithmic specification.

The first two columns of Panel A indicate that over a one-year window, there is only suggestive evidence that stock returns of connected borrowers are higher than those of their unconnected counterparts. Both point estimates are positive, but the standard errors are relatively large by comparison. In the third and fourth columns, we see stronger evidence that returns are predictable from a firm's personal connectedness to its syndicate members. The log specification indicates that doubling the number of personal connections increases the firm's risk-adjusted stock returns by almost 5 percent ($p<0.001$). The discrete specification effectively compares connected vs. unconnected deals, and indicates a two-year, risk-adjusted difference of over 10 percent. The final two columns show that at the three-year horizon (we use the most recent stock price if three years have not past), connected borrowers perform 17 percent better than borrowers not personally connected to their syndicates ($p<0.001$). Annualized, this corresponds to a risk-adjusted (excess) return of 5.6 percent.

One potential concern is that the results in Panel A may be picking up common, date-specific factors that influence returns. Although we have little reason to believe that such time effects would be systematically related to personal connections, Panel B presents the results of Fama-MacBeth monthly regressions. Here, we consider each month as a separate family of observations, and regress future risk-adjusted stock returns against the personal connections variables. For example, in July 2005, we regress the 12, 24, or 36-month future, characteristic-adjusted returns of every firm that borrowed in that month. By running such a regression month-by-month, we eliminate by construction cross-sectional correlation. The averaged coefficients on the connections variables are shown in Panel B, and in every case, strengthen relative to those seen in Panel A.

Panel C of Table 7 presents the evidence slightly differently. Here, we define the dependent variable *Extreme Low Return*, a binary variable that takes a value of one if the firm's stock return is -50% or below. This is arbitrarily defined, although the result is robust to other cutoffs. The marginal effects from probit regressions confirm the evidence in Panels A and B, where we find that at longer horizons, firm-bank personal connections significantly reduce the probability of a return low enough to likely impair the firm's debt service. As before, the discrete specification produces stronger return predictability; moreover, the results are stronger at the two- and three-year horizons in part because of statistical power. (Because we have defined *Extreme Return* as -50% or below, it is unsurprising that we have little variation at short horizons.)

We have also experimented with calendar time portfolios that involve long positions in connected borrowers, and short positions in unconnected ones. Because we have such a short time span, the number of monthly observations afforded by such an approach is small (around 100). In unreported results, we find trading profits on par with the results observed in Panels A and B. Long-short portfolios average between 20 and 30 basis points per month, and regardless of the holding period (12, 24, or 36 months), yield positive trading profits in more than half the months. However, even the best of these yields only a t-statistic in the 1.8 range, bordering on statistical significance, but relatively impressive for such a limited number of monthly observations.

The evidence in this section speaks to the reason why more lenient terms are awarded to personally connected firms. One the one hand, bankers may gain value from cutting their friends good deals – i.e., on terms not justified by the firm's fundamentals or future prospects - and may therefore be willing to finance such private benefits with their own shareholders' money. On the other hand, personal relationships may reduce monitoring costs or information asymmetries, often cited as reasons why institutional lending may exist at all (e.g., Bernanke (1983)).

We find no evidence that the favorable lending terms extended to personally connected firms stem from agency problems on the part of bankers. Whether measured by future stock returns or credit ratings, firms perform *better* after completing a deal with a personally connected syndicate, suggesting that rather than facilitating poor deals, firm-bank connections appear to reduce the risk faced by member banks. Of course, none of the evidence herein can tell us whether personal connections allow syndicates to choose better deals *ex ante*, or whether they allow syndicate banks to monitor their borrowers more efficiently. While interesting, the distinction between adverse selection and moral hazard is secondary to whether connected deals are better or worse, to which the evidence in this section does speak.

V. Robustness and other considerations

A. Connection Types

Because we wish to make causal inferences between personal relationships and lending behavior, we consider only connections formed at third-party venues (school or other firms/banks not involved in the deal analyzed), and at least five years prior to the deal of interest. The time restriction is imposed to rule out any reverse causality, such as membership in social organizations being a reward for a favorable banking deal. The cross-sectional restriction addresses a less specific concern, and is simply meant to place additional distance between the formation of personal connections and the banking deals we analyze.

Practically, this means that we ignore the majority of the possible connections we can infer. Recall that connections exist not only from common schooling institutions or past workplaces, but also from active roles in common *social* organizations, e.g., think tanks (Council on Foreign Relations), charities (Saint Agnus Foundation), non-profit organizations (National Urban League), and philanthropies (Boston Science Museum). Including such connections confers a marked increase in statistical power; through sheer size, connections formed within the universe of social organizations far outnumber those formed via common schooling institutions and third-party workplaces. However, without being able to identify the specific dates when such *social* relationships are formed (and thus leaving them vulnerable to the reverse causality critique), we cannot defend their inclusion in our main analysis.

With this caveat in mind, we break up our existing connection measure into its components (*third-party past professional* and *school*), and to it add *social* connections in the first two columns of Table 8. As before, we include both the discrete (column 1) and logarithmic (column 2) specifications. In both columns, we see that all three varieties are negatively related to credit spreads, with *social* connections having the largest point estimate (-13 bp vs. -9 for the other types).

Given the strong result for *social* connections, it is tempting to formulate causal explanations for the impact of *social* connections on spreads similar to that for the other types of connections. Indeed, one could argue that because common social organizations provide a natural venue for relationships to persist into the future (*school* and *third-party past professional* connections have no comparable venue), that they would be particularly costly to damage. In connected deals where such valuable *social* relationships are effectively pledged as collateral, we might expect larger marginal effects on credit spreads. While consistent with the evidence in the Table 8, so too is the possibility for banking transactions to influence – rather than be influenced by – the social connections we observe. Without a way to distinguish between the two, we interpret the effects of *social* connections as merely suggestive evidence in support of the other connection variables.

B. Syndicate Features

The majority of our control variables, like most studies of capital structure, are defined at the firm level. Partly, this is because detailed data on financing's supply side is comparatively scarce; on the other hand, in situations where frictions are low and capital providers are relatively homogenous (e.g., bond markets), we would perhaps not expect lender-specific attributes to play an important role. The second is less applicable to bank financing, where the ability to screen and monitor borrowers may differ considerably between banks. To the extent that such differences are correlated with our connection measures, the coefficients we report may be biased.

Perhaps the most obvious possibility is that larger and/or more active banks have scale economies that allow them to undercut their smaller counterparts. Moreover, because larger banks have more employees and directors, the expected number of personal connections with any borrower will be larger. We have already addressed this possibility in some detail previously, having controlled for both the number of lenders in the syndicate as well as the aggregate lending activity of its member banks in all regressions. For robustness, we provide more detail along these lines here.

In the third column of Table 8, we exclude from consideration any deal in which any of the five most active banks was a participant. As seen, this restriction has an enormous impact on the number of observations (11,003 in Table 3 vs. 3,948 in Table 8, column 3), reflecting the ubiquity of the most active commercial banks. Nonetheless, even when the largest banks are absent, the effect of firm-bank personal connections survives. The coefficient reported in Table 8 (0.13) is nearly identical to the full sample (.12), and remains highly ($p<0.001$). Similar magnitudes are observed if the sample is cut even further, but as the number of observations decreases, so too does the ability to make statistical inferences.

The fourth column again considers the full sample, but includes fixed effects for each of twenty most active banks, defined by the number of deals in the previous year. (Eight-four percent of our observations include at least one of these banks.) Notably, their inclusion increases the explanatory power increases almost two percentage points, indicating the presence of lender-specific attributes on credit spreads. However, the effect of bank-firm personal connections remains virtually unchanged compared to the previous column or to Table 3, indicating an elasticity of slightly over .12 ($p<0.001$). Other unreported robustness checks including a larger number of fixed effects, or interacting previous years' activity with firm-bank personal connections, none of which has a meaningful effect on the variable of interest.

C. Measurement Error

All analysis involves proxies for personal connections between firms and lenders – never do we observe these relationships directly. Thus, when we include one's school classmates or past co-workers in a regression of lending terms or *ex post* performance, we have certainly introduced errors-in-variables. Because we have no reason to believe that this measurement error is systematically related to unobserved, *genuine* connections, the estimated coefficients are simply biased to zero, implying lower bounds on the underlying economic phenomena.

D. Firm-Bank Matching

We conclude by considering a natural extension of the results presented so far. In this section, we take a step back from analyzing terms of completed deals, and model the firm-bank matching process as a function of pre-existing personal relationships. Specifically, the favorable lending terms in Tables 2-4 suggest that when a firm has a choice, it should prefer doing business with a personally connected lender.

To test this, we begin by constructing the "population at risk" as the set of *possible* firm-bank interactions, regardless of whether that pair consummate a deal or not. For inclusion in a given year: 1) a firm must have completed at least one deal that year, and 2) the lending bank must have ranked in the top 300 in deal volume the previous year. The reason for the first condition is obvious, while the second is meant to reduce the number of firm-bank pairs to a manageable number. We have experimented with various cutoffs (e.g., requiring lenders to be among the top 50, 100, or 200 most active banks the previous year), and obtain similar results with each.

Shown in the first three columns are the marginal effects from probit regressions, where the dependent variable is an indicator for a firm-bank pair having done banking business that year. For example, assume that IBM raised debt capital from a 5-member syndicate in 2005. Our methodology would pair IBM with each of the 300 top syndicated lenders from 2004, resulting in 300 potential combinations involving IBM. During 2005, the dependent variable would take a value of one for the five IBM-bank observations corresponding to IBM's lenders. The remaining 295 observations would take a value of zero. In each column, we include controls for past deals between a given firm-bank pairing (the same three variables in all previous tables), as well as indicators for common geographic location between the firm and bank.

Our main interest is whether the presence of personal connections between a firm and bank predict a lending relationship. In the first column, the coefficient indicates that at least one firm-bank personal connection increases the probability of matching by over 3.2 percent ($p<0.001$). Given that the unconditional mean of this variable is 3.9 percent, this point estimate suggests that the presence of personal connections increases, by almost 85 percent, the likelihood that a randomly chosen firm-bank pairing completes a deal. Subsequent columns add bank and firm fixed effects, each of which cut the effect by one percent in absolute terms. Taking the final column as the most indicative of the underlying economic model, we see that the presence of personal connections increases the likelihood of a firm-bank pair doing business by about 30%.

The final three columns repeat this exercise, but instead of a discrete dependent variable, we regress the logarithm of the number of deals between a firm-bank pairing on the logarithm of personal connections between them. Similar results to the first three columns obtain.

The evidence in Table 9 represents more than an afterthought to the previous analysis in Sections III and IV, which is already conditioned on a deal occurring. Here, we see that in addition to altering conditional lending terms and outcomes, personal connections help shape a firm's banking partners. In other words, personal connections influence both the probability of a “friendly” bank deal occurring, and then change the conditional payoff if it does. The ex ante expected value of a firm’s personal connections, of course, depends on both.

Moreover, the fact that banks are more likely to lend to connected firms is not an obvious implication of the earlier results. One might imagine that some banks, wary of appearing corrupt, might lend only to exceptionally qualified firms with whom they are personally connected. The matching results here are direct evidence against this: banks and firms that share personal connections are *more likely* to deal with one another. Moreover, the success of personal relationships in predicting lending relationships is unrelated to firm fundamentals such as profitability and market-to-book (Section III, page 16), evidence against a "higher bar" for the fundamentals of connected firms.

VI. Conclusions

A number of theories credit the very existence of banks with screening or monitoring advantages relative to more disperse creditors. Yet, what exactly is it about banks, and some more than others, that confers them special ability to manage such difficult borrowers? A banker’s answer to this question will likely involve the word “relationship.” This paper studies a specific kind of relationship – *personal relationships* between employees at firms and their lenders.

We ask two related questions: 1) do personal relationships lead to more favorable financing terms, and 2) if so, are these decisions justified by ex post performance? With detailed data on roughly 20,000 syndicated loans by over 5,000 public U.S. firms and almost 2,000 commercial banks, we find that the answer to both questions is “yes.” Compared to syndicated deals where the firm’s management (or directors) is not personally connected to any syndicate bank, connected ones are associated with substantially lower interest rates, fewer covenants, and larger loan amounts. The interest rate concessions depend on the borrower’s risk, with higher risk firms awarded larger rate reductions. Furthermore, after initiating a deal with a personally connected syndicate, firms improve their credit ratings and enjoy substantially higher stock returns. Together, the concessions in lending terms in connected situations appear justified by *ex post* performance.

It is difficult to posit a plausible, non-causal interpretation for the role played by firm-bank personal connections in the commercial loan market. By focusing exclusively on personal relationships formed several years prior to the banking deals we analyze – and at different venues from the borrower or lender – we exclude the possibility that personal relationships are a product of existing or anticipated banking relationships.

Finally, we note that most of the world's developed economies have explicit insider trading laws intended to prevent parties with more than an arm's length relationship with the firm from reaping undue financial gain when trading its securities.[13] Given the ostensible goal of such legislation to level the informational playing field among market participants, it is interesting that no similar statutes apply to a firm's financing arrangements. Firms are free to raise capital from whichever sources they like, irrespective of any affiliation that may compromise either party's objectivity or allegiance to their own shareholders. Indeed, studies such as La Porta, Lopez-de-Silanes, and Zamarripa (2003) show, quite dramatically, that some distance between firms and banks can be healthy.

Yet were this the full story, the lack of regulation against all but arm's length lending arrangements would be difficult to explain. Instead, it is possible that the very same relationships that allow banks to be exploited allow them to resolve information or agency problems with lenders, potentially improving lending decisions and outcomes. The reliance on personal relationships in microcredit groups such as the Grameen Bank of Bangladesh is a well-publicized example. There, borrowers are screened and monitored by members of their social circle, which allows credit to be provided even in the absence of collateral (Besley and Coate

[13] Examples in the U.S. include the Securities Act of 1933, Securities Exchange Act of 1934, and Insider Trading Sanctions Act of 1984.

(1995), Woolcock (1998), Yunus (1993)). In this market, personal relationships create value by implicitly monetizing social capital, making tangible the information and reciprocity afforded members of a social network. The evidence in this paper suggests that such a model can also act at the corporate level. How firm-bank personal relationships alter lending terms over the life of a loan – such as following covenant violations or in renegotiation – we leave to future work.

References

Acharya, Viral V., Sreedhar Bharath and Anand Srinivasan, 2007, Does Industry-wide Distress Affect Defaulted Firms? - Evidence from Creditor Recoveries, *Journal of Financial Economics* 85(3), 787-821.

Agrawal, Deepak, Navneet Arora, and Jeffrey Bohn, 2004, Parsimony in Practice: An EDF-based Model of Credit Spreads, *Technical Paper*, Moody's KMV.

Almeida, Heitor, and Thomas Philippon, 2007, The Risk-Adjusted Cost of Financial Distress, *Journal of Finance* 62(6), 2557 – 2586.

Altman, Edward I. and Duen Li Kao, 1992, Rating drift of high yield bonds, *Journal of Fixed Income* 1(4), 15–20.

Altman, E. and V. Kishore, 1996, Almost Everything You Wanted To Know About Recoveries On Defaulted Bonds, *Financial Analysts Journal*, Nov/Dec, 57-64. Reprinted in High Yield Bonds: Market Structure Portfolio Management and Credit Models, T. Barnhill and W. Maxwell, Editors, McGraw-Hill, 1999.

Basta, Theodore, Bahar Hafizi, Baljit Mann, Bridge Marsh, Alex Spiro, and Peter Wasserman, 2009, Current States of the Syndicated Loan Markets in the United States and Canada, presentation at American Banker Association. Available at http://www.lsta.org/WorkArea/showcontent.aspx?id=5920

Basta, Theodore, Tom Price, and Jean Cho, 2006, Secondary Market Pricing, in *The Handbook of Loan Syndication and Trading*, edited by Alicia Sansone, 421 – 506, McGraw-Hill.

Berger, A., and Udell, G. F., 1995, Relationship lending and lines of credit in small firm finance, *Journal of Business* 68, 351–381

Bernanke, B., 1983, Non-Monetary Effects of the Financial Crisis in the Propagation of the Great Depression, *American Economic Review*, Vol. 73, 257-76.

Besley, Timothy and Stephen Coate, 1995, Group lending, repayment incentives and social collateral, *Journal of Development Economics* 46, 1–18.

Bharath, S. T.,, Sandeep Dahiya, Anthony Saunders and Anand Srinivasan , 2009, Lending Relationships and Loan Contract Terms, *Review of Financial Studies*, Forthcoming.

Bharath, S. T., J. Sunder, and S. V. Sunder. 2008. Accounting quality and debt contracting, *Accounting Review* 83, 1–28.

Bharath, Sreedhar, Sandeep Dahiya, Anthony Saunders, and Anand Srinivasan, 2007, So what do I get? The bank's view of lending relationships, *Journal of Financial Economics* 85, 368-419.

Bohn, Jeffrey, and Peter Crosby, 2003, Modeling Default Risk, *Technical Paper*, Moody's KMV.

Boot, Arnoud W A., and Anjan V. Thakor, 1994, Moral Hazard and Secured Lending in an Infinitely Repeated Credit Market Game, *International Economic Review* 35(4), 899-920.

Chava, Sudheer, Dmitry Livdan, and Amiyatosh Purnanandam, 2009, Do Shareholder Rights Affect the Cost of Bank Loans?, *Review of Financial Studies* 22(8), 2973-3004.

Chava, Sudheer, and Michael R. Roberts, 2008, How Does Financing Impact Investment? The Role of Debt Covenants, *Journal of Finance* 63(5), 2085 – 2121.

Chen, Long, David Lesmond and Jason Wei, 2007, Corporate Yield Spreads and Bond Liquidity, *Journal of Finance* 62, 119-149

Chen, Long, Pierre Collin-Dufresne, and Robert S. Goldstein, 2009, On the Relation Between the Credit Spread Puzzle and the Equity Premium Puzzle, *Review of Financial Studies* 22(9), 3367-3409.

Chutatong Charumilind, Raja Kali, and Yupana Wiwattanakantang, 2006, Connected Lending: Thailand before the Financial Crisis, *Journal of Business* 79, 181-218.

Cohen, Lauren, Andreas Frazzini, and Christopher Malloy, 2008, The small world of investing: Board connections and mutual fund returns, *Journal of Political Economy* 116(5), 951-979.

Cremers, Martijn , Driessen, Joost, Maenhout, Pascal J. and Weinbaum, David, 2004, Individual Stock-Option Prices and Credit Spreads, Yale ICF Working Paper No. 04-14.

Daniel, Kent, Mark Grinblatt, Sheridan Titman, and Russ Wermers, 1997, Measuring Mutual Fund Performance with Characteristic Based Benchmarks, *Journal of Finance* 52(3), 1035-1058.

Degryse, Hans, and Patrick Van Cayseele, 2000, Relationship Lending within a Bank-Based System: Evidence from European Small Business Data, *Journal of Financial Intermediation* 9(1), 90-109.

Diamond, Douglas W., 1984, Financial Intermediation and Delegated Monitoring, *Review of Economic Studies* 51, 393-414.

Diamond, Douglas W., 1991, Monitoring and Reputation: The Choice Between Bank Loans and Directly Placed Debt, *Journal of Political Economy* 99(4), 689-721.

Drucker, Steven, and Manju Puri, 2009, On Loan Sales, Loan Contracting, and Lending Relationship, *Review of Financial Studies* 22(7), 2835-2872.

Dvorak, Brian, 2008, A Brief History of Active Credit Portfolio Management, *Technical Paper*, Moody's KMV.

Engelberg, Joseph, Pengjie Gao, and Christopher A. Parsons, 2009, The Value of a Rolodex: CEO Pay and Personal Networks, *Working Paper*, University of North Carolina and University of Notre Dame.

Ericsson, Jan and Elkamhi, Redouane, 2009, Time Varying Default Risk Premia in Corporate Bond Markets, Working Paper, AFFI-EUROFIDAI.

Elton, E., M.J. Gruber, D. Agrawal, and C. Mann, 2001, Explaining the Rate Spread on Corporate Bonds, *Journal of Finance* 56(1), 247 - 277.

Faulkender, Michael, and Mitchell A. Petersen, 2006, Does the Source of Capital Affect Capital Structure, *Review of Financial Studies* 19(1), 45-79.

Fama, Eugene, 1985, What's different about banks?, *Journal of Monetary Economics* 15, 29-39.

Fama, E., and K. French, 1989, Business Conditions and Expected Returns on Stocks and Bonds, *Journal of Financial Economics* 25, 23 - 49.

Ferreira, M., and P. Matos, 2009, Universal Banks and Corporate Control: Evidence from the Global Syndicated Loan Market, *Working Paper*, the University of Southern California.

Fleming, Michael J., 2003, Measuring Treasury Market Liquidity, FRBNY Economic Policy Review 2003(9), Federal Reserve Bank of New York.

Fracassi, C., 2008, Corporate Finance Policies and Social Networks, *Working Paper*, University of California at Los Angeles.

Fracassi, C., and G. Tate, 2009, External Networking and Internal Firm Governance, 2009, *Working Paper*, University of California at Los Angeles.

Gorton, Gary, and James A. Kahn, 1993, The Design of bank Loan Contracts, Collateral, and Renegotiation, *Working Paper* 4273, National Bureau of Economic Research, Boston, MA.

Güner, B., U. Malmendier, and G. Tate, 2008, Financial Expertise of Directors, *Journal of Financial Economics* 89(1), 20-43.

Gupta, Anurag, Ajai K. Singh, and Allan A. Zebedee, 2008, Liquidity in the pricing of syndicated loans, *Journal of Financial Markets* 11, 339-376.

Hochberg, Yael, Alexander Ljungqvist and Yang Lu, 2007, Whom you know matters: Venture capital networks and investment performance, *Journal of Finance* 62, 251-301.

Hoshi, Takeo, Anil Kashyap, and David Scharfstein, 1990, The role of banks in reducing the costs of financial distress in Japan, *Journal of Financial Economics* 27(1), 67 – 88.

Huang, Jing-Zhi and Huang, Ming, 2003, How Much of Corporate-Treasury Yield Spread Is Due to Credit Risk?: A New Calibration Approach, *Working Paper*, Penn State University and Cornell University.

Hwang, B. and S. Kim, 2009, It pays to have friends, *Journal of Financial Economics*, forthcoming.

Hovakimian, Armen G., Ayla Kayhan, and Sheridan Titman, 2009, Credit Rating Targets, *Working Paper*, Baruch College and University of Texas at Austin.

Kisgen, Darren J., and Philip E. Strahan, 2009, Do Regulation on Credit Ratings Affect A Firm's Cost of Capital, Working Paper 14890, National Bureau of Economic Research, Boston, MA.

Kroszner, Randall S., and Philip E. Strahan, 2001, Bankers on Boards: monitoring, conflicts of interest, and lender liability, *Journal of Financial Economics* 62(3), 415 – 452.

La Porta, Rafael, Florencio Lopez-de-Silanes, and Guillermo Zaparripa, 2003, Related Lending. *Quarterly Journal of Economics*, 118 (1), 231-268.

Laeven, Luc, 2001, Insider Lending and Bank Ownership: The Case of Russia, *Journal of Comparative Economics* 29 (2), 207-229.

Levy, Amonon, and Zhenya Hu, 2007, Incorporating Systematic Risk in Recovery: Theory and Evidence, *Technical Paper*, Moody's KMV.

Longstaff, Francis, Sanjay Mithal, Eric Neis, 2005, Corporate Yield Spreads: Default Risk or Liquity? New Evidence from the Credit-Default Swap Market, *Journal of Finance* 60, 2213-2253.

Modigliani, F., and M. Miller, 1958, The Cost of Capital, Corporation Finance and the Theory of Investment, *American Economic Review* 48(3): 261—297.

Morck, Randall, and Masao Nakamura, 1999, Banks and Corporate Control in Japan, *Journal of Finance* 54(1), 319-339.

Parsons, Christopher A. and Titman, Sheridan, 2009, Empirical Capital Structure: A Review, *Foundations and Trends in Finance* 3(1), 1-93.

Petersen, Mitchell A., and Raghuram G. Rajan, 1994, The Benefits of Lender Relationship: Evidence from Small Business Data, *Journal of Finance* 49 (1), 3 – 37.

Prowse, Stephen, 1992, The structure of corporate ownership in Japan, *Journal of Finance* 47, 1121–1140.

Qian, Jun, and Philip E. Strahan, 2007, How Laws and Institutions Shape Financial Contracts: The Case of bank Loans, *Journal of Finance* 62(6), 2803 – 2834.

Rajan, Raghuram G., 1992, Insiders and Outsiders: The Choice between Informed and Arm's-Length Debt, *Journal of Finance* 47(4), 1367-1400.

Rajan, Raghuram G., and Luigi Zingales, 1998, Which Capitalism? Lessons From the East Asian Crisis, *Journal of Applied Corporate Finance* 11(3), 1 – 11.

Roberts, Michael R., and Amir Sufi, 2009a, Control Rights and Capital Structure: An Empirical Investigation, *Journal of Finance* 64(4), 1657 – 1695.

Roberts, Michael R., and Amir Sufi, 2009b, Renegotiation of Financial Contracts: Evidence from Private Credit Agreements, *Journal of Financial Economics* 93, 159 – 184.

Ross, S., 1985, Debt and Taxes and Uncertainty, *Journal of Finance* 40, 637-58.

Schmidt, Breno, 2008, Costs and Benefits of “Friendly” Boards during Mergers and Acquisitions, *Working Paper*, University of Southern California.

Stein, Jeremy C., 2003. Agency, information and corporate investment, Handbook of the Economics of Finance, in: G.M. Constantinides & M. Harris & R. M. Stulz (ed.), Handbook of the Economics of Finance, edition 1, volume 1, chapter 2, pages 111-165.

Woolcock, Michael. 1998. “Social Capital and Economic Development: Toward a Theoretical Synthesis and Policy Framework” Theory and Society 27:151–208.

Yunus, Muhammad. 1993. “Reflections on the Essentials of Grameen System.” Going Forward, The Peer Group Lending Exchange. Conference Report and Conference Papers. November 1993, Hot Springs, Arkansas. Toronto, Canada: Calmeadow.

Figure 1: Credit Ratings Evolution for Connected and Unconnected Firms

This figure shows the evolution of long-term public debt ratings. Ratings for firms that complete loans with personally connected banking syndicates (at least one *School* or *Third-party Past Professional Connection*) are shown in gray; those of their counterparts borrowing from non-connected syndicates are shown in black. Initial credit ratings are those as of the loan's start date. Final ratings correspond to those as of July 2009. Firms with initial ratings above AA or below CCC are omitted due to a small number of observations.

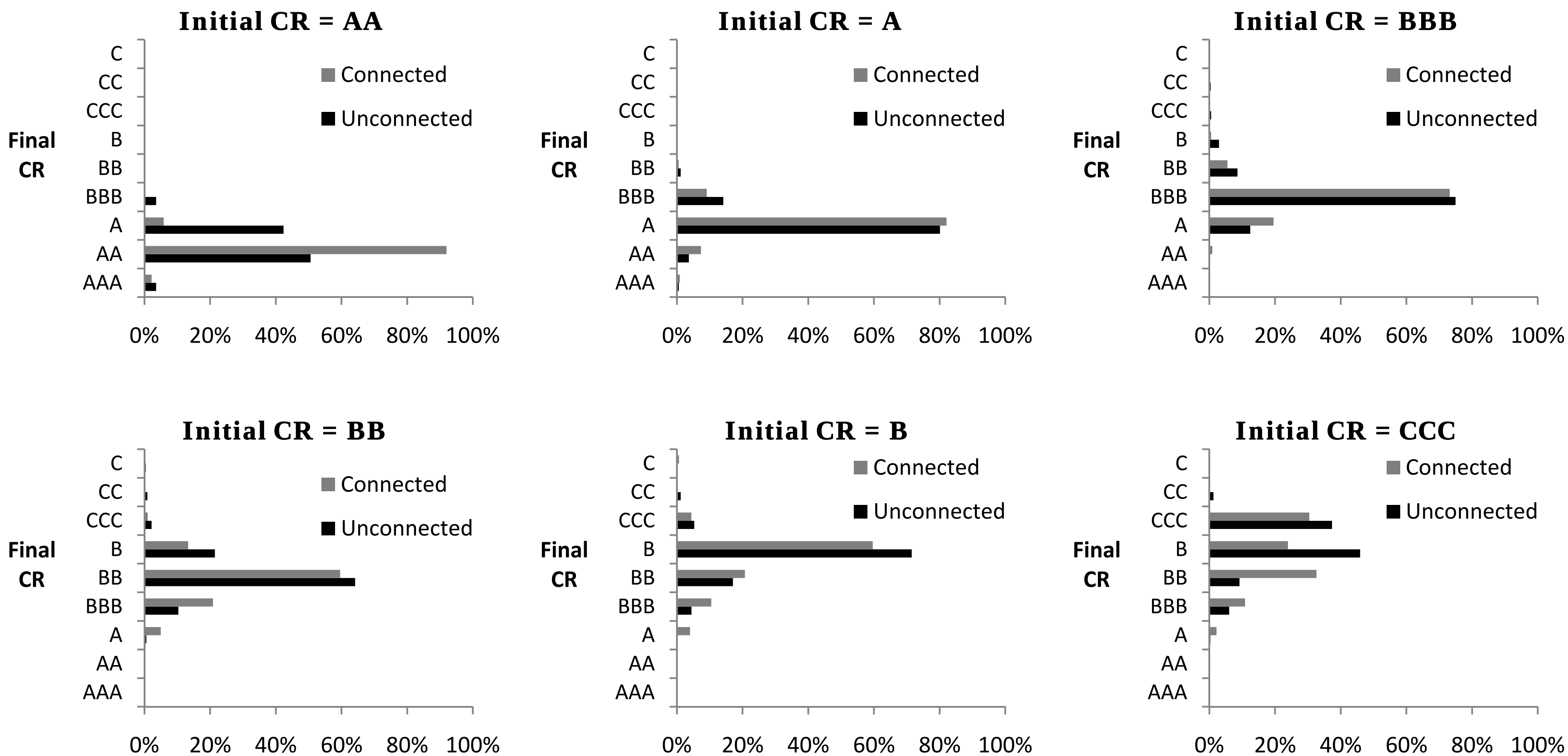

Table 1: Summary Statistics

Table 1 reports summary statistics for several variables used in the paper. Panel A reports data on syndicated loans, extracted from the *Dealscan* database. Variables shown are the *Dollar Value of Each Syndicated Loan* in millions of dollars, the *Total Number of Covenants*, the *All-in Drawn Spreads* in basis points, the *Number of Lenders*, and the *Number of Local Banks*. A lender is considered local if its headquarters is located within 100 kilometers from the headquarters of the borrower. Panel B reports summary statistics for our personal connections variables. *School Connections* is calculated by summing all instances in which a director/executive of the borrower and a director/executive of the lender attended: 1) the same educational institution, and 2) matriculated within two years of one another. *Third-party Past Professional Connections* are formed similarly, but with a common past employer. All professional connections are least five years removed from the date of any banking activity and do not include instances in which the connection was made at the lending bank or the borrowing firm. With *social connections*, we sum all instances in which a director/executive of the borrower and a director/executive of the lender have active roles in a common social organization, e.g., serving on the board of United Way. *Deal in the past 1-3 years, Deal in the past 4-6 years, and Deal in the past 7 years or earlier* are indicator variables taking a value of one if the current borrower borrowed from one or more members of the current syndicate in the most recent three years, the three years before that, or the three before that, respectively. Panel C reports the summary statistics for several borrower fundamentals, including one-year lagged *Total Assets* (in millions of dollars), *Market to Book* ratio, *Capital Expenditures* (normalized by lagged total assets), *Tangible Assets* (normalized by the lagged total assets), *Profitability* as of the most recent fiscal year end prior to the loan origination, *Expected Default Frequency* (EDF) at the end of the month prior to the loan origination, and *EDF Implied Spreads* at the end of the month prior to loan origination.

	Mean	Median	Std	10^{th}	90^{th}
Panel A: Deal characteristics					
Dollar Value of Each Syndicated Loan (in $M)	656	225	1,670	25	2,500
Total Number of Covenants	3.14	3.00	3.39	0.00	9.00
All-in Draw Spreads (in basis points)	206.48	187.50	146.95	40.00	375.00
Number of Lenders	7.50	5.00	8.42	1.00	17.00
Number of Local Banks	1.79	1.00	2.79	0.00	5.00
Panel B: Connection Measures					
Past School Connections Per Syndicated Loan	0.26	0.00	0.87	0.00	1.00
Third-Party Past Professional Connections Per Syndicated Loan	1.28	0.00	4.15	0.00	4.00
Current Social Connections Per Syndicated Loan	2.17	0.00	6.12	0.00	6.00
Deal in Past 1-3 Yrs Indicator	0.16	0.00	0.36	0.00	1.00
Deal in Past 4-6 Yrs Indicator	0.15	0.00	0.36	0.00	1.00
Deal in Past 7 or earlier Indicator	0.10	0.00	0.31	0.00	1.00
Panel C: Firm characteristics					
Total Assets (in $M)	13044.20	1217.82	65290.59	87.55	18954.20
Profitability	0.38	0.13	32.65	0.02	0.27
Tangibility	0.58	0.46	6.79	0.08	0.91
MA / BA	1.81	1.34	2.83	0.95	2.93
Capital Expenditure / Total Assets	0.08	0.04	0.28	0.00	0.15
EDF (in %)	2.65	0.44	5.26	0.03	8.62
EDF Implied Spreads (EIS, in %)	323.18	117.38	540.64	21.30	888.68

Table 2: Firm-Bank Personal Connections and Interest Rates

Table 2 relates the firm's borrowing cost, measured as its *All-in Drawn Spread*, to borrower/lender personal connections. Key control variables include a set of lender characteristics, loan characteristics, and macroeconomic conditions at time of loan origination. The *Connected Indicator* takes a value of one if there exists at least one *School connection* or *Third-party Past Professional Connection* between the borrower and any syndicate bank. *Deal in the past 1-3 years, Deal in the past 4-6 years, and Deal in the past 7 years or earlier* are indicator variables taking a value of one if the current borrower borrowed from one or more members of the current syndicate in the most recent three years, the three years before that, or the three before that, respectively. The set of loan characteristic control variables include the logarithm of time till *Maturity* (i.e., the tenor length in months) and the *Number of Lenders* in the loan syndicate. The set of syndicate characteristic control variables include the total number of syndicated loan transactions conducted by the participating banks in the prior year (*Number of Syndicated Loans [t-1]*), and the *Number of Local Banks* in the syndicate, where local is defined as within 100 kilometers from the headquarters of the borrower. The set of macro control variables include the levels and changes in default spread (the yield spread between BAA and AAA corporate bond indices), the level of and changes in term spreads (the yield spread between 10-year Treasury and 3-month Treasury), and the most recent monthly returns of the S&P 500 index. *Securitized* fixed effects indicate whether the loan is explicitly secured, whether it is unsecured, or whether this information is missing in *Dealscan*. Year, industry and firm fixed-effects are conventionally defined. We use Fama-French 30-industry classifications to define industry dummy variables. Column 1 examines all loans; columns 2, 3 and 4 examine high (A, AA, and AAA), medium (B, BB, and BBB) and low rating (CCC and below) loans, and column 5 examines loans of firms lacking public credit ratings. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: All-in Drawn Spreads				
	All Loans (1)	High Rating Loans (2)	Medium Rating Loans (3)	Low Rating Loans (4)	Unrated Loans (5)
Connected Indicator	-27.68***	-8.452**	-20.12***	-51.11**	-46.52***
	(2.720)	(3.373)	(3.366)	(20.88)	(5.808)
AAA Credit Rating	-173.8***	-6.951			
	(8.628)	(6.381)			
AA Credit Rating	-161.2***				
	(8.000)				
A Credit Rating	-144.1***	8.272			
	(6.006)	(5.458)			
BBB Credit Rating	-102.3***		-110.0***		
	(5.467)		(5.384)		
BB Credit Rating	-44.24***		-43.75***		
	(5.192)		(4.666)		
B Credit Rating	-3.582				
	(5.053)				
CCC Credit Rating	-35.98***				
	(4.664)				
CC Credit Rating	15.55			35.31	
	(12.75)			(25.14)	
C Credit Rating	1.563			36.96	
	(37.17)			(37.78)	
Log(Maturity)	1.564	1.463	-0.0325	32.18	2.340
	(5.596)	(3.012)	(8.268)	(51.12)	(10.16)
Deal in Past 1-3 Yrs Indicator	-13.30***	-0.651	-8.215**	-11.27	-19.37***
	(2.853)	(4.089)	(3.648)	(20.11)	(5.568)
Deal in Past 4-6 Yrs Indicator	-7.361**	3.399	-9.694***	-14.13	-0.378
	(2.967)	(4.947)	(3.266)	(24.91)	(7.024)
Deal in Past 7 or early Indicator	-6.773**	-3.151	-4.279	-40.18	-12.75*
	(3.043)	(2.503)	(3.983)	(26.98)	(6.845)
Number of Lenders	0.207	-0.231	0.0587	-0.191	0.319
	(0.164)	(0.192)	(0.178)	(0.742)	(0.447)
# of Loans Offered by Syndicate Prior Year	-0.0210***	-0.00483***	-0.0177***	-0.0322***	-0.0305***
	(0.00111)	(0.00179)	(0.00129)	(0.0102)	(0.00242)
Local Bank Indicator	0.535	0.298	0.252	0.965	1.207
	(0.470)	(0.475)	(0.535)	(3.838)	(1.302)
Macroeconomic Controls	YES	YES	YES	YES	YES
Seniority Fixed Effect	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES
Observations	17428	1705	8666	359	6698
Adjusted R^2	0.431	0.368	0.448	0.250	0.230

Table 3: Firm-Bank Connections and Loan Spreads

Table 3 relates the firm's borrowing cost to borrower/lender personal connections. Key control variables include a set of borrower financial fundamentals, lender characteristics, loan characteristics, and macroeconomic conditions at time of loan origination. The *Connected Indicator* takes a value of one if there exists at least one *School Connection* or *Third-party Past Professional Connection* between the borrower and any syndicate bank. The logarithm of this variable is self-explanatory. The dependent variables in regressions 1 and 2 are the *All-in Drawn Spreads* reported by *Dealscan*. The dependent variables in regressions 3 and 4 are the logarithm of the *All-in Drawn Spreads*. The set of borrower fundamental control variables include the *CAPM Beta* estimated using the past three-years of monthly returns (with a minimum of 18 monthly observations), logarithm of *Total Assets*, *Market to Book* ratio, *Capital Expenditures* (normalized by lagged total assets), *Tangible Assets* (normalized by the lagged total assets), and *Profitability* as of the most recent fiscal year end prior to the loan origination. The set of loan characteristic control variables include the logarithm of time till *Maturity* (i.e., the tenor length in months), and the *Number of Lenders* in the loan syndicate. The set of syndicate characteristic control variables include the total number of syndicated loan transactions conducted by participating banks in the prior year (*Number of Syndicated Loans [t-1]*), and the *Number of Local Banks* in the syndicate, where local is defined as within 100 kilometers from the headquarters of the borrower. The set of macro control variables include the levels and changes in default spread (the yield spread difference between BAA and AAA corporate bond indices), the level of and changes in term spread (the yield spread difference between 10-year Treasury and 3-month Treasury), and the most recent monthly returns of the S&P 500. *Securitized* fixed effects indicate whether the loan is explicitly secured, whether it is unsecured, or whether this information is missing in *Dealscan*. *EDF decile* fixed effects pertain to the set of dummy variables which take value of one if the borrower's monthly EDF value at time of loan origination falls into one of the ten *EDF* deciles. Year, industry and firm fixed-effects are conventionally defined. We use Fama-French 30-industry classifications to define industry dummy variables. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: All-in Drawn Spreads		Dependent Variable: log(All-in Drawn Spreads)	
	(1)	(2)	(3)	(4)
Connected Indicator	-17.77***	-17.29***		
	(3.431)	(3.704)		
Log (1+ Number of Connections)			-0.134***	-0.0483***
			(0.0146)	(0.0118)
CAPM Beta	0.470	0.169	0.0186**	0.00545
	(1.611)	(1.899)	(0.00797)	(0.00832)
Log(Total Assets)	-4.338***	-13.36**	-0.0687***	-0.0548**
	(1.621)	(5.415)	(0.0120)	(0.0221)
M/B	-1.517**	-3.704**	-0.0199***	-0.0231**
	(0.682)	(1.704)	(0.00637)	(0.0100)
Capital Expenditure / Total Assets	-1.448	29.89*	-0.0110	0.162**
	(15.65)	(17.87)	(0.0756)	(0.0678)
Tangible / Total Assets	-6.505	1.063	-0.0290	0.0213
	(4.387)	(5.087)	(0.0233)	(0.0251)
Profitability	-31.05***	-75.12***	-0.136**	-0.388***
	(8.608)	(17.01)	(0.0602)	(0.0750)
Log(Maturity)	11.04**	5.328	0.112***	0.0184
	(5.156)	(4.918)	(0.0280)	(0.0235)
Deal in Past 1-3 Yrs Indicator	-3.446	1.088	-0.0199	0.000936
	(3.344)	(3.313)	(0.0204)	(0.0189)
Deal in Past 4-6 Yrs Indicator	-3.087	0.510	0.0151	0.0204
	(3.158)	(3.141)	(0.0192)	(0.0162)
Deal in Past 7 or early Indicator	-8.134**	-9.701***	-0.0202	-0.0469**
	(3.412)	(3.363)	(0.0231)	(0.0200)
# of Loans Offered by Syndicate Prior Year	-0.0175***	-0.0121***	-8.82e-05***	-7.46e-05***
	(0.00150)	(0.00142)	(9.63e-06)	(7.71e-06)
Local Bank Indicator	2.149***	2.548***	0.00935**	0.0128***
	(0.618)	(0.679)	(0.00393)	(0.00330)
Number of Lenders	-0.200	-0.845***	0.00213	-0.00393**
	(0.300)	(0.289)	(0.00221)	(0.00157)
Macroeconomic Controls	YES	YES	YES	YES
Seniority Fixed Effect	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES
Industry Fixed Effect	YES	NO	YES	NO
Firm Fixed Effect	NO	YES	NO	YES
Observations	11003	11003	11003	11003
Adjusted R^2	0.504	0.745	0.615	0.860

Table 4: Firm-Bank Connections, Loan Covenants and Loan Sizes

Panel A of Table 4 relates the *Number of Covenant* restrictions of the loan to borrower/lender personal connections. Panel B considers as the dependent variable the natural logarithm of the *Loan Amount* (dollars). Control variables include a set of borrower financial fundamentals, lender characteristics, loan characteristics, and macroeconomic conditions at time of loan origination. The *Connected Indicator* takes a value of one if there exists at least one *School connection* or *Third-party Past Professional Connection* between the borrower and any syndicate bank. The logarithm of this variable is self-explanatory. The same set of firm, loan, lender, industry, and macro controls in Table 3 are employed here. The dependent variable in regressions 1 and 2 is a dummy variable that takes a value of one if the firm has any covenants listed in *Dealscan;* the dependent variable in columns 3 and 4 is the number of covenants. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

Panel A: Firm-Bank Connections, Loan Covenants

	Dependent Variable: Covenant Indicator		Dependent Variable: Number of Covenants	
	(1)	(2)	(3)	(4)
Connected Indicator		-0.0124		0.0715
		(0.0146)		(0.112)
Log (1+ Number of Connections)	-0.0226***		-0.112*	
	(0.00829)		(0.0634)	
Firm Characteristics Controls	YES	YES	YES	YES
Loan Characteristics Controls	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES
Seniority Fixed Effect	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	NO	NO
Firm Fixed Effect	NO	NO	YES	YES
Observations	11,964	11,964	11,964	11,964
Pseudo (1&2) or Adjusted (3&4) R^2	0.378	0.377	0.678	0.678

Panel B: Firm-Bank Connections and Loan Sizes

	log(Tranche Amount)	log(Tranche Amount)	log(Tranche Amount)	log(Tranche Amount)
	(1)	(2)	(3)	(4)
Connected Indicator		0.134***		0.147***
		(0.0318)		(0.0347)
Log (1+ Number of Connections)	0.0352*		0.107***	
	(0.0205)		(0.0208)	
Firm Characteristics Controls	YES	YES	YES	YES
Loan Characteristics Controls	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES
Seniority Fixed Effect	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	NO	NO
Firm Fixed Effect	NO	NO	YES	YES
Observations	11964	11964	11964	11964
Adjusted R^2	0.652	0.653	0.812	0.812

Table 5: Firm-Bank Connections and Future Credit Rating Downgrades

Table 5 reports the marginal effects of the borrower/lender personal connections on future credit rating changes at different horizons. The same standard set of firm, loan, industry, and macro controls in Table 3 are employed here. The dependent variables are indicators for whether the firm experienced a downgrade in its long-term S&P credit rating over various horizons after completing a syndicated loan. The initial credit rating is the borrower's credit rating when the syndicated deal was completed. Marginal effects from Probit regressions are shown. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Credit Rating Downgrade: Future 12 months		Credit Rating Downgrade: Future 24 months		Credit Rating Downgrade: Future 36 months	
	1	2	3	4	5	6
Connected Indicator	-0.0226***		-0.0561***		-0.0724***	
	(0.00753)		(0.0113)		(0.0145)	
Log (1+ Number of Connections)		-0.0104***		-0.0142**		-0.0186**
		(0.00400)		(0.00586)		(0.00755)
Firm Characteristics Controls	YES	YES	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	YES	YES	YES	YES
Observations	5,758	5,758	5,154	5,154	4,255	4,255
Pseudo R^2	0.089	0.089	0.106	0.101	0.122	0.117

Table 6: Connections and Alterative Measures of Future Credit Risk

Table 6 relates future *Expected Default Frequencies* (Panel A) and *Expected Default Frequency Implied Spreads* (Panel B) to borrower/lender past connections, a set of borrower fundamentals, lender characteristics, and macroeconomic conditions at the time of loan origination. The set of control variables is the same as those reported in Table 3. The number of connections describes the sum of *School* and *Third-Party Past Professional Connections*. The reference date is that when the syndicated deal is initiated. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

Panel A: Connections and Firm's Future Expected Default Frequencies (EDF)

	Dependent Variable: Expected Default Frequencies (EDFs)					
	EDF, 12 month-ahead		EDF, 24 month-ahead		EDF, 36 month-ahead	
Connected Indicator	-0.427***		-0.741***		-0.734***	
	(0.104)		(0.177)		(0.211)	
Log (1+ Number of Connections)		-0.140**		-0.215*		-0.311**
		(0.0599)		(0.124)		(0.135)
Current EDF	0.636***	0.637***	0.366***	0.368***	0.228**	0.229**
	(0.0659)	(0.0659)	(0.0784)	(0.0784)	(0.0898)	(0.0897)
Firm Characteristics Controls	YES	YES	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	YES	YES	YES	YES
Observations	9082	9082	8192	8192	6819	6819
Adjusted R^2	0.527	0.526	0.293	0.291	0.215	0.213

Panel B: Connections and Firm's Future EDF Implied Spreads (EIS)

	Dependent Variable: EDF Implied Spreads (EIS)					
	EIS, 12 month-ahead		EIS, 24 month-ahead		EIS, 36 month-ahead	
Connected Indicator	-49.27***		-77.23***		-80.39***	
	(11.43)		(19.49)		(24.36)	
Log (1+ Number of Connections)		-18.96***		-22.24		-34.97**
		(6.577)		(14.20)		(15.49)
Current EDF Implied Spreads (EIS)	0.525***	0.527***	0.357***	0.359***	0.203***	0.203***
	(0.0572)	(0.0572)	(0.0612)	(0.0612)	(0.0631)	(0.0629)
Firm Characteristics Controls	YES	YES	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	YES	YES	YES	YES
Observations	9071	9071	8181	8181	6804	6804
Adjusted R^2	0.519	0.518	0.333	0.332	0.256	0.254

Table 7: Connections and Future Stock Returns

Table 7 relates future stock returns of the borrower to borrower-lender personal connections, a set of borrower financial fundamentals, lender characteristics and macroeconomic conditions at time of loan origination. In Panel A and Panel B, the dependent variable is the cumulative Daniel et al. (1997) characteristic-adjusted returns 12, 24 and 36 months after loan origination. In Panel C, the dependent variable takes one if there is the cumulative risk-adjusted return since loan origination of -50% or less. The set of control is the same as those reported in Table 3. The number of connections describes the sum of *School* and *Third-Party Past Professional Connections*. The reference date is that when the syndicated deal is initiated. Panel A shows the results of time-series cross-sectional regressions; Panel B shows the results of (monthly) Fama-MacBeth regressions; Panel C shows marginal effects from Probit estimations. Robust standard errors clustered by firm (in Panel A and Panel C) and Fama-MacBeth standard errors are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

Panel A: Connections and Firm's Future Cumulative Returns, Time-Series Cross-Sectional Regressions

	Dependent Variable: Return at Different Horizons					
	Return, 12-Month ahead		Return, 24-Month ahead		Return, 36-Month ahead	
Connected Indicator	0.0344*		0.106***		0.170***	
	(0.0189)		(0.0312)		(0.0430)	
Log (1+ Number of Connections)		0.0164*		0.0499***		0.0743***
		(0.00886)		(0.0149)		(0.0206)
Firm Characteristics Controls	YES	YES	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	YES	YES	YES	YES
Observations	9,113	9,113	9,113	9,113	9,113	9,113
Adjusted R^2	0.025	0.025	0.037	0.036	0.051	0.049

Panel B: Connections and Firm's Future Cumulative Returns, Fama-MacBeth Regressions

	Dependent Variable: Return at Different Horizons					
	Return, 12-Month ahead		Return, 24-Month ahead		Return, 36-Month ahead	
Connected Indicator	0.0491**		0.1243***		0.2107***	
	(0.0239)		(0.0302)		(0.0519)	
Log (1+ Number of Connections)		0.0280***		0.0823***		0.1168***
		(0.0104)		(0.0152)		(0.0248)
Firm Characteristics Controls	YES	YES	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES	YES	YES
Observations	9,113	9,113	9,113	9,113	9,113	9,113
Average Cross-Sectional R^2	0.076	0.067	0.055	0.051	0.059	0.050

Panel C: Connections and Firm's Future Extreme Low Return

	Dependent Variable: Extreme Low Returns					
	Extreme Low Return 12-Month ahead		Extreme Low Return 24-Month ahead		Extreme Low Return 36-Month ahead	
Connected Indicator	-0.0147**		-0.0311***		-0.0496***	
	(0.00671)		(0.0107)		(0.0131)	
Log (1+ Number of Connections)		-0.00969*		-0.0142**		-0.0326***
		(0.00543)		(0.00713)		(0.00839)
Firm Characteristics Controls	YES	YES	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	YES	YES	YES	YES
Observations	9,113	9,113	9,113	9,113	9,113	9,113
Pseudo R^2	0.175	0.175	0.117	0.118	0.089	0.089

Table 8: Loan Spreads and Alternative Definitions of Connections

Table 8 relates *All-in Drawn Spreads* to borrower-lender personal connections, a set of controls for borrower fundamentals, lender characteristics, loan characteristics, and macroeconomic conditions at time of loan origination, as well as a set of specified fixed-effects. In regression 1, the dependent variable is numerical *All-in Drawn Spreads*; in regressions 2 to 4, the dependent variable is its natural logarithm. In column 3, we exclude all observations involving "busy" syndicates, those that ranked in the Top 5 in terms of loan volume the previous year. In column 4, we aggregate all observations, but include indicator variables for every bank in the Top 20 ranked by previous year deal volume. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: All-in Drawn Spreads	Dependent Variable: Log(All-in Drawn Spreads)		
	(1)	(2)	(3)	(4)
School Connection Indicator	-9.152***			
	(2.988)			
Third-Party Past Professional Connection Indicator	-8.723**			
	(3.415)			
Social Connection Indicator	-13.92***			
	(3.411)			
Log (1 + Number of School Connections)		-0.0699**		
		(0.0295)		
Log (1 + Number of Professional Connections)		-0.128***		
		(0.0161)		
Log (1 + Number of Social Connections)		-0.0410***		
		(0.0144)		
Log (1 + Number of Connections)			-0.126***	-0.128***
			(0.0363)	(0.0140)
Firm Characteristics Controls	YES	YES	YES	YES
Loan Characteristics Controls	YES	YES	YES	YES
Bank Characteristics Controls	YES	YES	YES	YES
Macroeconomic Controls	YES	YES	YES	YES
Seniority Fixed Effect	YES	YES	YES	YES
EDF Decile Fixed Effect	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	YES	YES
Firm Characteristics Controls	YES	YES	YES	YES
Top-20 Bank Fixed Effect	NO	NO	NO	YES
Observations	11,003	11,003	3,948	11,003
Adjusted R^2	0.506	0.622	0.457	0.639

Table 9: Firm-Bank Matching

Table 9 considers possible matches between all firms in Dealscan that borrowed in year t and the 300 most active banks in year t-1. The Connected Indicator takes a value of one if there exists at least one *School Connection* or *Third-Party Past Professional Connection* between the firm and bank. The logarithm of this variable is self-explanatory. Firm-Bank location control is a dummy that takes the value of 1 if the headquarters of the firm and the bank are within 100 kilometers. Firm-Bank Deal History Controls are a series of dummy variables which take the value one if the firm and bank did a deal in the past (see Table 2). Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable:					
	DEAL INDICATOR			LOG(1+NUMBER OF DEALS)		
Connected Indicator	0.0321**	0.0203***	0.0103**			
	(0.0150)	(0.00445)	(0.00441)			
Log (1+ Number of Connections)				0.0486**	0.0278***	0.0191***
				(0.0194)	(0.00638)	(0.00690)
Firm-Bank Deal History Controls	YES	YES	YES	YES	YES	YES
Firm-Bank Location Control	YES	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES	YES
Bank Fixed Effect	NO	YES	YES	NO	YES	YES
Firm Fixed Effect	NO	NO	YES	NO	NO	YES
Observations	1,517,279	1,517,279	1,517,279	1,517,279	1,517,279	1,517,279
Pseudo R^2	0.130	0.163	0.188	0.105	0.135	0.156

Appendix A: Variable Definitions and Construction

Variable Name	Variable Definitions and Constructions	Source of Data
All-in Drawn Spreads	All-in drawn spreads of each tranche	Dealscan
Log(Maturity)	Logarithm of tenor length	Dealscan
Number of Lenders	Number of lenders within each syndicate	Dealscan
Number of Loans Offered by Syndicate Prior Year	The total number of non-overlapping loans offered by syndicate members during the prior year	Dealscan
Seniority Fixed Effect	Dummy variable that takes the value of one if the loan is a senior loan, and zero otherwise	Dealscan
Deal in Past 1-3 Yrs Indicator	Dummy variable that takes the value of one if the firm borrows from a bank in the syndicate during the prior three years	Dealscan
Deal in Past 4-6 Yrs Indicator	Dummy variable that takes the value of one if the firm borrows from a bank in the syndicate during the four to six years before the current year	Dealscan
Deal in Past 7 or early Indicator	Dummy variable that takes the value of one if the firm borrows from a bank in the syndicate more than six years before the current year	Dealscan
Local Bank Indicator	Dummy variable that takes the value of one if one of the syndicate member banks is located within 100 km of the borrower's headquarters and zero otherwise	Hand-collected
M/B	Market value of equity / book value of equity	CRSP/Compustat
Log(Total Assets)	logarithm of Total Assets (AT) at (t)	Compustat
Capital Expenditure / Total Assets	Capital Expense (t) / Total Assets (t-1)	Compustat
Tangibility / Total Assets	(PP&E + Inventory) (t) / Total Assets (t-1)	Compustat
Profitability	Operating Income Before Depreciation (t) / Total Assets (t-1)	Compustat

Industry Fixed Effect	Industry fixed effect, where the industry classification is defined by Fama-French (1997) 30-industry classifications	CRSP
Characteristics-Adjusted Return, 12-month ahead	The cumulative DGTW characteristic-adjusted return 12-months ahead, beginning at the month immediately after the deal.	CRSP/Compustat
Characteristics-Adjusted Return, 24-month ahead	The cumulative DGTW characteristic-adjusted return 24-months ahead, beginning at the month immediately after the deal.	CRSP/Compustat
Characteristics-Adjusted Return, 24-month ahead	The cumulative DGTW characteristic-adjusted return 36-months ahead, beginning at the month immediately after the deal.	CRSP/Compustat
CAPM Beta	Beta estimate from the the Capital Asset Pricing Model (CAPM), using the past 36 months of monthly returns, with a minimum of 18 months of return data	CRSP
Idiosyncratic Volatility	Residual standard deviation of the estimate from the Capital Asset Pricing Model (CAPM), using the past 36 months of monthly returns, with a minimum of 18 months of return data	CRSP
Expected Default Frequencies (EDF®)	The expected default frequency computed and calibrated to actual default events by Moody's KMV. See Crosbie and Bohn (2003) for details.	Moody's-KMV
Expected Default Frequencies Implied Spreads (EIS®)	The expected default frequency implied credit spreads is the product of the estimated expected default frequency and the estimated expected loss given default (LGD).	Moody's-KMV
EDF Decile Fixed Effect	Dummy variable that equals one if the EDF value falls into one of the ten EDF deciles, where EDF deciles are defined over the cross-sectional EDF values within the month	Moody's-KMV
Return [t-1, 0]	Cumulative past 12-month raw return	CRSP
Return [t-3, t-2]	Cumulative past 36-month raw return excluding the most recent 12-month return	CRSP

Level of Term Spread	The difference between the 10-year treasury yield and the 3-month treasury yield	Federal Reserve
Change of Term Spread	The change of term spreads between current month and prior month	Federal Reserve
Default Spread	The difference between Moody's BAA corporate bond index yield and Moody's AAA corporate bond index yield	Federal Reserve
Change of Default Spread	The change of default spread between current month and prior month	Federal Reserve