Friends with Money*

Joseph Engelberg

Pengjie Gao

Christopher A. Parsons‡

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Abstract: We explore whether personal connections between employees at firms and banks influence lending and borrowing practices. Such firm-bank connections predict large concessions in interest rates, comparable to single shifts in credit ratings. Personal relationships also predict larger loan amounts and fewer restrictive covenants. We find no evidence that these terms reflect "sweetheart deals." Subsequent firm performance (e.g., future credit ratings and stock returns) *improves* after completing a "connected" bank deal, suggesting social networks between banks and firms either lead to better information flow *ex ante* or better monitoring *ex post.*

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‡ 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 the 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, such relationships may catalyze information flow or reduce monitoring costs, placing the connected bank at an advantage relative to competing lenders. Here, both parties would 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 connections 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 from 2000 through 2007. The set of borrowers involves over 5,000 public firms, and the set of lenders over 1,900 commercial banks. Next, we request from BoardEx 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: Does such a personal relationship influence lending terms?

Establishing a causal relation requires a careful account fof 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 it allows us to identify connections that predate, 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 both received MBAs from Stanford in 1974. 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 (e.g., that personal connections are somehow a proxy for firm risk).

In pooled cross-sectional regressions of interest rates charged by syndicates, we find that the presence of a personal connection between the firm and lender – those 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 cost, but also because adverse selection and monitoring problems are most severe in these situations. 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.

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. It likewise suggests that banking connections are portable, moving with CEOs and bankers as they migrate across firms, potentially causing firms to compete over them (more on this below).

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 types 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 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 considerable insight by examining the evolution of each borrower's credit rating subsequent to initiating a bank deal. Although not

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

specifically related to a given transaction, this summary statistic measures 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 way of measuring a firm's likelihood of defaulting on more senior claims, such as syndicated bank loans.

We consistently find that personally connected borrowers' long-term credit ratings improve compared to their un-connected counterpart borrowers. As a typical example, of the 1,290 BB-rated firms that initiated syndicated bank deals with at least one connected bank, 63% maintained the same credit rating in the years immediately following, 22% improved and 15% worsened. In contrast, the comparable distribution for the 1,880 BB-rated firms completing deals with unconnected banks was 64%, 11%, and 25%. This identical pattern holds across *every* single 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 *ex post* stock returns confirms that such improvements were not foreseen *ex ante* 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 three, ten, and eighteen percent; in other words, firms completing deals to connected banks experience substantially higher stock returns than those borrowing from unconnected syndicates. Fama-McBeth (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.[2] The vast majority of banks in our data set are private, and therefore do not lend themselves to an analysis of *ex post* performance. However, such an analysis would be equally interesting, and would shed light on

[2] Of course, stock returns are only predictable presumably because firm-bank personal connections are difficult to observe directly; as this information becomes more transparent (for example, the formation of the current dataset), we would expect significant announcement return, and little *ex post* predictability.

whether the higher interest rates charged of non-connected borrowers is sufficient compensation for the adverse selection costs they bear.

The credit spread, credit ratings, and stock return evidence together suggests an intuitive story – a firm's managers and directors have time varying, private information about future fundamentals, and personal connections allow this information to be credibly conveyed to lenders. If so, then from the perspective of the firm, a CEO's personal connections to lenders are valuable assets. In our final tests, we explore whether CEOs endowed with large numbers of personal connections to bankers command a wage premium. They do. We find that in cross-sectional regressions that employ the usual pay controls, each connection to a banker increases the CEO's compensation by over 36 percentage points, or about $21,000 on average. These are not artifacts of size or other firm characteristics; even with firm fixed effects, we find that more "financially connected" CEOs are better paid, and more so in years directly before large debt issues.

To our knowledge, our analysis of personal connections and the lending market is novel, both in how it predicts lending terms and *ex post* performance. At a broad level, because personal connections are among the strongest predictors of borrowing costs, our results are directly relevant for understanding cross-sectional differences in firms' costs of capital, but also bear relevance 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. At a more narrow level, the evidence pinpoints a *specific technology* that allows banks – 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)).

Our study also represents, by far, the largest and most comprehensive study in a growing literature exploring the impact of personal networks on information flow and corporate 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.

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.[3] 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.

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 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 performance. We consider robustness and some extensions to our basic results in Section V, and then conclude.

[3] 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.

II. Data and Identification Strategy

We aggregate data from a number of sources. Our analysis involves the initiation of bank loans to large 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.[4]

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.[5] 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 interest rate charged (the “all-in drawn spread”), covenant variables and deal size, which we analyze as functions of the pre-existing personal connections between the 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.

Our sample period spans the years 2000-2007 inclusive. For these years, we are able to match executives and directors from both syndicate banks and borrowing firms to the BoardEx database, made available to us by Management Diagnostic Limited after a custom data request. 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

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

[5] 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.

(2008)). The network of social connections we are able to infer involves 5,057 firms, 1,924 commercial banks, and 65,074 different individuals (either directors or executives at their respective institutions). By several orders of magnitude, these data comprise the largest and most comprehensive existing set of personal connections in any comparable study.

Generally, establishing a causal relation between firm-bank personal connections and lending behavior is challenging because such personal relationships exist for reasons, and these reasons may independently influence the banking transactions being analyzed. Specifically, the decision to forge or maintain a relationship may be correlated with the firm's risk, fundamentals, reputation, or other imperfectly measured attributes relevant for lending terms. One can also envision that causation may run in the opposite direction, with directorships or invitations to social organizations awarded to the most accommodating banker. Guner, Malmendier, and Tate (2008) grapple with this explicitly, "financing needs may determine the board representation of financial institutions (p. 325)," making it imperative to either adequately control for the firm's existing financial needs, or to exploit exogenous variation in the appointment of directors.[6] Krozner and Strahan (2001) also explore how such potentially conflicted directors may distort lending decisions, and consequently face a similar identification problem.

Fortunately, the BoardEx database allows us to identify personal connections that predate the dependent variables of interest by several years, and often decades. In particular, BoardEx provides the necessary ingredients for us to define: 1) *social* connections formed when two people are simultaneously active members in organizations such as charities, school boards, etc., 2) *past professional* connections formed when two people shared a place of employment, at least five years in the past (*t-5* or prior), and 3) *school* connections formed when two people graduate from same educational institution within two years of one another.

[6] These authors instrument for the appointment of financial experts on board using the banking crisis between 1976 and 1985. The idea is that during this time, commercial bankers were not attractive as board members. This is a valid instrument (the exclusion restriction) as long as the crisis did not have an impact on the financing needs of firms that, otherwise, would have appointed a financial expert.

We limit our analysis entirely to the second and third types, so as to minimize the potential for either reverse causality, or for bias stemming from omitted variables. Formally, our identification strategy requires only the assumption that there are no unobserved factors that determine both: 1) borrowing and lending interactions between firm *i* and bank *j* in year *t*, and 2) personal connections between the management of firm *i* and bank *j* at least **five years** prior, i.e., *t-5* or before. In other words, when attempting to predict the lending terms between Pepsi and a syndicate involving Wachovia in 2004, we take as exogenous that their respective CFOs attended the same university some 30 years prior. By construction, this convention either eliminates or greatly mitigates the ability for non-causal alternative interpretations to explain the empirical patterns we later document.

In Panel B of Table 1, we list summary statistics for all three possible types of connections: *school*, *past professional*, and *social*. Because the latter type is potentially subject to the endogeneity critique described above, we ignore them for all but the robustness checks we present in Table 8. The connection measures are calculated at the syndicate level; for example, the mean value of *Past Professional Connections* is 2.02, 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.

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 extracted from CRSP. Panel C of Table 1 shows characteristic risk-adjusted (Daniel et al. (1997)) stock returns at the 12-, 24-, and 36-month interval. In each of these cases, the starting date corresponds to the initiation of a syndicated bank deal. Our distribution of credit ratings (not reported) is standard, with a modal value just below (BB) 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 are summary statistics for two proprietary credit risk measures made available to us from the Moody's-KMV: *Expected Default Frequencies (EDFs)* and *EDF Implied Spreads (EISs).*[7] 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 protective covenants.

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. Treasuries 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

[7] Interested readers can consult Bohn and Crosby (2003) for an overview of methodology behind the EDF, Agrawal, Arora and Bohn (2004) for an overview of the methodology behind EISs, and Dvorak (2008) for discussion of the adoption of these credit risk measures in practice.

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, these take three forms: 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, and 3) *default losses* – should a firm default on its debt, lenders can anticipate only partial recovery of principal and interest. [8,9]

Clearly, connections between lenders and borrowers are unlikely to affect tax statutes, but may affect either of the other two 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., news about an upcoming patent becoming known by competitors). Similarly, it is possible is 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

[8] According to the 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 the syndicated loans traded on the secondary market were about 50 to 65 basis points from January, 2002 to July, 2007. Since the subprime crisis, the spreads reached 136 basis points by December, 2007, and widened to 325 basis points after filing of bankruptcy by the Lehman Brothers. In sharp contrast, the average bid-ask spread for Treasuries are usually less than 1 basis point (Fleming (2003)). By any measure, liquidity in the secondary market for syndicated loans is low.

[9] Although true that 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 does not vary too much with default risk. Almeida and Philippon's (2007) summary of this evidence (Table II) justifies their use of a constant 51 basis points as the liquidity and tax adjustment. For more detailed discussion of the decomposition of the credit spread, see Elton, Gruber, Deepak, and Mann (2001), Huang and Huang (2003), Chen, Lesmond, and Wei (2007), and Ericsson and Elkhami (2009).

deals, or to actually make deals better, personal connections have the possibility to reduce 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 infer firm-syndicate personal relationship data for almost 20,000 deals, although this number is cut 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 BoardEx, at least one personal connection (common schooling or past workplace) between the borrowing firm and a syndicate bank exists among 5,729 deals (29%). In such cases, the average (median) credit spread is 127 (88 bp). In the remaining 13,825 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). For comparison, consider the coefficients on the rating indicators, summary statistics intended to gauge the firm's ability to service its debt obligations. As expected, credit spreads are strongly related to spreads (unrated firms are the omitted category), capturing cross-sectional differences in the neighborhood of 175-200 bp between the least risky (AAA) and most risky (C) firms.[10] Although substantial, this is considerably smaller than observed in the market for corporate bonds, explained at least in part by the seniority of bank debt.[11] As indicated, a spread concession of 28 basis points is comparable to a firm improving its rating from A to AAA (174-144=30 bp), or two-thirds of the transition following an upgrade from BBB to A (144-102=42 bp). By any measure, this represents a substantial effect.

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. For example, 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. However, 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 them, we include dummy variables for whether the borrower has transacted with at

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[10] There is a notable drop in spreads between credit ratings A and BBB. The latter corresponds to the investment grade threshold, a common requirement for institutional investors. Several important investor groups are restricted from holding non-investment grade debt securities, which can include corporate bonds and syndicated loans. Commercial banks could not invest in non-investment grade securities since the ruling by the U.S. Treasury Department in 1936. The Financial Institutions Reform, Recovery, and Enforcement Act (FIRREA) of 1989 stipulated that the Savings and Loans were prohibited from holding non-investment grade bonds by 1994. Additionally, some important bond market participants face extra costs holding non-investment grade bonds, e.g., the incentives of insurance companies to match maturities and risk of liabilities and assets. See Kisgen and Strahan (2009) for a summary of the historical development of regulations on credit rating for major bond market participants.

[11] For comparison, we calculate corporate bond spreads using TRACE corporate bond trading data from January 2003 through December 2007. Average yields range from 4.71% for the highest rated bonds (AAA/AA) to 9.56% for bonds rated B and below.

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., the first column indicates that previous banking relationships are in fact associated with lower spreads, and intuitively, that this declines as the relationship becomes stale. However, even the largest banking relationship indicator has a magnitude (-13 bp) less than half that for the firm-bank personal relationship indicator.[12]

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 spreads of their lenders.[13] 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 the respective zip codes and, when available, calculate the distance between their headquarters. If less than or equal to 100 km, we include a *Local Bank* indicator. 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 lender. To make sure that firm-bank 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 variables to describe the macroeconomic conditions that are measured at higher frequency. Motivated by Fama and French (1989) and Collin-Dufresne,

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[12] As an alternative to including indicators for previous banking relationships in the regressions, 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.

[13] 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.

Goldstein, and Martin (2001), we include the following five variables: the *level of term spread* (the difference between 10-year treasury yield and 3-month treasury yield), the *one-year change of term spreads*, the *default spread* (the difference between the Moody's BAA corporate bond index yield and Moody's AAA corporate bond index yield), the *one-year change of default spreads*, and the *one-year value-weighted 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 columns 2, which indicates that on average, personally connected deals are perceived by syndicates as being considerably less risky. The point estimate on the *personal connections indicator* is -8.4 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 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 relationships present. The fourth column contains only 359 observations, but because the magnitude on the relationship indicator is so high (-50 bp), it nevertheless yields a statistically significant estimate for this sample. Perhaps the most obvious takeaway from Table 2 is that firm-bank relationships are a robust determinant of borrowing costs, but most so for firms with poor credit.

The final two columns show the results for the 45% of firms lacking a public credit rating at the time the syndicated deal is initiated. Interestingly, the personal relationship effects for these unrated firms are comparable to those observed with the sample of low credit rating firms (particularly those with CCC credit or worse), with a magnitude of -48 bp. Because we know relatively little about the credit characteristics of these firms, we do not emphasize the size of 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. In particular, firms with particularly 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 plausible reconciliation for the results in column five.

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 the credit spread is determined (in part) by the syndicate's anticipation of incomplete recovery in default, factors that influence recoveries are also likely to be priced into spreads. To appreciate this, contrast a consulting firm whose value is mostly derived from relationships, with a public utility consisting mostly of hard assets such as turbines and generators. In liquidation, the former is likely to be worthless without the cooperation of management, exposing the firm's creditors to the type of hold-up problems described by Hart and Moore (1994). The utility's creditors, on the other hand, can expect moderate or even high recoveries (Altman and Kishore (1996); Acharya, Bharath, and Srinivasan (2007)), depending on the resale market for turbines.[14]

To address arguments of this type, in Table 3 we augment the specification with a number of firm and industry-specific control variables likely to affect asset recoveries in liquidation. Unfortunately, systematic data on debt recoveries is sparse, although evidence such as Altman and Kishore (1996) and Acharya, Bharath, and Srinivasan (2007) articulates perhaps the best argument that recoveries vary by industry and industry business conditions. As a

[14] Shleifer and Vishny (1992) describe an equilibrium in which the financial position of one's competitors influences the liquidation values of assets difficult to redeploy outside of the industry.

conrol, we include dummy variables for each of the Fama and French 30 industry classifications, which is mostly responsible for the increase in explanatory power seen in the first column.

In addition, we 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). Arguments that these attributes proxy for liquidation values - e.g., the growth options of firms with high market-to-book are likely to be impaired during bankruptcy – enjoy a rich tradition in capital structure research.[15] Finally, given that financial distress is "more likely to occur in bad times" (Almeida and Philippon (2007), p. 2557), and that such systematic risk should be priced (Ross (1985)), we include each firm's asset beta (extracted from a monthly time-series regression of its stock returns, and then de-levered). If creditors account for the expected correlation of default losses with the aggregate market, we should expect a positive coefficient.

Requiring data availability for all of these variables substantially reduces the size of our sample, to just over eleven thousand firms. Because credit ratings are so important for predicting credit 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.

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 (-19 bp).[16] 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

[15] See Parsons and Titman (2009) for a review of empirical capital structure.

[16] We have verified that the reduction in the magnitude on the firm-bank *personal connections* indicator (from -28 to -19 bp) is primarily due to the changing of the sample, rather than to the addition of the control variables. Given that firms without COMPUSTAT data are more likely to be young, small, growth firms with potentially the greatest information asymmetries, this difference is perhaps expected.

firm-level variables either are, or border on being, statistically significant, with perhaps size (lagged assets) and market-to-book ratios having the most predictive power.

The second column shows the results of the same specification, but includes firm fixed effects. Given that we rely exclusively on connections that are formed several years prior to the date of the banking transaction, within-firm variation occurs only when: 1) the same firm conducts multiple deals over our sample period, and 2) with different syndicate banks so as to provide variation in the number of personal firm-bank personal connections. Although the set of borrowing firms is reduced to those firms conducting multiple deals with differing syndicates (single-time borrowers are absorbed completely by the fixed effects), the significant, negative coefficient show that the impact of personal connections is nevertheless identified purely from within-firm variation. In fact, the magnitude is virtually identical with and without firm fixed effects (-18 bp vs. -19 bp), and remains highly significant ($p<0.001$).

The fixed effects evidence is particularly compelling because, despite our attempts to control for the probability of and losses given default in columns 1, the increase in R^2 makes clear that time-invariant, latent firm characteristics play an important role in lenders' risk assessments. This specification allows us to implicitly control for these unobserved risk sources, and therefore allows us to more precisely identify the effects of firm-bank personal connections. In unreported results, we also have run models with firm-year fixed effects, and although the estimable sample is reduced to only a few hundred observations (those in which a firm completes multiple syndicated deals with different partners in the *same year*), the point estimates on personal connections is similar to that found in the first two columns of Table 3.

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 roughly 12 percent less. On average, this means that two additional connections (the mean of this variable) is associated with a spread reduction of approximately 179*0.12 = 21.5 bp, similar to the coefficient on the indicator variables in the first two columns. Interestingly, this specification also indicates a positive and significant coefficient on *Beta*; the predicted (positive) sign consistent with creditors pricing the systematic risk inherent in default. The final column that although including firm fixed effects substantially decreases the magnitude of the spread-connection elasticity (point estimate of -0.036), it remains highly significant ($p<0.001$).

Before proceeding to the next tests, 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 form 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.

Anticipating later evidence, we simply remark that this result is what one would expect if personal connections were used to resolve information asymmetry. For example, consider a situation in which the syndicate charges an interest rate based on the firm's profitability – the specific value of which is unknown, but known to be normally distributed. Suppose further that each bank personally connected to the firm's management receive an independent, unbiased, but noisy signal of the firm's profitability with some non-zero precision, but that banks not personally connected receive nothing. In such a setting, the textbook formula for Bayesian updating of a normally distributed variable indicates that each additional connected bank will still be valued (the syndicate's posterior variance of the firm's profitability will shrink with each additional signal it receives), but progressively less so as the number of signals increases.

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. Essentially, covenants are provisions in a debt contract that specify conditions that define "technical default." Even if a firm has not missed an interest or principal payment, violating 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 consequences of covenant violations, both as far as they relate to the intervention of creditors (Chava and Roberts, 2008; Nini, Smith, and Sufi, 2009; Roberts and Sufi, 2009a) as well as to renegotiation between borrowers and creditors (Roberts and Sufi, 2009b). Given such evidence, a possible reconciliation of the evidence documented in the previous section is that lower spreads may be justified by tighter and/or more restrictive covenants that constrain the firm's behavior when the debt contracts are in place.[17]

To test this, we take a highly reduced form approach, and simply sum the number of covenants listed for each credit facility. 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. 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.

[17] Covenants play other important roles in the loan syndication process, particular the secondary market transactions of the loan. To free up lenders' capital, loan originators usually sell a large fraction of the syndicated loans on the secondary market. Similar information asymmetry and moral hazard problems arise between originator and buyer of a syndicated loan. Covenants help to resolve such agency issues, as shown by, e.g., Drucker and Puri (2008) and Gupta, Singh, and Zebedee (2008).

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 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 the first column indicates only suggestive evidence for the indicator connections variable, but a stronger result for the more continuous connection variable (column 2). As seen, by doubling the number of personal connections, the probability of covenants being required decreases by 1.7 percent, a result significant at the 2 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 following columns is a linear regression 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. However, never do personal connections *positively* predict covenants, and therefore do not provide an alternative interpretation to the results in Tables 2 and 3.

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 to be the more severe) benefit the most. Here, we consider whether the effects we document only affect the smallest loans, or whether the effects generalize to larger stakes.

Table 4, Panel B considers 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 size of loan. The discrete specification in column 1 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. The final two columns of Panel B show that with firm fixed effects, both specifications indicate a strong, positive relation. The third column shows that compared to the specification in column 1, the inclusion of firm fixed effects slightly strengthens the result. In the final column, the elasticity is a precisely estimated 0.076, indicating that two additional connections (a 100 percent increase from the mean) increases average loan balances by $27 million.

IV. Ex-post performance

The results of Section III indicate that firm-bank personal connections shift lending terms to benefit the firm, 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 loans emanating from connected vs. 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 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 bank deal, and tracked forward.

A. Future Credit Ratings

If a firm's fundamentals deteriorate after securing a loan or line of credit, this should theoretically be captured by changes in future credit ratings. For publicly rated firms, *Dealscan* provides their long-term Standard and Poors' long-term public debt ratings when syndicated credit facilities are initiated. From Moodys (and checked against COMPUSTAT), we then collect each borrower's future credit rating at 12, 24, and 36 months subsequent to the deal. Our interest is whether firm-bank personal connections can be used to predict future changes in credit ratings. Figure 1 shows graphical evidence that they can.

Before proceeding, we note one important change to the sample throughout Section IV. 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 multiple lines of credit within the same loan package, this clearly constitutes only one independent observation for the firm's *ex post* performance. Relative to the analysis in Section III, collapsing the sample at the package level thus reduces the sample by about 20 percent.[18]

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[18] We note that whether we use the current or previous convention for the analysis in Section III, the results are nearly unchanged.

In Panel A of Figure 1, we show the evolution of future credit ratings following personally connected deals, and in Panel B, that for unconnected deals.[19] The striking differences between Panels A and B underscore the importance of personal connections as an *ex ante* indictor 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 syndicates are more likely to worsen.

Such a pattern holds without exception for *every* rating category. 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 same list for firms that borrow from an unconnected syndicate: AAA: 10%, AA: 44.2%, A: 15.6%, BBB: 10.5%, BB: 23.6%, B: 7.0%, <CCC: 0%. A nearly identical (but mirror) pattern emerges if one considers upgrades.

Table 5 puts this in a regression framework, allowing such future changes in credit ratings to also depend on firm and industry characteristics. The first, second, and third columns track credit ratings changes at the 12, 24, and 36-month interval after the initiation of a syndicated bank deal. In every case, the dependent variable is the discrete indicator *Downgrade*, taking a value of one if the firm is subsequently downgraded (e.g., BBB to BB or below), and zero otherwise. The reduction in the number of observations reflects not only the requirement of a public rating (see the difference between Tables 2 and 3), but also for a public rating at the required future interval. Specifically, for firms that conduct deals in the latter part of our sample, not enough time has passed for their future credit ratings to be analyzed.

As seen in columns 1, 3, and 5, syndicated deals where at least one personal connection is present 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

[19] Figure 1 tracks future credit ratings as far ahead as possible, through the date our data was collected (July 2009). We present this figure to give a complementary perspective to the regression results in Table 5, which standardizes the *ex post* sample periods.

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 unreported results, we have estimated the same regressions, but exclude firms whose future credit ratings do not change. The results are magnified even further in this case. Similarly, regressions of credit rating upgrades indicate the expected mirror pattern, as can be seen in Figure 1. In the second, fourth, and sixth columns, we see that the logarithmic specification also significantly predicts *downgrades*, more so at longer horizons.

When analyzing credit ratings, it is important to realize 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, we 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 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 a public debt at the time a syndicated bank deal is initiated. Here, we examine a different dependent variable, gaining over 3,000 firms relative to the analysis of credit rating changes.

In Table 6, we present results of regressing future *EDF*s and *EIS*, both firm-level credit risk estimates provided to us by Moody's, on the firm-bank personal connections used in our previous tests. The dependent variable is either the firm's *EDF* (Panel A) or *EIS* (Panel B) at one of the following future dates: 12 months, 24 months, or 36 months following the initiation of the syndicated credit facility. Although we include the same set of firm and industry characteristics as in previous regressions, the key is control is the firm's *current* value of either *EDF* or *EIS*, i.e, the value at the date when the loan originates.

Comparing the different *EDF* horizons in Panel A columns one, three, and five, we see that the presence of firm-bank personal connections remain highly significant over each window, but as in Table 5, becomes more important as the horizon increases. For example, in the 36-month period shown in column 5, we see that firm-bank personal connections are associated with almost a 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, as well as the usual set of control variables. The first column indicates that even controlling for its *EIS* at the time the loan is initiated, the presence of personal connections to syndicate members reduces its future, expected borrowing cost by 47 basis points twelve months in advance. (In unreported results, we also find that as in Table 2, this is largest for the most risky firms.) By 24 months, the expected reduction is almost 80 basis points, in the neighborhood of being upgraded from junk ($\leq$BB) to investment grade ($\geq$ BBB). At three years, the marginal effect is 90 basis points. As in the *EDF* regressions, the log specification (columns 2, 4, and 6) is not as strong, but presents 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 the inherently places the latter at higher default 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 risk; 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 *characteristics-adjusted return*, following Daniel, Grinblatt, Titman, Wermers (1997).[20] 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 their unconnected counterparts. Both point estimates are positive, but the standard errors are

[20] Essentially, this approach adjusts individual stock returns by subtracting the returns from a portfolio with similar size, book-to-market ratio and price momentum characteristics. Chan, Dimmock and Lakonishok (2009) provide considerable evidence favoring this characteristics-adjusted return to the factor-adjusted return model.

relatively large by comparison. In the third and fourth columns, however, we see strong evidence that returns are predictable from a firm's connectedness to its syndicate member. The log specification indicates that doubling the number of personal connections increases the firm's risk-adjusted stock returns by almost 4 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 more recent stock price if three years have not past), connected borrowers perform almost 18 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 our personal connections variables, Panel B presents the results of Fama-McBeth 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, for 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 cross-sectional correlation in stock returns due purely to when firms borrow. The averaged coefficients on the connections variables are shown in Panel B, and in every case, strengthen relative to Panel A.

Panel C of Table 7 presents the evidence in a slightly different way. Here, we define a dependent variable *Extreme Negative Return*, a binary variable that takes a value of 1 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 generally 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 horizon 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 time 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 window, the number of monthly observations afforded by such an approach is only around 100, a considerable burden to statistical inference. However, in unreported results, we find magnitudes to such a trading strategy on par with the results observed in Panels A and B. Such 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 small time sample.

The evidence here 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, 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 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, this distinction is secondary to whether connected deals are better or worse, which the evidence in this section can speak.

V. Other Considerations

A. Endogeneity and Robustness

The considerable time lag between the genesis of personal connections and the outcomes of interest (loan origination and *ex post* performance) goes a long way toward refuting alternative, non-causal interpretations of our results. To be more specific about such objections, we classify them as follows: 1) *reverse causality* – better lending terms catalyze the formation of personal relationships, perhaps as *quid pro quo* to charitable or accommodating bankers, 2) *omitted variables* – unobserved factors drive both firm-bank connections and superior lending terms, so that any observed empirical relation between connections and lending terms is incidental.

The first is excluded by the way we construct our connection measures - we consider only personal relationships formed prior to when banking transactions occur, in most cases, by more than a decade. However, this is not without cost. Although our timing convention almost certainly rules out reverse causation, it forces us to ignore the majority of connections BoardEx makes available, and thus, sacrifice a considerable amount of useful cross-sectional variation. To see this, recall that we can infer connections not only from common schooling institutions or past workplaces, but also from active roles in common *social* organizations, the likes of which include think tanks (Council on Foreign Relations), charities (Saint Agnus Foundation), non-profit organizations (National Urban League), and institutes (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 workplaces. However, without being able to identify the specific dates when such *social* relationships are formed (and this leaving them vulnerable to the reverse causality critique), we cannot defend their inclusion in our main analysis.

With this caveat in mind, for robustness we break up our existing connection measure into its components (*past professional* and *school*) and also add *social* connections in Table 8. As before, we include both the discrete (column 1) and logarithmic (column 2) specifications. In each case, we find that all three connection types are negatively related to credit spreads, with *social* connections having the largest point estimate. When all are included together, the coefficients for *school*, *past professional*, and *social* indicators are -9 bp, -10 bp, and -13 bp, respectively. The fact that each connection type – all formed in different venues at potentially different times – is independently significant indicates robustness, rendering a spurious relation with spreads unlikely.

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 continuation mechanism for relationships to *persist* into the future (*school* and *past professional* connections have no comparable mechanisms), that they would be particularly costly to damage. In connected deals where such valuable *social* relationships are effectively pledged as collateral, we might therefore expect larger marginal effects on credit spreads. While consistent with the evidence, 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.

The second type of endogeneity posits the existence of factors unobserved by the econometrician, which are correlated with both firm-bank personal connections and lending terms. Two features of our empirical setting pose a challenge to such alternative explanations. The first, as before, is the timing. Because connections are measured so far in advance of the banking transactions being observed, any unobserved determinant of them would have to act over many years. Second, because our connection measures are specific to firm-bank *pairings*, any proposed explanation must account for the effects being observed only between certain firm-bank matches. Arguments operating only at the firm level are insufficient.

To be more specific, suppose that a CFO's external network to bankers simply proxies for having studied finance or economics, perhaps at an elite university. Presumably, either alone could have a causal impact on the firm's financial sophistication or performance, irrespective of any influence of firm-bank personal networks. If credit agencies either do not observe or recognize these sources of value, then even controlling for ratings, we might expect the size of a CEO's network to bankers to proxy for default risk.

Although such an argument is plausible, it fails falsification. Note that Tables 5 through 7 indicates predictability in firm performance, but only at *specific times*. After the initiation of connected deals, *ex post* stock returns and credit ratings improve; at other times, these performance differences disappear. This is confirmed in the spread regressions in Table 3 (columns 2 and 4), which includes examines credit spreads for the same firm, but with different syndicate partners. In these regressions, the necessary variation arises from a firm having different syndicate partners, and thereby different numbers of connections. Quite clearly, any variation of the argument that firm-bank personal connections simple capture default risk does not explain the differential results we observe between connected and unconnected syndicates, nor why they would manifest only at certain times.

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 point applies less in the context of bank financing, 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 higher. 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, 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 on reported in Table 3 (0.13) is nearly identical to the full sample (.12), and is significant at far better than the one percent level. 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 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 effects on credit spreads. However, the effect of bank-firm personal connections remains virtually unchanged compared to the previous column or Table 3, indicating an elasticity of slightly over .11 ($p<0.001$). Other robustness checks including a larger number of fixed effects, or interacting previous years' activity with firm-bank personal connections, have virtually no effect on variable of interest (not reported).

C. Measurement Error

All our analysis involves proxies for personal connections between firms and lenders – never do we observe these relationships directly. Thus, when we include the number of one's school classmates in a regression of lending terms or *ex post* performance, we have introduced an errors-in-variables problem. As is well known, if this measurement error is true noise, then the coefficients on the personal connections will be biased to zero, implying that the magnitudes we report are lower bounds on the underlying economic phenomena.

We have no way or determining a priori whether we are under- or overestimating the number of actual firm-bank personal connections. On the one hand, not every school classmate will qualify as a connection in the understood sense; on the other, BoardEx allows us access to only a small fraction of the potential venues in which relationships may be fostered. For this exact reason, we focus much of our analysis on log-log specification that is invariant to such scale differences. As an example, if we have systematically underestimated the number of firm-bank personal connections by a factor of two, this is irrelevant for an elasticity calculation.

D. Executive Compensation

We conclude by exploring a potentially interesting implication of our results pertaining to the labor market for CEOs. In a related paper, Engelberg, Gao, and Parsons (2009) study a broad cross-section of industries, finding that a CEO's "rolodex" is a strong predictor of pay. CEOs with large numbers of connections to those *outside the firm* (for which governance-based pay explanations bear no relevance) make substantially more money, with important connections being rewarded the most. Their interpretation was that because networks are excludable, the CEO should be able to extract a labor market premium by allowing the firm access to the information privy only to network members. The present study provides a specific illustration of how this might occur: without the personal connection to reduce asymmetric information between borrowers and lenders, the firm's borrowing cost increases. Unless the labor market for CEOs is perfectly competitive, part of these savings should accrue to him or her.

To get a sense of the magnitudes involved, consider that the average deal involves slightly more than $650 million, but ranges into the tens of billions (Table 1). Taking the estimates in the first two columns of Table 3 as a guide, an annual reduction in credit spread of 18 bp equates to pre-tax savings of $1.18 million. Thus, the average firm expected to be active in the debt market has roughly one million dollars in after-tax surplus to divide between the management and shareholders.

In Table 9, we present more direct evidence suggesting that a CEO's personal connections to commercial bankers are in fact rewarded in the labor market. For each of CEO during every year, we construct his or her "bank rolodex," measured as the number of *school* or *past professional* connections to any of the commercial bankers in our data. Importantly, we construct this measure for all CEOs, regardless of whether their firms are active in the debt market that year. We know already that the value of such relationships is explicitly monetized *ex post* to a deal occurring (Tables 2 through 4); however, a deep network of financial connections can presumably create value in an *ex ante* sense, such as reducing search costs should the firm need to raise bank debt in the future. In either case, the empirical prediction between CEO pay and financial connections is unambiguous.

In the first column of Table 9, we regress the logarithm of the CEO's total direct compensation (extracted from the EXECUCOMP files) against his or her *bank rolodex*. The coefficient indicates that each additional connection to commercial bankers increases the CEO's pay by .36%, equating to roughly $21,000. Alternatively, the standard deviation of *bank rolodex* is 29.6, which would correspond to an average total pay increase of over $600,000. The second column breaks up the *bank rolodex* into its components, and indicates that each, on its own, remains a highly statistically significant determinant of pay. The market value of *school connections to bankers* is particularly striking, at over 2.7 percent each. For comparison, in column three, we add to the regression each CEO's "non-bank rolodex," the main explanatory variable of interest in Engelberg et al. (2009), measured as connections to other CEOs and directors of public firms (rather than to bankers). As seen, although all connection measures are highly significant, connections to bankers are associated with the highest wage premia. The differences between the *non-bank rolodex* are significant at the 5.6 percent (*past professional*) and .59 percent (*school*) level respectively.

The fourth column indicates perhaps the strongest causal evidence that banking connections are value assets for corporate CEOs. By including firm fixed effects in our regressions, we gain identification from within-firm variation in the CEO's *bank rolodex*, which gets most of its variation when CEOs are replaced. The coefficient on the variable of interest remains highly significant ($p<.001$), preserving most of its magnitude relative to the specification in the first column. In effect, when a firm hires a CEO with extensive networks to bankers (relative to the old CEO), this is explicitly reflected in the new CEO's wage. Explanations related to unobserved firm characteristics do not explain this result.[21] Together, these results imply that both in the cross-section and time series, compensation committees recognize the value of a CEO's personal connections to commercial bankers, tying the loan-level evidence of the previous section to the labor market.[22]

[21] However, there is clearly the possibility that a CEO's rolodex (bank or non-bank) may be correlated with other determinants of productivity. Engelberg, Gao, and Parsons (2009) discuss this possibility extensively and argue that evidence for a causal link between a CEO's network of external connections and pay exists.

[22] In unreported results, further causal evidence is that the effect of the financial rolodex on pay strengthens if one considers only firms active in the commercial debt market that year.

VI. Conclusion

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," an inviting if not ambiguous term. 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 alter financing terms, and 2) if so, are these good or bad decisions? 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 a resounding "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 concession depends on the firm's borrowing risk, with higher risk leading to a larger interest rate reduction. Furthermore, after initiating a deal with a personally connected syndicate, firms improve their credit ratings and enjoy substantially higher risk-adjusted stock returns. In aggregate, the concessions in lending terms for connected deals 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. The timing of connections is crucial. By focusing exclusively on personal relationships formed several years prior to the banking deals we analyze, we exclude the possibility that social relationships are simply a product of existing or anticipated banking relationships, the latter of which we already know influences lending terms. Indeed, our results hold irrespective of whether the firm and bank have consummated a deal in the recent past, suggesting that personal relationships offer additional advantages relative to those gained simply from repeated business.

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.[23] Given the ostensible goal of such legislation to level the informational playing field among market participants, it is noteworthy 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 (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 – we leave to future work.

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

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Figure 1: Credit Ratings Evolution for Connected and Unconnected Firms

Each figure is conditional on an initial credit rating at the time of the deal as reported from Dealscan. The final credit rating (as reported by Dealscan) is the credit rating at the end of the facility. The graphs plot the fraction of firms at each credit rating conditional on their initial rating. The plots separate connected firms (those with at least 1 personal connection to any of the lending banks) from unconnected ones (those with no personal connections to the lending banks). We omit initial credit categories of AAA, CC and C because we have too few observations in these categories.

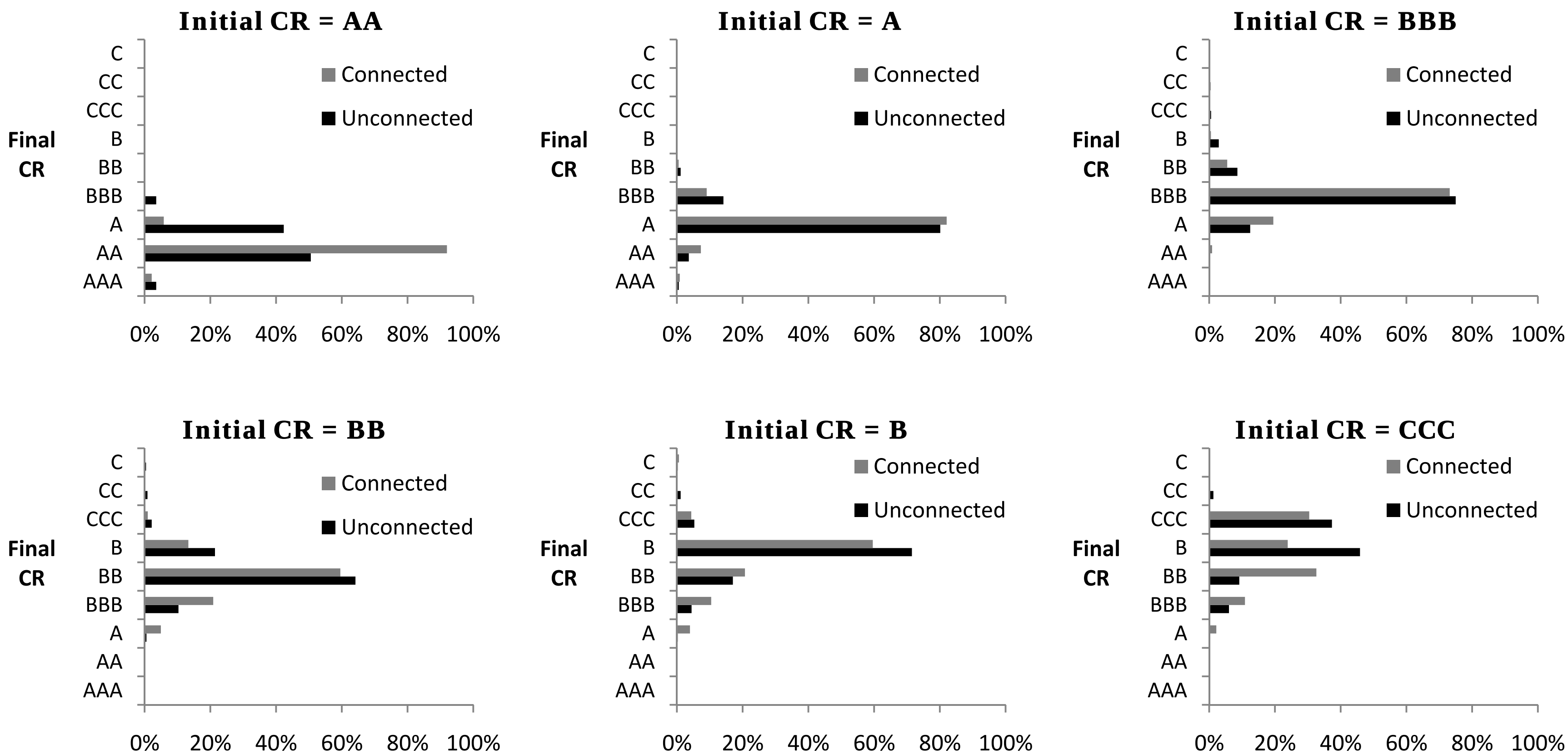

Table 1: Summary Statistics

Table 1 reports summary statistics for several variables used in the paper. Panel A reports data on syndicated loans, extracted from the *Dealscan* database. 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 *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. *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. With *Social Connections*, we sum all instances in which 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, *EDF Implied Spreads* at the end of the month prior to loan origination, and the Daniel, Grinblatt, Titman and Wermers (DGTW) size, book-to-market and momentum *Characteristics-adjusted Returns* of past 12, 24 and 36 months 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					
School Connections Per Syndicated Loan	0.26	0.00	0.87	0.00	1.00
Professional Connections Per Syndicated Loan	2.02	0.00	7.45	0.00	5.00
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
Characteristics-adjusted Return, past 12 months	0.04	-0.01	0.62	-0.50	0.55
Characteristics-adjusted Return, past 24 months	0.08	-0.03	0.96	-0.69	0.79
Characteristics-adjusted Return, past 36 months	0.11	-0.05	1.24	-0.84	1.02

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 *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 characteristics 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 characteristics 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 headquarter of the borrower. The set of macro control variables include the levels and changes in default spread (the yield spreads between BAA and AAA corporate bond indices), the level of and changes in term spreads (the yield spreads between 10-year Treasury and 3-month Treasury), and the most recent monthly returns of S&P 500 index returns. *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 examines 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	-28.07***	-8.359**	-19.99***	-50.81**	-47.82***
	(2.738)	(3.401)	(3.376)	(20.63)	(5.825)
AAA Credit Rating	-173.6***	-7.054			
	(8.675)	(6.406)			
AA Credit Rating	-160.4***				
	(8.010)				
A Credit Rating	-144.1***	8.055			
	(6.014)	(5.479)			
BBB Credit Rating	-102.4***		-110.0***		
	(5.469)		(5.383)		
BB Credit Rating	-44.22***		-43.69***		
	(5.199)		(4.670)		
B Credit Rating	-3.710				
	(5.058)				
CCC Credit Rating	-35.89***				
	(4.667)				
CC Credit Rating	15.62			34.68	
	(12.75)			(25.20)	
C Credit Rating	1.581			36.25	
	(37.13)			(37.80)	
Log(Maturity)	1.594	1.482	-0.0867	32.17	2.647
	(5.596)	(3.011)	(8.270)	(51.46)	(10.16)
Deal in Past 1-3 Yrs Indicator	-13.34***	-0.736	-8.197**	-10.98	-19.50***
	(2.855)	(4.091)	(3.651)	(20.10)	(5.563)
Deal in Past 4-6 Yrs Indicator	-7.297**	3.374	-9.691***	-14.75	-0.116
	(2.965)	(4.954)	(3.267)	(24.81)	(7.010)
Deal in Past 7 or early Indicator	-6.675**	-3.121	-4.231	-39.80	-12.55*
	(3.041)	(2.503)	(3.981)	(27.00)	(6.834)
Number of Lenders	0.212	-0.228	0.0634	-0.168	0.330
	(0.164)	(0.192)	(0.178)	(0.744)	(0.446)
# of Loans Offered by Syndicate Prior Year	-0.0209***	-0.00485***	-0.0176***	-0.0322***	-0.0304***
	(0.00111)	(0.00180)	(0.00130)	(0.0102)	(0.00242)
Local Bank Indicator	0.516	0.284	0.229	0.889	1.188
	(0.470)	(0.473)	(0.535)	(3.845)	(1.303)
Macroeconomic Controls	YES	YES	YES	YES	YES
Seniority Fixed Effect	YES	YES	YES	YES	YES
Year Fixed Effect	YES	YES	YES	YES	YES
Observations	17,428	1,705	8,666	359	6,698
Adjusted R^2	0.431	0.368	0.448	0.250	0.231

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 *Past Professional Connection* between the borrower and any syndicate bank. 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 *Connected Indicator* takes a value of one if there exists at least one *School Connection* or *Past Professional Connection* between the borrower and any syndicate bank. The logarithm of this variable is self-explanatory. The set of borrower financial fundamental control variables include *CAPM Beta* estimated using the past three-years of monthly returns (with 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 characteristics 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 characteristics 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 headquarter of the borrower. The set of macro control variables include the levels and changes in default spread (the yield spreads difference between BAA and AAA corporate bond indices), the level of and changes in term spreads (the yield spreads difference between 10-year Treasury and 3-month Treasury), and the most recent monthly returns of S&P 500 index returns. *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	-18.69***	-17.91***		
	(3.446)	(3.731)		
Log (1+ Number of Connections)			-0.120***	-0.0356***
			(0.0130)	(0.0107)
CAPM Beta	0.471	0.173	0.0189**	0.00555
	(1.612)	(1.898)	(0.00796)	(0.00831)
Log(Total Assets)	-4.258***	-13.26**	-0.0696***	-0.0541**
	(1.621)	(5.411)	(0.0121)	(0.0221)
M/B	-1.509**	-3.667**	-0.0202***	-0.0230**
	(0.680)	(1.703)	(0.00641)	(0.01000)
Capital Expenditure / Total Assets	-1.477	30.06*	-0.00875	0.159**
	(15.63)	(17.83)	(0.0756)	(0.0678)
Tangible / Total Assets	-6.511	1.040	-0.0302	0.0201
	(4.386)	(5.085)	(0.0233)	(0.0250)
Profitability	-31.00***	-75.20***	-0.138**	-0.387***
	(8.604)	(17.01)	(0.0603)	(0.0748)
Log(Maturity)	11.05**	5.363	0.114***	0.0188
	(5.157)	(4.915)	(0.0281)	(0.0236)
Deal in Past 1-3 Yrs Indicator	-3.483	1.099	-0.0201	0.00105
	(3.341)	(3.313)	(0.0204)	(0.0190)
Deal in Past 4-6 Yrs Indicator	-3.035	0.532	0.0160	0.0194
	(3.156)	(3.140)	(0.0193)	(0.0162)
Deal in Past 7 or early Indicator	-8.036**	-9.630***	-0.0209	-0.0474**
	(3.408)	(3.360)	(0.0231)	(0.0200)
# of Loans Offered by Syndicate Prior Year	-0.0174***	-0.0120***	-8.93e-05***	-7.65e-05***
	(0.00151)	(0.00142)	(9.68e-06)	(7.83e-06)
Local Bank Indicator	2.120***	2.509***	0.00906**	0.0131***
	(0.619)	(0.679)	(0.00394)	(0.00329)
Number of Lenders	-0.193	-0.832***	0.00219	-0.00411***
	(0.301)	(0.288)	(0.00221)	(0.00156)
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 Fixed Effect	NO	YES	NO	YES
Observations	11,003	11,003	11,003	11,003
Adjusted R^2	0.505	0.745	0.615	0.859

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). 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 *Past Professional Connection* between the borrower and any syndicate bank. The same set of firm, loan, lender, industry, and macro controls in Table 3 are employed here. 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 *Connected Indicator* takes a value of one if there exists at least one *School Connection* or *Past Professional Connection* between the borrower and any syndicate bank. The logarithm of this variable is self-explanatory. 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	-0.0129		0.0745	
	(0.0146)		(0.1115)	
Log (1+ Number of Connections)		-0.0169**		-0.0872*
		(0.00720)		(0.0522)
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 Fixed Effect	NO	NO	YES	YES
Observations	11,964	11,964	11,964	11,964
Adjusted R^2	0.377	0.378	0.677	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	0.132***		0.142***	
	(0.0320)		(0.0346)	
Log (1+ Number of Connections)		0.0258		0.0761***
		(0.0176)		(0.0179)
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 Fixed Effect	NO	NO	YES	YES
Observations	11,964	11,964	11,964	11,964
Adjusted R^2	0.653	0.652	0.812	0.812

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

Table 5 relates future credit rating changes at different horizons to borrower/lender personal connections. 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. Shown are marginal effects from Probit estimation. Stars *, **, 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	-0.0237***		-0.0583***		-0.0709***	
	(0.00757)		(0.0114)		(0.0145)	
Log (1+ Number of Connections)		-0.0078**		-0.0128***		-0.0145**
		(0.00367)		(0.00534)		(0.00681)
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	0.090	0.088	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 financial fundamentals, lender characteristics, and macroeconomic conditions at time of loan origination. The set of control variables is the same as those reported in Table 3. The number of connection describes the sum of *School* and *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	-0.411***		-0.745***		-0.835***	
	(0.105)		(0.179)		(0.210)	
Log (1+ Number of Connections)		-0.0886*		-0.170*		-0.270**
		(0.0505)		(0.100)		(0.108)
Current EDF	0.636***	0.638***	0.367***	0.369***	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	-47.04***		-78.03***		-90.55***	
	(11.51)		(19.70)		(24.26)	
Log (1+ Number of Connections)		-12.44**		-17.10		-29.01**
		(5.575)		(11.36)		(12.45)
Current EDF Implied Spreads (EIS)	0.526***	0.527***	0.357***	0.359***	0.203***	0.203***
	(0.0572)	(0.0573)	(0.0610)	(0.0612)	(0.0629)	(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 firm's future stock returns to borrower/lender past 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 connection describes the sum of *School* and *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 of probit estimation. 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	0.0331*		0.106***		0.178***	
	(0.0191)		(0.0314)		(0.0434)	
Log (1+ Number of Connections)		0.0108		0.0360***		0.0552***
		(0.00732)		(0.0122)		(0.0169)
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-squared	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	0.0407*		0.1151***		0.1973***	
	(0.0237)		(0.0313)		(0.0514)	
Log (1+ Number of Connections)		0.0130		0.0496***		0.0713***
		(0.0090)		(0.0133)		(0.0234)
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
Adjusted R^2	0.075	0.067	0.053	0.048	0.056	0.047

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	-0.0149**		-0.0319***		-0.0506***	
	(0.00686)		(0.0108)		(0.0131)	
Log (1+ Number of Connections)		-0.00903*		-0.00983		-0.0251***
		(0.00480)		(0.00645)		(0.00751)
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 firm's syndicated loan's *All-in Drawn Spreads* to borrower/lender connections, a set controls for borrower financial 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 in 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	-8.918***			
	(2.986)			
Professional Connection Indicator	-10.37***			
	(3.473)			
Social Connection Indicator	-13.24***			
	(3.436)			
Log (1 + Number of School Connections)		-0.0699**		
		(0.0295)		
Log (1 + Number of Professional Connections)		-0.0410***		
		(0.0144)		
Log (1 + Number of Social Connections)		-0.128***		
		(0.0161)		
Log (1 + Number of Connections)			-0.130***	-0.114***
			(0.0351)	(0.0128)
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-squared	0.506	0.622	0.457	0.639

Table 9: Connections and CEO Compensation

In Table 9, the natural logarithm of total *Executive Compensation* is regressed against various social connection measures and firm characteristics. In all regressions, the dependent variable is dollar value of *Total Direct Compensation*, taken from ExecuComp. The sample spans 2000-2007. Among the independent variables, *Rolodex (Bank)* is the sum of *School Connections* and *Past Professional Connections* between the firm's CEO and executives/directors within the commercial banking industry. *Rolodex (Bank, Past Education)* and *Rolodex (Bank, Past Professional)* are the specific decomposition of this variable. *Rolodex (Non-Bank)* is the sum of *School* and *Past Professional Connections* to all directors and executives of other non-bank public firms. *Tenure* is the time (in years) since the executive became CEO. *Age* is the CEO's age according to ExecuComp. *Assets* are extracted from Compustat. *Idiosyncratic volatility* is the average squared error taken from a CAPM regression of monthly returns over the past 3 years. *Return [t-1, 0]* is the raw one-year cumulative return ending on the fiscal year end date. *Return [t-3, t-2]* is the raw two-year cumulative return ending on the prior fiscal year end date. *Market-to-Book* is self-explanatory. Regressions 1 to 3 include Year and industry fixed effects; Regression 4 includes firm fixed effects. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5% and 1% levels, respectively.

	Dependent Variable: log (Total Direct Compensation)			
	(1)	(2)	(3)	(4)
Rolodex (Bank)	0.00363***			0.00254***
	(0.000536)			(0.000607)
Rolodex (Non-Bank)			0.000570***	
			(0.000163)	
Rolodex (Bank, Past Education)		0.0271***	0.0186***	
		(0.00661)	(0.00649)	
Rolodex (Bank, Past Professional)		0.00399***	0.00243***	
		(0.000754)	(0.000876)	
Log(Total Assets)	0.415***	0.425***	0.409***	0.313***
	(0.0149)	(0.0136)	(0.0152)	(0.0493)
Return [t-1, 0]	0.0321	0.0317	0.0323	0.0310
	(0.0224)	(0.0224)	(0.0224)	(0.0219)
Return [t-3, t-2]	0.0989***	0.0987***	0.0988***	0.0824***
	(0.0164)	(0.0163)	(0.0164)	(0.0161)
Idiosyncratic Volatility	9.665***	9.672***	9.492***	6.098
	(2.645)	(2.627)	(2.645)	(5.035)
M/B	0.000146*	0.000150**	0.000145*	-3.14e-05**
	(7.62e-05)	(7.52e-05)	(7.52e-05)	(1.32e-05)
Tenure	0.0132**	0.0133**	0.0128**	-0.00674
	(0.00557)	(0.00559)	(0.00554)	(0.00586)
$Tenure^2$	-0.000682***	-0.000684***	-0.000664***	1.89e-05
	(0.000203)	(0.000204)	(0.000202)	(0.000195)
Year Fixed Effect	YES	YES	YES	YES
Industry Fixed Effect	YES	YES	YES	YES
Firm Fixed Effect	NO	NO	NO	YES
Observations	10,419	10,419	10,419	10,419
Adjusted R^2	0.349	0.349	0.351	0.707

Appendix A: Variable Definitions and Constructions

Variable Name	Variable Definitions and Constructions	Source of Data
All-in Drawn Spreads	All-in draw 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 takes value of one if the loan is a senior loan, and zero otherwise	Dealscan
Deal in Past 1-3 Yrs Indicator	Dummy variable takes value of one if the firm borrows from the syndicated loans market during prior three years	Dealscan
Deal in Past 4-6 Yrs Indicator	Dummy variable takes value of one if the firm borrows from the syndicated loans market during further back three years	Dealscan
Deal in Past 7 or early Indicator	Dummy variable takes value of one if the firm borrows from the syndicated loans market during further seven years or early	Dealscan
Local Bank Indicator	Dummy variable takes value of one if one of the syndicate member bank is located within 100 km away from the borrower's headquarter 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 characteristics-adjusted return 12-month ahead, beginning at the month immediately after facility activation's month.	CRSP/Compustat
Characteristics-Adjusted Return, 24-month ahead	The cumulative DGTW characteristics-adjusted return 24-month ahead, beginning at the month immediately after facility activation's month.	CRSP/Compustat
Characteristics-Adjusted Return, 24-month ahead	The cumulative DGTW characteristics-adjusted return 36-month ahead, beginning at the month immediately after facility activation's month.	CRSP/Compustat
CAPM Beta	Beta estimate from the Capital Asset Pricing Model (CAPM), using past 36 month monthly return, with minimum of 18 months of return data	CRSP
Idiosyncratic Volatility	Residual standard deviations estimate from the Capital Asset Pricing Model (CAPM), using past 36 month monthly return, with minimum of 18 months of return data	CRSP
Expected Default Frequencies (EDF®)	The expected default frequency computed and calibrated to actual default events by the 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 multiplication of the estimated expected default frequency and estimated expected loss given default (LGD).	Moody's-KMV
EDF Decile Fixed Effect	Dummy variable equals one if EDF value falls into one of the ten EDF deciles, where EDF deciles are defined over cross sectional EDF values within the month	Moody's-KMV
Rolodex (Bank)	The sum of *School Connections* and *Past Professional Connections* between the firm's CEO and executives/directors within the commercial banking industry.	BoardEx
Rolodex (Non-Bank)	The sum of *School* and *Past Professional Connections* to all directors and executives of other non-bank public firms.	BoardEx
Rolodex (Bank, Past Education)	The number of *School Connections* between the firm's CEO and executives/directors within the commercial banking industry.	BoardEx

Rolodex (Bank, Past Professional)	The number of *Past Professional Connections* between the firm's CEO and executives/directors within the commercial banking industry.	BoardEx
Return [t-1, 0]	Cumulative past 12-month raw return	CRSP
Return [t-3, t-2]	Cumulative past 36-month raw return excluding most recent 12-month return	CRSP
Tenure	Number of years since the individual becomes the CEO of the firm	EXECUCOMP
Total Direct Compensation	The dollar value of total direct compensation of the CEO (TDC1)	EXECUCOMP
Level of Term Spreads	The difference between 10-year treasury yield and 3-month treasury yield	Federal Reserve
Change of Term Spreads	The change of term spreads between current month and prior month	Federal Reserve
Default Spreads	The difference between the Moody's BAA corporate bond index yield and Moody's AAA corporate bond index yield	Federal Reserve
Change of Default Spreads	The change of default spreads between current month and prior month	Federal Reserve