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Friends with money ☆

Joseph Engelberg [a,1], Pengjie Gao [b,2], Christopher A. Parsons [a,*]

[a] Rady School of Management, University of California at San Diego, La Jolla, CA, USA
[b] Mendoza College of Business, University of Notre Dame, USA

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ABSTRACT

When banks and firms are connected through interpersonal linkages – such as their respective management having attended college or previously worked together – interest rates are markedly reduced, comparable with single shifts in credit ratings. These rate concessions do not appear to reflect sweetheart deals. Subsequent firm performance, such as future credit ratings or stock returns, improves following a connected deal, suggesting that social networks lead to either better information flow or better monitoring.

1. Introduction

Stein (2003) characterizes information and agency problems as the "most pervasive and important" violations of the Modigliani and Miller (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 could overlook its flaws, thereby putting his or her own shareholders' capital at undue risk. On the other hand, such relationships could catalyze information flow or reduce monitoring costs, placing the connected bank at an advantage relative to competing lenders. Here, both parties stand to benefit: Banks make better lending decisions and, assuming the associated savings are shared, firms reduce 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

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* Corresponding author. Tel.: +1 919 265 9246.
E-mail addresses: joseph_engelberg@kenan-flagler.unc.edu (J. Engelberg), pgao@nd.edu (P. Gao), chris_parsons@kenan-flagler.unc.edu (C.A. Parsons).
[1] Tel.: +1 919 962 6889.
[2] Tel.: +1 574 631 8048.

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explore whether such relationships lead banks to make choices that harm their own shareholders or whether they improve their capital allocation decisions.

To address these questions, we assemble a data set of roughly 20 thousand commercial loans made to US companies between 2000 and 2007. The set of borrowers involves more than five thousand public firms; the set of lenders, more than 19 hundred commercial banks (the majority of which are private). Next, from BoardEx we obtain a list of common organizations in which each of the 65 thousand unique directors and executives in our universe of firms and banks could have fostered personal relationships. This list tells us, for instance, if the president of Wachovia Bank and the chief executive officer (CEO) of Pepsi Co. attended college together, or if they overlapped in their first job after graduate school. The main question: Do personal relationships such as these influence lending terms?

Establishing a causal relation requires a careful account of the endogeneity of personal relationships. A serious concern is reverse causality, whereby lending interactions lead to the formation of social relationships. To illustrate, 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 generate correlation between lending terms and firm-bank personal relationships, but not for causal reasons.

Perhaps the most significant advantage of our data is that they allow us to infer relationships whose formation predates, by several years or decades, the lending transactions we analyze. If Pepsi borrows from a Wachovia-led syndicate in 2004, we take as exogenous that their respective top executives could have both received a masters of business administration from Stanford University in 1974 or both worked for Xerox in 1982. Such a long lag between relationship formation and lending transactions removes reverse causality concerns by construction.

In pooled cross-sectional regressions of interest rates charged by syndicates, we find that the presence of at least one preexisting, personal relationship between the firm and lender removed by at least 5 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 (bps); because spreads are bound at zero, the effect for highly rated firms cannot be large. However, the effect climbs steadily as credit quality deteriorates. Firms with ratings in the BBB–B range can expect interest rate concessions of about 20 bps. The magnitude more than doubles again for firms rated even worse or that lack a rating altogether (45–50 bps). One might expect the result to strengthen not only because default risk increases borrowing costs, but also because adverse selection and monitoring problems are most severe for these firms. In pooled models controlling for a variety of firm, industry, loan, and macroeconomic characteristics, we observe similar magnitudes, averaging between 15 and 20 bps across all credit categories, or about 10% of the average charged spread. For comparison, the average spread between A and AA ratings is 16 bps.

Although reverse causality is eliminated by how we form the connection variables, one might suspect that firm-bank personal connections could be correlated with unobserved firm or lender attributes and that these attributes drive the results we observe. For example, firms with large management teams, on average, share more personal ties with any lender, not just the ones with whom they conduct business. Likewise, bigger or more active banks also (mechanically) have more average connections and could also have cost advantages relative to smaller lenders, allowing interest rates to be affected.

Because many firms borrow from multiple syndicates over our eight-year sample period, and because the most active banks lend to multiple firms, we can estimate the model with fixed effects for both borrowers and active lenders. This specification is important because it applies to the specific endogeneity concerns discussed above as well as to any argument that relies on systematic differences between connected versus unconnected firms, or between connected versus unconnected banks. Because we are explicitly controlling for both time-invariant bank and firm heterogeneity through fixed effects for each group, and for time-varying risk through observable control variables (e.g., credit ratings), identification comes from the differences-in-differences implied by the construction of a connection variable that is formed at the firm-bank level. It thus follows that any alternative interpretation must either appeal to dynamic performance changes within firms or to an omitted variable that also operates at the firm-bank level.

One such possibility is geographical proximity. If banks close to their borrowers have information or monitoring advantages, and if personal connections are a function of distance, then the results we find could simply reflect the effects of local information networks (e.g., Coval and Moskowitz, 2001; Brickley, Linck, and Smith, 2003) in commercial lending. As in Degryse and Ongena (2005), we find higher rates when borrowers and lenders are located within the same city, but the effect of personal connections remains strong.

A second possibility is that personal connections are simply picking up the familiar result that lending terms can change when a firm and bank do repeated business with each other.[3] Empirical tests also reject this possibility and provide some insight into previous findings. First, the impact of personal connections holds strongly both for a firm's historical banking partners and for banks with which it has no prior lending experience. This finding underscores that, in relationship banking, it appears to be access to specific people that makes the difference, not familiarity with a firm's physical assets. Perhaps more important, the effect of past banking relationships is substantially weakened when personal relationships are added to the regression. This raises the possibility that banking relationships could themselves stem from

[3] See Peterson and Rajan (1994), Berger and Udell (1995), Degryse and Van Cayseele (2000), Bharath, Dahiya, Saunders, and Srinivasan (2011), and Schenone (2010).

personal relationships, suggesting an original determinant of one's financing partners.

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

The second half of the paper considers the normative implication of these results. Taking as given that firm-bank personal connections alter the terms of lending in the firm's favor, we ask whether these appear to be good or bad decisions. Although the source of our banking data [Dealscan of Loan Pricing Corporation (LPC)] does not provide data on specific loan performance, we gain insight by examining the evolution of each borrower's credit rating subsequent to initiating a bank deal. Although not specifically related to a given transaction, these summary statistics measure a firm's ability to meet its outstanding debt obligations, part of which includes the bank transactions we analyze.[4]

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

Analysis of subsequent stock returns indicates even stronger results and confirms that such performance improvements were not foreseen by the market. Pooled timeseries cross-sectional regressions of characteristic risk-adjusted stock returns (following Daniel, Grinblatt, Titman, and Wermers, 1997) indicates 1-, 2-, and 3-year excess returns of 3%, 10%, and 17%, respectively. In other words, firms completing deals to connected banks experience substantially higher stock returns than those borrowing from unconnected syndicates. Fama and MacBeth (1973) regressions indicate similar effects.

At a minimum, their superior ex post performance indicates that personally connected deals are fundamentally different – namely better – than those lacking such relationships. Although perhaps the most intuitive explanation is that personal relationships improve information flow and lending efficiency, an important caveat is that personal relationships could induce inefficiency in lending decisions, but nevertheless predict positive subsequent performance. To see how, imagine a banker who is afraid of appearing corrupt and so requires personally connected firms to meet a higher standard compared with more anonymous borrowers. Here, even if personally connected bankers are no more informed than their unconnected counterparts, they would nevertheless (assuming all bankers have information the market does not) be associated with superior deals ex post. Generally, we are not able to definitively distinguish between these stories, and ultimately, this limits what our study can say about the impact of personal relationships on lending efficiency.

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.[5] 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 likely contributes to why we find such a positive effect of personal connections on lending decisions. In addition, the exogeneity of relationship formation allows for a causal interpretation often made difficult in other settings.

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

We organize the paper as follows. In the next section, we describe the lending and connections data, and we outline our empirical strategies. We begin our formal analysis in Section 3, where we explore the extent to which firm-bank connections influence lending terms including interest rates, covenants, and loan amounts. Section 4 is dedicated to answering the question of whether or not personal connections are associated with better or worse future firm performance. We consider robustness and some extensions to our basic results in Section 5, and then we conclude in Section 6.

[4] Because credit ratings pertain to a firm's public debt, analyzing credit rating changes represents a conservative way of measuring changes in the likelihood of default on more senior claims, such as syndicated bank loans.

[5] Domestic studies include Kroszner and Strahan (2001) and Güner, Malmendier and Tate (2008). Rajan and Zingales (1998) and Charumilind, Kali and Wiwattanakantang (2006), Morck and Nakamura (1999) and Hoshi, Kashyap and Scharfstein (1990), Laeven (2001), and La Porta, Lopez-de-Silanes, and Zamarripa (2003) study connected lending in Asia, Japan, Russia, and Mexico, respectively.

2. Data and identification strategy

Management Diagnostic Limited (MDL) is a data purveyor that collects biographical information on executives and board members of public companies. Its main product, BoardEx, reports work histories, educational backgrounds, and current participation in social organizations for CEOs, chief financial officers (CFOs), other executives, and current directors. BoardEx has been used to examine the role of social networks in a variety of corporate finance settings (e.g., Schmidt, 2008; Cohen, Frazzini and Malloy, 2008; Fracassi and Tate, 2009).

We supplement BoardEx with biographical information on personnel from a large number of public and private commercial banks, made generously available after a custom data request to MDL. The union of these data results in a universe of 5,057 public US firms, 1,924 commercial banks, and 65,074 individuals (either directors or executives at their respective institutions).[6] From these we infer interpersonal linkages between bankers and borrowers.

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

Instead, we wish to identify examples in which social connections are plainly exogenous to the lending deals we analyze. Consequently, we focus on two specific types of connections that meet this criterion: (1) school connections, formed when two people graduate from the same educational institution within 2 years of one another (e.g., Stanford Class of 1984 or 1985), and (2) third-party past professional connections, formed when two people overlap through either a common past job (e.g., both having worked for IBM in their first job after graduation) or past board membership (e.g., both having served on Coca Cola's board). Third-party past professional connections must predate the lending deal by more than 5 years and cannot involve either the borrowing firm or lending institution in any way. This requirement ensures that connections inferred between a banker at bank *X* and manager at firm *Y* are formed at a distant place (say, at firm *Z* or during college) and time (at least 5 years ago).

As a practical matter, this eliminates most of the connections we can infer, including those that arise from current common participation in social organizations such as charities, volunteer groups, and museum boards. To distinguish them from their school and third-party past professional analogs, we refer to these as social connections (admitting a slight abuse of language given that all the connections we analyze are ultimately "social"). Although social connections could have a causal influence on lending behavior, BoardEx does not list the start and end dates for many of them, e.g., we cannot in all cases tell how long a CFO has served on the board of the Bronx Zoo, only that he is currently serving (see also Schmidt, 2008; Fracassi and Tate, 2009). In such cases, we are not able to tell whether this seat came after a banking transaction with another Bronx Zoo director, or vice versa. For this reason, we ignore social connections entirely in our main analysis. What we lose in statistical power, however, we gain in the ability to make precise, causal inferences insofar as personal connections influence lending outcomes.

In Panel B of Table 1, we list summary statistics for all three possible types of connections: school, third-party past professional, and social. The connection measures are calculated at the syndicate level; for example, the mean value of *Third-Party Past Professional Connections* is 1.28, indicating that executives or directors of the typical borrower share roughly two past jobs (since removed by 5 years or more) with executives or directors at any of the syndicate banks. *Past School Connections* are far less common (mean 0.26), in part because of the time restriction we impose: Two individuals must have attended the same educational institution (e.g., Harvard Business School), but no more than 2 years apart (e.g., respective graduation years of 1991 and 1992 would count as a connection, but 1991 and 1993 would not). As seen, social connections are the most common. Throughout our main analysis, we neglect entirely the effects of social connections, which could be subject to reverse causation in the context of lending deals.

The remainder of the connection-formation process, however, is more subjective, and no theory exists to provide guidance. For example, one might expect a firm's connections to the lead bank in a syndicate to be most valuable for information flow or monitoring, so that perhaps we should consider only these. However, the fact that syndicate members often conduct multiple deals together, and rotate the identity of the lead bank across deals (e.g., Cai, 2010) suggests that personal connections to seemingly peripheral participant banks might be similarly valued.[7] A second consideration is measurement error. Clearly, many connections we assign as such are not (most people do not know every member of their graduating class, let alone keep in touch with them years later), which attenuates any marginal effects we observe. The specification we report balances our intuition between which connections we think have the potential for information transmission and errors-in-variables bias.[8]

[6] About 16 hundred of the banks in our data set are private, making this the first study to consider the impact of networks and information flow involving nonpublic firms.

[7] Cai (2010) studies the syndicated lending market from 2004 to 2006 and finds that 77% of lead arrangers also participate in syndicates in which they are not the lead and that "it is a common practice for lenders to maintain stable relationships with certain other lenders and rotate their roles between leading and participating within the group". See also Lin and Paravisini (2010) for evidence of reputation concerns among syndicate members.

[8] In unreported results, we experiment with a number of alternative definitions for connections, including considering only those between the borrower and lead bank in the syndicate, requiring connections to be

Table 1
Summary statistics.
The table reports summary statistics for several variables used in the paper. Panel A reports data on syndicated loans, extracted from the Dealscan database. Variables shown are the *Dollar Value of Each Syndicated Loan* in millions of dollars, the *Total Number of Covenants*, the *All-in Drawn Spreads* in basis points, the *Number of Lenders*, and the *Number of Local Banks*. A lender is considered local if its headquarters is within 100 km of the headquarters of the borrower. Panel B reports summary statistics for our personal connections variables. *Past School Connections* is calculated by summing all instances in which a director or executive of the borrower and a director or executive of the lender attended the same educational institution and matriculated within 2 years of one another. *Third-Party Past Professional Connections* is formed similarly, but with a common past employer. All professional connections are at least 5 years removed from the date of any banking activity and do not include instances in which the connection was made at the lending bank or the borrowing firm. With *Current Social Connections*, we sum all instances in which a director or executive of the borrower and a director or 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 Indicator*, *Deal in the Past 4–6 Years Indicator,* and *Deal in the Past 7 Years or Earlier Indicator* take values of one if the current borrower borrowed from one or more members of the current syndicate in the most recent three years, the 3 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 (*Total Assets*), profitability as of the most recent fiscal year-end prior to the loan origination (*Profitability*), tangible assets normalized by the lagged total assets (*Tangibility*), market-to-book ratio (*M/B*), capital expenditures normalized by lagged total assets (*Capital Expenditures/Total Assets*), Expected Default Frequency at the end of the month prior to the loan origination (*Expected Default Frequency*), and EDF Implied Spread at the end of the month prior to loan origination (*EDF Implied Spread*).

	Mean	Median	Standard deviation	10th Percentile	90th Percentile
Panel A: Deal characteristics					
Dollar value of each syndicated loan (millions of dollars)	656.00	225.00	1,670.00	25.00	2,500.00
Total number of covenants	3.14	3.00	3.39	0.00	9.00
All-in Drawn Spreads (basis points)	206.48	187.50	146.95	40.00	375.00
Number of lenders	7.50	5.00	8.42	1.00	17.00
Number of local banks	1.79	1.00	2.79	0.00	5.00
Panel B: Connection measures					
Past school connections per syndicated loan	0.26	0.00	0.87	0.00	1.00
Third-party past professional connections per syndicated loan	1.28	0.00	4.15	0.00	4.00
Current social connections per syndicated loan	2.17	0.00	6.12	0.00	6.00
Deal in past 1–3 years indicator	0.16	0.00	0.36	0.00	1.00
Deal in past 4–6 years indicator	0.15	0.00	0.36	0.00	1.00
Deal in past 7 years or earlier indicator	0.10	0.00	0.31	0.00	1.00
Panel C: Firm characteristics					
Total Assets (millions of dollars)	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
M/B	1.81	1.34	2.83	0.95	2.93
Capital Expenditure/Total Assets	0.08	0.04	0.28	0.00	0.15
Expected Default Frequency (percent)	2.65	0.44	5.26	0.03	8.62
EDF Implied Spread (percent)	323.18	117.38	540.64	21.30	888.68

However, we proceed with the acknowledgment that superior specifications could provide even more informative estimates.

Our analysis involves bank loans made to publicly traded companies within the US, the majority of which are syndicated between multiple banks that share lending risk. See Sufi (2007) for a detailed discussion of the syndicated loan market. The source for these data is Dealscan, a proprietary product from Loan Pricing Corporation. 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.[9]

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.[10] For each facility, Dealscan lists a number of relevant firm and borrower characteristics including the amount loaned (or available as a line of credit), the identity of the firm and participant banks, the stated purpose of the loan, information about covenants, interest rate, maturity, and presence or absence of securitized collateral. Our main variables of interest are the rate charged (the all-in drawn spread), covenant variables, and deal size, which we analyze as functions of the preexisting personal connections between personnel at firm and syndicate banks. However, we employ the majority of the other available variables as controls. In Panel A of Table 1, we list a number of relevant summary statistics. Because these are standard, we omit their discussion.

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

(*footnote continued*)
formed via multiple channels (e.g., requiring two individuals to have overlapped in school and shared a common past employer), and defining connections only for the firm's CEO and CFO, rather than for the largest possible set of executives and directors made available by BoardEx. All results hold for each of these alternative specifications.

[9] For recent examples, see Bharath, Dahiya, Saunders and Srinivasan (2007) and Qian and Strahan (2007).

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

available, we examine various firm-level proxies instead. Two of these are very familiar: changes in public credit ratings and risk-adjusted stock returns, the former from Dealscan (Compustat also lists these) and the latter from the Center for Research in Security Prices (CRSP). Our distribution of credit ratings (not reported) is standard, with a modal value (BB) just below the investment-grade threshold. Hovakimian, Kayhan, and Titman's comprehensive study of credit rating targets (2009, Table 1) finds a nearly identical distribution (see Table 1).

Shown also in Table 1 (Panel C) are summary statistics for two proprietary credit risk measures made available to us from Moody's KMV: Expected Default Frequencies (EDFs) and EDF Implied Spread (EIS). [11] 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, and the second is a synthetic spread based upon the firm's Expected Default Frequency. Importantly, EDF Implied Spread is intended to predict spreads on bonds, not on senior bank debt. Thus, EDF Implied Spreads and All-in Drawn Spreads on bank debt are not directly comparable.

3. Personal connections and lending terms

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

3.1. Credit spreads

Unless a firm can issue riskless debt, the interest rate it pays will include a spread, usually quoted in basis points above LIBOR (London Interbank Offered Rate) or 10-year US Treasury yields. Dealscan employs the former benchmark. In our sample of syndicated bank deals, the average (median) spread is 206 (188) bps, indicating that if banks can borrow from other banks at 5%, then, over the same horizon, the average (median) firm can borrow at a statutory rate of 7.06% (6.88%).

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

However, in a regression that controls for other determinants of credit risk, this difference settles to approximately 28 basis points (Table 2, Column 1). As seen, this is comparable to shifts in credit ratings. For example, an improvement of two rating categories from A to AAA decreases borrowing costs by 174–144 = 30 bps, whereas a single upgrade from BBB to A reduces the interest rate by 144–102 = 42 bps.[12]

An important set of controls is the indicators for previous banking, but not personal, relations between the borrower and syndicate banks. Theories of financial intermediation have been advanced to predict both positive and negative effects on spreads for repeated firm-bank interactions. Boot and Thakor (1994) argue that when reusable information is generated in the process of originating a bank loan, subsequent spreads are lower because, effectively, the fixed costs of information production can be spread out over a larger number of transactions. However, banking relations can create or exacerbate hold-up problems (Hart and Moore, 1994), increasing the lender's market power.[13] Whether spreads decline over the course of a banking relationship is, thus, an empirical question recently taken up by Bharath, Dahiya, Saunders, and Srinivasan (2011), who find that repeated transactions are generally associated with reduced borrowing costs.

Following these authors, we include dummy variables for whether the borrower has transacted with at least one of the syndicate members in the last 3 years (t-3 through present), in the previous 3 years (t-6–t-4), or even further back (t-9–t-7). Confirming the findings of Bharath, Dahiya, Saunders, and Srinivasan (2011), Column 1 indicates that previous banking relations are 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 bps) less than half that of the firm-bank personal relationship indicator.[14]

Also included is the number of lenders in the syndicate (*Number of Lenders*), as well as the number of aggregate deals completed by the syndicate members in the previous year (*Number of Loans Offered by Syndicate Prior Year*). With these

[11] See Bohn and Crosby (2003) and Agrawal, Arora and Bohn (2004) for an overview of the methods behind the estimation of Expected Default Frequency and EDF Implied Spread. See Dvorak (2008) for discussion of the adoption of these credit risk measures in practice.

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

[13] However, analysis in DeAngelo (1981) suggests that more concentrated relationships (auditors rather than lenders in her study) could not increase market power in the way described. The reason is that a larger portfolio increases the incentive for the auditor to service any given client, which works in the client's favor. In the current context, this argument would imply an additional reason that spreads should decline over the course of a banking relationship, similar to the fixed cost argument.

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

Table 2
Firm-bank personal connections and interest rates.

The table relates the firm's borrowing cost, measured as its *All-in Drawn Spread*, to borrower and lender personal connections. Key control variables include a set of lender characteristics, loan characteristics, and macroeconomic conditions at time of loan origination. The *Connected Indicator* takes a value of one if there exists at least one school connection or third-party past professional connection between the borrower and any syndicate bank. *Deal in the Past 1–3 Years Indicator*, *Deal in the Past 4–6 Years Indicator, and Deal in the Past 7 Years or Earlier Indicator* take 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 3 years before that, or the three before that, respectively. The set of loan characteristic control variables include the logarithm of time until *maturity* [i.e., the tenor length in months; *Log(Maturity)*] and the *Number of Lenders* in the loan syndicate. The set of syndicate characteristic control variables include the total number of syndicated loan transactions conducted by the participating banks in the prior year (*Number of Loans Offered by Syndicate Prior Year*), and the number of local banks in the syndicate (*Local Bank Indicator*), in which local is defined as within 100 km of the headquarters of the borrower. The set of macro control variables include the levels and changes in default spread (the yield spread between BAA and AAA corporate bond indices), the level of and changes in term spreads (the yield spread between ten-year Treasury and three-month Treasury), and the most recent monthly returns of the Standard & Poor's 500 index. *Seniority Fixed Effect* indicates 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 and French 30-industry classifications to define industry dummy variables. Column 1 examines all loans; Columns 2, 3 and 4 examine high (A, AA, and AAA), medium (B, BB, and BBB), and low rating (CCC and below) loans, respectively, 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% level, respectively.

	Dependent variable: All-in Drawn Spreads				
	All loans (1)	High rating loans (2)	Medium rating loans (3)	Low rating loans (4)	Unrated Loans (5)
Connected Indicator	−27.68***	−8.452**	−20.12***	−51.11**	−46.52***
	(2.720)	(3.373)	(3.366)	(2.088)	(5.808)
AAA credit rating	−173.8***	−6.951			
	(8.628)	(6.381)			
AA credit rating	−161.2***				
	(8.000)				
A credit rating	−144.1***	8.272			
	(6.006)	(5.458)			
BBB credit rating	−102.3***		−110.0***		
	(5.467)		(5.384)		
BB credit rating	−44.24***		−43.75***		
	(5.192)		(4.666)		
B credit rating	−3.582				
	(5.053)				
CCC credit rating	−35.98***				
	(4.664)				
CC credit rating	15.55			35.31	
	(12.75)			(25.14)	
C credit rating	1.563			36.96	
	(37.17)			(37.78)	
Log(*Maturity*)	1.564	1.463	−0.0325	32.18	2.340
	(5.596)	(3.012)	(8.268)	(51.12)	(10.16)
Deal in past 1–3 years indicator	−13.30***	−0.651	−8.215**	−11.27	−19.37***
	(2.853)	(4.089)	(3.648)	(20.11)	(5.568)
Deal in past 4–6 years indicator	−7.361**	3.399	−9.694***	−14.13	−0.378
	(2.967)	(4.947)	(3.266)	(24.91)	(7.024)
Deal in past 7 years or earlier indicator	−6.773**	−3.151	−4.279	−40.18	−12.75*
	(3.043)	(2.503)	(3.983)	(26.98)	(6.845)
Number of lenders	0.207	−0.231	0.0587	−0.191	0.319
	(0.164)	(0.192)	(0.178)	(0.742)	(0.447)
Number of loans offered by syndicate prior year	−0.0210***	−0.00483***	−0.0177***	−0.0322***	−0.0305***
	(0.00111)	(0.00179)	(0.00129)	(0.0102)	(0.00242)
Local bank indicator	0.535	0.298	0.252	0.965	1.207
	(0.470)	(0.475)	(0.535)	(3.838)	(1.302)
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes
Seniority fixed effect	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes
Number of observations	17,428	1,705	8,666	359	6,698
Adjusted R^2	0.431	0.368	0.448	0.250	0.230

variables, we wish to model any size effects that could lead larger or more active syndicate banks to charge different rates of their borrowers.[15] As seen, the number of lenders does not appear significant, whereas more active banks charge somewhat lower spreads.

In addition, we collect for each borrower and syndicate bank their zip codes and, when available, calculate the distance between their respective headquarters. If located less than 100 km apart, the *Local Bank Indicator* takes a value of one and zero otherwise. We include this variable for two reasons. The first is that if information collection

[15] We also estimate each of our models with indicators for individual banks, with little change in the results. See Section 5 for these and other issues related to robustness.

or monitoring costs depend on proximity, then we want to account for these cost differences in our regressions. The second is that the main variables of interest, those relating to personal connections, could be highly correlated with the proximity between a bank and borrower. To make sure that firm-bank personal connections are not simply picking up common location, we model the latter explicitly. The *Local Bank Indicator* has a positive relation with spreads, which is similar to the findings in Degryse and Ongena (2005). The effect is small and statistically insignificant in Table 2 but is significant in other specifications (e.g., Table 3). Nevertheless, the inclusion of the *Local Bank Indicator* has little effect on the relation between personal connections and spreads.

Finally, we include a number of macroeconomic controls. Following Fama and French (1989) and Collin-Dufresne, Goldstein, and Martin (2001), we include the following five variables: Level of Term Spread (the difference between ten-year Treasury yield and three-month Treasury yield), One-Year Change in Term Spread, Default Spread (the difference between the Moody's Baa corporate bond index yield and the Moody's Aaa corporate bond index yield), One-Year Change in Default Spread, and One-Year Value-Weighted Return on the S&P 500 index. Generally, none of these provides 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 in which the borrower's credit rating is A or better (A, AA, or AAA) are shown in Column 2, which indicates that, on average, personally connected deals are perceived by syndicates as being less risky. The point estimate on the personal connections indicator is -8 bps, which, although small in an absolute sense, is almost 20% of the average spread for this group (mean 43 bps).

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

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

A potential criticism of the results of Table 2 is that although we have controlled for the probability of default with credit ratings, we have not accounted for differential recoveries given default. Because recoveries depend on industry and firm characteristics (for evidence, see Altman and Kishore, 1996; Acharya, Bharath, and Srinivasan, 2007), we include in Table 3 a number of firm- and industry-specific control variables likely to affect asset recoveries in liquidation. Along with dummies for each of the Fama and French (1989) 30 industry classifications, we also include each firm's lagged total assets [in logarithms; *Log* (*Total Assts*)]), market-to-book ratio (*M/B*), capital expenditures scaled by assets (*Capital Expenditure/Total Assets*), percentage of assets that are tangible (*Tangible/Total Assets*), profitability earnings before interest, taxes, depreciation, and amortization (EBITDA, scaled by assets; *Profitability*), and capital asset pricing model beta (*CAPM Beta*). If creditors account for the expected correlation of default losses with the aggregate market (Ross, 1985; Almeida and Philippon, 2007), we should expect a positive coefficient on the latter.

Requiring data availability for all of these variables substantially reduces the size of our sample, to just over 11 thousand observations. Summary risk measures are so important for predicting spreads, but because so many firms are not publicly rated, in Table 3 we account for default risk with Moody's KMV Expected Default Frequency for which we have more extensive coverage. We group firms into deciles of EDF and then include dummies for nine of these in the regressions. Including the numerical value of *EDF* makes almost no difference.

The first column of Table 3 shows the results. Although the coefficient on the personal connections indicator (Connected Indicator) drops somewhat, it remains highly significant, both statistically ($P < 0.001$) and economically (-18 bps).[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 firm-level variables either are, or border on being, statistically significant, with size, profitability, and market-to-book having the most predictive power.

For making causal inferences, it is important that personal connections are not simply capturing other firm attributes that could affect borrowing costs. In the second column, we include firm fixed effects and, thus, hold the

[16] The reduction in magnitude on the firm-bank personal connections indicator coefficient is primarily due to the changing of the sample (firms without Compustat data are more likely to be young, small, growth firms), not to the addition of new control variables.

Table 3
Firm-bank connections and loan spreads.

This table relates the firm's borrowing cost to borrower and lender personal connections. Key control variables include a set of borrower financial fundamentals, lender characteristics, loan characteristics, and macroeconomic conditions at time of loan origination. The *Connected Indicator* takes a value of one if there exists at least one school connection or third-party past professional connection between the borrower and any syndicate bank. The logarithm of this variable is self-explanatory. The dependent variables in columns 1 and 2 are the *All-in Drawn Spreads* reported by Dealscan. The dependent variables in columns 3 and 4 are the logarithm of the *All-in Drawn Spreads*. The set of borrower fundamental control variables include the capital asset pricing model (*CAPM Beta*) estimated using the past 3 years of monthly returns with a minimum of 18 monthly observations, logarithm of total assets [*Log(Total Assets)*], market-to-book ratio (*M/B*), capital expenditures normalized by lagged total assets (*Capital Expenditure/Total Assets*), tangible assets normalized by the lagged total assets (*Tangibility/Total Assets*), and profitability as of the most recent fiscal year end prior to the loan origination (*Profitability*). The set of loan characteristic control variables include the logarithm of time until maturity [i.e., the tenor length in months; *Log(Maturity)*], and the *Number of Lenders* in the loan syndicate. The set of syndicate characteristic control variables include the total number of syndicated loan transactions conducted by participating banks in the prior year (*Number of Loans Offered by Syndicate Prior Year*), and the number of local banks in the syndicate (*Local Bank Indicator*), in which local is defined as within 100 km of the headquarters of the borrower. The set of macro control variables include the levels and changes in default spread (the yield spread difference between BAA and AAA corporate bond indices), the level of and changes in term spread (the yield spread difference between ten-year Treasury and three-month Treasury), and the most recent monthly returns of the Standard & Poor's 500. Seniority Fixed Effect indicates whether the loan is explicitly secured, whether it is unsecured, or whether this information is missing in Dealscan. *EDF Decile Fixed Effec*t pertains to the set of dummy variables that take a value of one if the borrower's monthly Expected Default Frequency (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 and 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% level, respectively.

	Dependent variable: All-in Drawn Spreads		Dependent variable: log(All-in Drawn Spreads)	
	(1)	(2)	(3)	(4)
Connected Indicator	−17.77***	−17.29***		
	(3.431)	(3.704)		
Log (1+*Number of Connections*)			−0.134***	−0.0483***
			(0.0146)	(0.0118)
CAPM Beta	0.470	0.169	0.0186**	0.00545
	(1.611)	(1.899)	(0.00797)	(0.00832)
Log(*Total Assets*)	−4.338***	−13.36**	−0.0687***	−0.0548**
	(1.621)	(5.415)	(0.0120)	(0.0221)
M/B	−1.517**	−3.704**	−0.0199***	−0.0231**
	(0.682)	(1.704)	(0.00637)	(0.0100)
Capital Expenditure/Total Assets	−1.448	29.89*	−0.0110	0.162**
	(15.65)	(17.87)	(0.0756)	(0.0678)
Tangibility/Total Assets	−6.505	1.063	−0.0290	0.0213
	(4.387)	(5.087)	(0.0233)	(0.0251)
Profitability	−31.05***	−75.12***	−0.136**	−0.388***
	(8.608)	(17.01)	(0.0602)	(0.0750)
Log(*Maturity*)	11.04**	5.328	0.112***	0.0184
	(5.156)	(4.918)	(0.0280)	(0.0235)
Deal in past 1–3 years indicator	−3.446	1.088	−0.0199	0.000936
	(3.344)	(3.313)	(0.0204)	(0.0189)
Deal in past 4–6 years indicator	−3.087	0.510	0.0151	0.0204
	(3.158)	(3.141)	(0.0192)	(0.0162)
Deal in past 7 years or earlier indicator	−8.134**	−9.701***	−0.0202	−0.0469**
	(3.412)	(3.363)	(0.0231)	(0.0200)
Number of loans offered by syndicate prior year	−0.0175***	−0.0121***	−8.82e-05***	−7.46e-05***
	(0.00150)	(0.00142)	(9.63e-06)	(7.71e-06)
Local bank indicator	2.149***	2.548***	0.00935**	0.0128***
	(0.618)	(0.679)	(0.00393)	(0.00330)
Number of lenders	−0.200	−0.845***	0.00213	−0.00393**
	(0.300)	(0.289)	(0.00221)	(0.00157)
Macroeconomic controls	Yes	Yes	Yes	Yes
Seniority fixed effect	Yes	Yes	Yes	Yes
EDF decile fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	No	Yes	No
Firm fixed effect	No	Yes	No	Yes
Number of observations	11,003	11,003	11,003	11,003
Adjusted R^2	0.504	0.745	0.615	0.860

borrower constant but vary the lending syndicate. This procedure admits only the set of firms that complete at least one deal with a connected syndicate and at least one with a nonconnected syndicate.

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

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

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

3.2. *Covenants*

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

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

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

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

For robustness, shown in the next columns are results from linear regressions, in which the dependent variable is the number of covenants included (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.

3.3. *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 which adverse selection and managerial incentive problems are likely the greatest) benefit the most. Here, we consider whether the effects we find apply only to small loans, or whether they generalize to larger stakes.

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

Estimates in the first and second columns suggest that increasing the number of firm-bank personal connections increases the amount lent. The log–log specification indicates an elasticity of roughly 3.5 (Column 1). The discrete specification (Column 2) shows that compared with deals lacking personal connections, syndicated deals among personally connected members are over 13% larger, translating to roughly $45 million on average. In the final two columns of Panel B with firm fixed effects, both specifications indicate a strong, positive relation. Compared with the specification in Column 1, the inclusion of firm fixed effects slightly strengthens the result. The elasticity is 0.076, indicating that 1.5 additional connections increase the average loan balance by more than $40 million. The discrete model shown in the final column indicates a slightly strengthened effect for

Table 4
Firm-bank connections, loan covenants and loan sizes.

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

Panel A: Firm-bank connections, loan covenants

	Dependent variable: *Covenant Indicator*		Dependent variable: *Number of Covenants*	
	(1)	(2)	(3)	(4)
Connected Indicator		−0.0124		0.0715
		(0.0146)		(0.112)
Log (1+ *Number of Connections*)	−0.0226***		−0.112*	
	(0.00829)		(0.0634)	
Firm characteristics controls	Yes	Yes	Yes	Yes
Loan characteristics controls	Yes	Yes	Yes	Yes
Bank characteristics controls	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes
Seniority fixed effect	Yes	Yes	Yes	Yes
EDF decile fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	No	No
Firm fixed effect	No	No	Yes	Yes
Number of observations	11,964	11,964	11,964	11,964
Pseudo (Columns 1 and 2) or Adjusted (Columns 3 and 4) R^2	0.378	0.377	0.678	0.678

Panel B: Firm-bank connections and loan sizes

	log(Tranche Amount)	log(Tranche Amount)	log(Tranche Amount)	log(Tranche Amount)
	(1)	(2)	(3)	(4)
Connected Indicator		0.134***		0.147***
		(0.0318)		(0.0347)
Log (1+ *Number of Connections*)	0.0352*		0.107***	
	(0.0205)		(0.0208)	
Firm characteristics controls	Yes	Yes	Yes	Yes
Loan characteristics controls	Yes	Yes	Yes	Yes
Bank characteristics controls	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes
Seniority fixed effect	Yes	Yes	Yes	Yes
EDF decile fixed effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	No	No
Firm fixed effect	No	No	Yes	Yes
Number of observations	11,964	11,964	11,964	11,964
Adjusted R^2	0.652	0.653	0.812	0.812

personal connections on loan balances, compared with the model without firm effects.

4. Ex-post performance

The results of Section 3 indicate that firm-bank personal connections shift lending terms in the borrower's favor but are silent with respect to the reasons why. Holding risk constant, more lenient terms would result in a wealth transfer from the bank to the firm's shareholders. However, if firm-bank connections alter the risk profile of the borrower – either by mitigating adverse selection problems or improving the bank's ability to monitor and alleviate borrower's moral hazard incentives – then the concessions shown in Tables 2–4 could be warranted. The ideal test would be to compare default rates between connected and unconnected syndicates. Unfortunately, Dealscan does not provide data on the performance of individual loans, and because the secondary market for such securities is extremely illiquid, examining prices is not feasible. Absent performance data on specific loans, we examine various firm-level performance metrics that, while noisy, nevertheless provide information about the firm's ability to service its debt obligations: credit ratings,

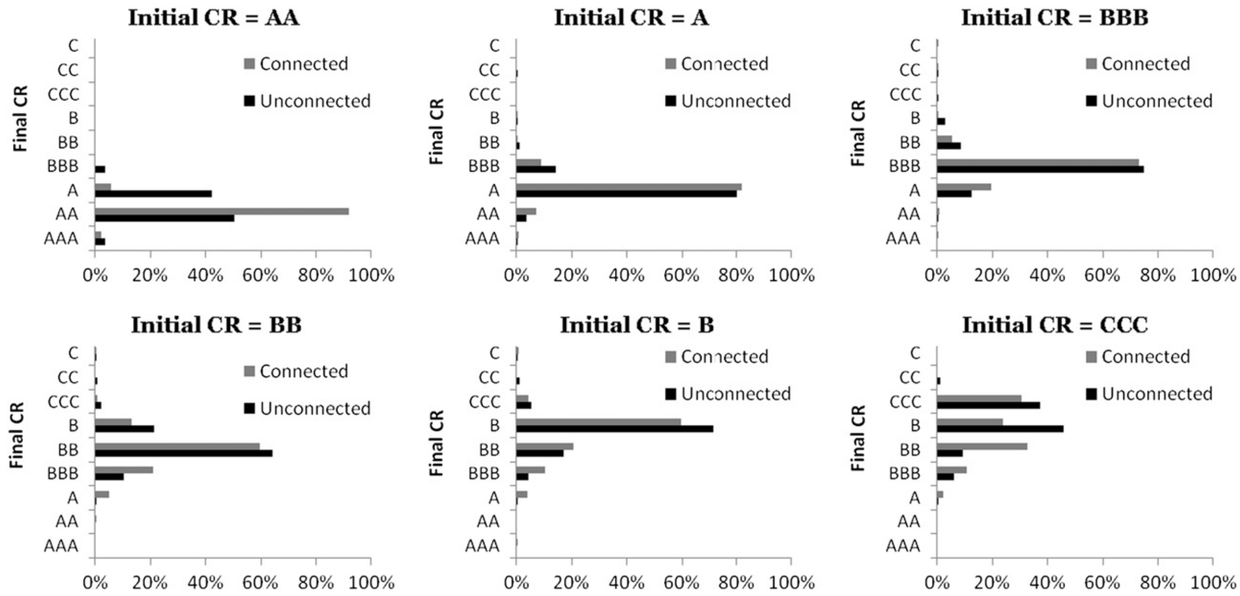

Fig. 1. Credit ratings (CRs) evolution for connected and unconnected firms. This figure shows the evolution of long-term public debt ratings. Ratings for firms that complete loans with personally connected banking syndicates (at least one school or third-party past professional connection) are shown in gray; those of their counterparts borrowing from nonconnected syndicates are shown in black. Initial credit ratings are those as of the loan's start date. Final ratings correspond to those as of July 2009. Firms with initial ratings above AA or below CCC are omitted due to a small number of observations.

Expected Default Frequencies, EDF Implied Spreads, and stock returns. All of these are benchmarked to the date of the syndicated deal and tracked forward.

4.1. Future credit ratings

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

Before proceeding, we note one important change to the sample. In Section 3, 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% and a subordinated \$300 million line of credit at 8%. Following Bharath, Sunder, and Sunder (2008), we treated these as independent observations in our previous analysis. However, while the fact that loan characteristics vary across tranches justifies their inclusion in the previous application, this is clearly inappropriate when examining firm-level performance. Even if a firm borrows against multiple lines of credit within the same loan package, this clearly constitutes only one independent observation for the firm's ex post performance. Collapsing at the package level reduces the sample by about 20%, relative to that analyzed in Section 3.[17]

In Fig. 1, we compare the evolution of future credit ratings following personally connected deals (gray bars), to that following unconnected deals (black bars). Initial credit ratings are shown in each panel, starting with rating category AA. Final ratings are those as of July 2009. The striking differences between the black and gray bars in Fig. 1 underscore the importance of personal connections as an *ex ante* indicator of deal quality. As seen, the credit ratings of connected (unconnected) firms tend to drift upward (downward) or remain the same.

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

Table 5 puts these univariate patterns in a regression framework. The first, second, and third pairs of columns, respectively, track credit ratings changes at the 12-, 24-, and 36-month interval after the initiation of a syndicated bank deal. In each case, the dependent variable is a discrete indicator *Credit Rating Downgrade*, taking a value of one if the firm is subsequently downgraded (e.g., BBB to BB or below) and zero otherwise.[18]

[17] The results in Section 3 are nearly identical in each specification.

[18] Considering separately upgrades, downgrades, and no change in a single ordered probit specification yields similar qualitative predictions, but for ease of interpretation, we use the binary specification and

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

	Credit rating downgrade: future 12 months		Credit rating downgrade: future 24 months		Credit rating downgrade: future 36 months	
	(1)	(2)	(3)	(4)	(5)	(6)
Connected Indicator	−0.0226***		−0.0561***		−0.0724***	
	(0.00753)		(0.0113)		(0.0145)	
Log (1+ *Number of Connections*)		−0.0104***		−0.0142**		−0.0186**
		(0.00400)		(0.00586)		(0.00755)
Firm characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
EDF decile fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	5,758	5,758	5,154	5,154	4,255	4,255
Pseudo R^2	0.089	0.089	0.106	0.101	0.122	0.117

As seen in Columns 1, 3, and 5, the presence of at least one personal connection has a dramatic effect on the future trajectory of credit rating changes. With each passing year, connected firms are about 2.5% less likely to be downgraded than their unconnected counterpart borrowers. By the third year, the effect is over 7% and is significant at far better than the 1% level. In the second, fourth, and sixth columns, the logarithmic specification also significantly predicts downgrades, and more so at longer horizons.

4.2. EDFs and EISs

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

Comparing Table 6, Panel A, Columns 1 (12 months), 3 (24 months), and 5 (36 months), the presence of firm-bank personal connections remains an important predictor of Expected Default Frequency over each window. As in Table 5, the effect pronounces at longer horizons. For example, in the 36-month period shown in Column 5, firm-bank personal connections are associated with almost a three-fourths unit decrease in EDF. To put this in perspective, the average firm has an EDF of 2.71, which would correspond roughly to a BB rating. A unit shift of EDF in either direction would move the corresponding credit rating approximately one-half a rating category. The logarithmic specification for connections is somewhat weaker from a statistical significance perspective. However, all the point estimates are negative, and the final column is significant at the 5% level.

A similar picture emerges in Panel B, where each firm's future EIS is modeled as a function of firm-bank personal connections, along with the usual set of control variables. The first column indicates that even controlling for the firm's initial EIS, the presence of personal connections to syndicate members reduces its future, expected borrowing cost by 49 basis points 12 months in advance. By 24 months, the expected reduction is 77 basis points, in the neighborhood of being upgraded from junk (BB or worse) to investment grade (BBB or worse). At 3 years, the marginal effect is 80 basis points. As in the EDF regressions, the log specification (Columns 2, 4, and 6) is not as strong but paints largely the same picture.

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

4.3. Stock returns

The three dependent variables considered so far – future credit ratings, *Expected Default Frequency*, and *EDF Implied Spread* – are all explicitly designed to evaluate the

(*footnote continued*)
present marginal effects. For firms completing deals in the latter part of our sample, not enough time has passed for their future credit ratings to be analyzed (e.g., a firm borrowing in December 2007 does not, at the time of writing, have a 36 months ahead rating).

Table 6
Connections and alterative measures of future credit risk.

The table relates future *Expected Default Frequency* (EDF, Panel A) and *EDF Implied Spread* (EIS, Panel B) to borrower and lender past connections, a set of borrower fundamentals, lender characteristics, and macroeconomic conditions at the time of loan origination. The set of control variables is the same as those reported in Table 3. The *Number of Connections* describes the sum of current school connections and third-party past professional connections. The reference date is when the syndicated deal is initiated. Robust standard errors clustered by firm are in parentheses. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively.

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

	Dependent variable: *Expected Default Frequency*					
	EDF 12 months-ahead		*EDF* 24 months-ahead		*EDF* 36 months-ahead	
Connected Indicator	−0.427***		−0.741***		−0.734***	
	(0.104)		(0.177)		(0.211)	
Log (1+ *Number of Connections*)		−0.140**		−0.215*		−0.311**
		(0.0599)		(0.124)		(0.135)
Current *EDF*	0.636***	0.637***	0.366***	0.368***	0.228**	0.229**
	(0.0659)	(0.0659)	(0.0784)	(0.0784)	(0.0898)	(0.0897)
Firm characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
EDF decile fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,082	9,082	8,192	8,192	6,819	6,819
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

	Dependent variable: *EDF Implied Spread*					
	EIS 12 months-ahead		EIS 24 months-ahead		EIS 36 months-ahead	
Connected Indicator	−49.27***		−77.23***		−80.39***	
	(11.43)		(19.49)		(24.36)	
Log (1+ *Number of Connections*)		−18.96***		−22.24		−34.97**
		(6.577)		(14.20)		(15.49)
Current *EDF Implied Spreads (EIS)*	0.525***	0.527***	0.357***	0.359***	0.203***	0.203***
	(0.0572)	(0.0572)	(0.0612)	(0.0612)	(0.0631)	(0.0629)
Firm characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
EDF decile fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,071	9,071	8,181	8,181	6,804	6,804
Adjusted R^2	0.519	0.518	0.333	0.332	0.256	0.254

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 predictable from other information not captured in our regressions. It is important to note, however, that tests of firm-bank connectivity for future stock returns are joint tests. They test whether the information in connected or unconnected deals is value-relevant for equity prices and whether the market impounds this information immediately into prices.[19]

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

The first two columns of Panel A indicate that, over a one-year window, only suggestive evidence exists that stock returns of connected borrowers are higher than those of their unconnected counterparts. Both point estimates are positive, but the standard errors are relatively large by comparison. In the third and fourth columns, we see stronger evidence that returns are predictable from a firm's personal connectedness to its

[19] In an efficient market in which all personal connections were publicly available at the time of the loan, we would not expect predictability for stock returns following connected or unconnected deals.

Table 7
Connections and future stock returns.

The table relates future stock returns of the borrower to borrower and lender personal connections, a set of borrower financial fundamentals, lender characteristics, and macroeconomic conditions at time of loan origination. The dependent variable in both panels is the cumulative Daniel, Grinblatt, Titmann and Wermers (1997) characteristic-adjusted returns 12, 24, and 36 months after loan origination. The set of control is the same as those reported in Table 3. The Number of Connections describes the sum of current school connections and third-party past professional connections. The reference date is when the syndicated deal is initiated. Panel A shows the results of time-series cross-sectional regressions and Panel B shows the results of (monthly) Fama and MacBeth regressions. Robust standard errors clustered by firm are reported in Panel A and Fama and MacBeth standard errors are reported in Panel B. *, **, and *** represent significance at the 10%, 5%, and 1% level, respectively.

Panel A: Connections and firm's future cumulative returns, time-series cross-sectional regressions

	Dependent variable: *Return at Different Horizons*					
	12-months ahead		24-months ahead		36-months ahead	
Connected Indicator	0.0344*		0.106***		0.170***	
	(0.0189)		(0.0312)		(0.0430)	
Log (1+ *Number of Connections*)		0.0164*		0.0499***		0.0743***
		(0.00886)		(0.0149)		(0.0206)
Firm characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
EDF decile fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Industry fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,113	9,113	9,113	9,113	9,113	9,113
Adjusted R^2	0.025	0.025	0.037	0.036	0.051	0.049

Panel B: Connections and firm's future cumulative returns, Fama and MacBeth regressions

	Dependent variable: *Return at Different Horizons*					
	12-months ahead		24-months ahead		36-months ahead	
Connected Indicator	0.0491**		0.1243***		0.2107***	
	(0.0239)		(0.0302)		(0.0519)	
Log (1+ *Number of Connections*)		0.0280***		0.0823***		0.1168***
		(0.0104)		(0.0152)		(0.0248)
Firm characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank characteristics controls	Yes	Yes	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes	Yes	Yes
EDF decile fixed effect	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,113	9,113	9,113	9,113	9,113	9,113
Average Cross-Sectional R^2	0.076	0.067	0.055	0.051	0.059	0.050

syndicate members. The log specification indicates that doubling the number of personal connections increases the firm's risk-adjusted stock returns by almost 5% ($P<0.001$). The discrete specification effectively compares connected versus unconnected deals and indicates a two-year, risk-adjusted difference of over 10%. The final two columns show that, at the three-year horizon (we use the most recent stock price if 3 years have not passed), connected borrowers perform 17% better than borrowers not personally connected to their syndicates ($P<0.001$). Annualized, this corresponds to a risk-adjusted (excess) return of 5.6%.

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

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

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

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

5. Robustness and other considerations

5.1. Connection types

Because we wish to make causal inferences between personal relationships and lending behavior, we consider connections formed only at third-party venues (school or other firms or banks not involved in the deal analyzed) and at least 5 years prior to the deal of interest. The time restriction is imposed to rule out any reverse causality, such as membership in social organizations being a reward for a favorable banking deal. Practically, this means that we ignore the majority of the possible connections we can infer. Connections exist not only from common schooling institutions or past workplaces, but also from active roles in common social organizations, e.g., think tanks (Council on Foreign Relations), charities (Saint Agnus Foundation), nonprofit organizations (National Urban League), and philanthropies (Boston Science Museum). Including such connections confers a marked increase in statistical power. Through sheer size, connections formed within the universe of social organizations far outnumber those formed via common schooling institutions and third-party workplaces. However, without being able to identify the specific dates when such social relationships are formed (and thus leaving them vulnerable to the reverse causality critique), we cannot defend their inclusion in our main analysis.

With this caveat in mind, we break up our existing connection measure into its components (*Third-Party Past Professional Connection Indicator* and *School Connection Indicator*) and to it add *Social Connection Indicator* in the first two columns of Table 8. As before, we include both the discrete (Column 1) and logarithmic (Column 2) specifications. In both columns, we see that all three varieties are negatively related to credit spreads, with the Social Connections Indicator having the largest point estimate (-13 bps versus -9 for the other types).

Given the strong result for social connections, it is tempting to formulate causal explanations for the impact of social connections on spreads similar to that for the other types of connections. One could argue that because common social organizations provide a natural venue for relationships to persist into the future (school and third-party past professional connections have no comparable venue), that they would be particularly costly to damage. In connected deals in which such valuable social relationships are effectively pledged as collateral, we might expect larger marginal effects on credit spreads. While consistent with the evidence in Table 8, so, too, is the possibility for banking transactions to influence – instead of being 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.

5.2. 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 are comparatively scarce. In situations in which 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. This is less applicable to bank financing, where the ability to screen and monitor borrowers could differ considerably between banks. To the extent that such differences are correlated with our connection measures, the coefficients we report could be biased.

Perhaps the most obvious possibility is that larger 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 is larger.[20] In the third column of Table 8, we exclude from consideration any deal in which any of the five most active banks was a participant. As seen, this restriction has an enormous impact on the number of observations (11,003 in Table 3 versus 3,948 in Table 8, Column 3), reflecting the ubiquity of the most active commercial banks. Nonetheless, even when the largest banks are absent, the effect of firm-bank personal connections survives. The coefficient reported in Table 8 (0.13) is nearly identical to the full sample (0.12), and remains highly significant ($P < 0.001$). Similar magnitudes are observed if the sample is cut even further, but as the

[20] We have already addressed this possibility in some detail previously, having controlled for the number of lenders in the syndicate as well as the aggregate lending activity of its member banks in all regressions.

Table 8
Loan spreads and alternative definitions of connections.

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

	Dependent Variable: All-in Drawn Spreads	Dependent Variable: Log(All-in Drawn Spreads)		
	(1)	(2)	(3)	(4)
Past School Connections	−9.152***			
	(2.988)			
Third-Party Past Professional Connections	−8.723**			
	(3.415)			
Current Social Connections	−13.92***			
	(3.411)			
Log (1 + *Number of School Connections*)		−0.0699**		
		(0.0295)		
Log (1 + *Number of Professional Connections*)		−0.128***		
		(0.0161)		
Log (1 + *Number of Social Connections*)		−0.0410***		
		(0.0144)		
Log (1 + *Number of Connections*)			−0.126***	−0.128***
			(0.0363)	(0.0140)
Firm characteristics controls	Yes	Yes	Yes	Yes
Loan characteristics controls	Yes	Yes	Yes	Yes
Bank characteristics controls	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes
Seniority Fixed Effect	Yes	Yes	Yes	Yes
EDF Decile Fixed Effect	Yes	Yes	Yes	Yes
Year fixed effect	Yes	Yes	Yes	Yes
Industry Fixed Effect	Yes	Yes	Yes	Yes
Firm characteristics controls	Yes	Yes	Yes	Yes
Top 20 Bank Fixed Effect	No	N0	No	Yes
Number of observations	11,003	11,003	3,948	11,003
Adjusted R^2	0.506	0.622	0.457	0.639

number of observations decreases, so, too, does the ability to make statistical inferences.

The fourth column again considers the full sample but includes fixed effects for each of the 20 most active banks, defined by the number of deals in the previous year (84% of our observations include at least one of these banks). Notably, their inclusion increases the explanatory power increases almost 2 percentage points, indicating the presence of lender-specific attributes on credit spreads. However, the effect of bank-firm personal connections remains virtually unchanged compared with the previous column or to Table 3, indicating an elasticity of slightly over 0.12 ($P < 0.001$). Other unreported robustness checks include a larger number of fixed effects, or interacting previous years' activity with firm-bank personal connections, none of which has a meaningful effect on the variable of interest.

5.3. Measurement error

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

5.4. The Higher bar hypothesis

We know from Tables 5 to 7 that connected borrowers perform better ex post and from Tables 2 to 4 that this superiority is, at least partially, reflected by better deal terms. However, this evidence alone does not necessarily tell us anything about efficiency, i.e., whether personal connections improve lending decisions. It could be the case that personal relationships harm efficiency, but in such a way that still generates the empirical patterns we observe.

To appreciate this possibility in more detail, consider the following simple model.[21] There are two types of banks, those personally connected (C) to their borrowers and those not (N). Moreover, four types of firms request debt financing: 1, 2A, 2B, and 3. Type 1 and 3, good and bad firms, respectively, are easy to evaluate. Type 1 firms are always

[21] We thank Jeremy Stein for first suggesting the higher bar alternative and the simple model that illustrates its intuition.

awarded credit (by either C or N), and type 3 firms are always denied. By contrast, types 2A and 2B are harder to evaluate. Perhaps their information is softer or their managerial quality difficult to evaluate. In any case, 2A firms are positive net present value (NPV), and 2B firm are negative NPV.

Now consider what would happen if personal relationships solved the adverse selection problem faced by potential lenders. C banks, by virtue of their personal connections, could distinguish between 2A (good) firms and 2B (bad) ones, and they would lend only to the former. N banks, lacking the information required to make this distinction, would either always or never lend, depending on the parameter values. In either case, it is easy to see how the performance of a C bank's borrowers could exceed that of an N bank: Good firms are always awarded credit, and bad firms are always denied.[22] This is consistent with the evidence in the paper that connected borrowers perform better ex post and are awarded better deal terms ex ante.

Now consider an alternative possibility: Connections impart no special information, meaning that, like N banks, C banks cannot distinguish between types 2A and 2B firms. However, what if C banks are wary of the perception of corruption and, consequently are reluctant to lend to firms that require a subjective evaluation—namely, all type 2 firms? In this case, C banks loan only, or mostly, to type 1 firms, which are objectively creditworthy. Assuming that N banks still find it profitable to loan to type 2 firms, and that the typical type 2 borrower is inferior to the typical type 1 borrower, the ex post performance of C's borrowers exceed that of N's borrowers.

This is obviously not a formal model, but it does illustrate how both an information story (where connections improve lending decisions) and a higher bar story (where they do not) are difficult to distinguish based on ex post performance alone. In the former, private information is generated equally to all potential lenders, but connected borrowers simply use it differently. In the latter, personal connections change the information set of lenders. Clearly, teasing out whether connections create private information or whether they effectively parse deals by levels of existing private information is difficult. Predictions for ex post performance outcomes are similar.

The higher bar story, in its simplest form, represents a friction for borrowers, with no offsetting benefit. Thus, if there is a cost of applying for a loan, then any uncertainty about clearing the bar means that firm have an incentive to avoid personally connected banks. In an extreme case, we should see only (or mostly) arm's-length transactions and, regardless of how we define social connections in the current context, this is counterfactual. Unreported results indicate that personal relationships greatly increase the probability of a deal occurring, implying either that firms are more likely to approach cozy borrowers or are more likely to be approved once they do apply (maybe both). Unlike the higher bar story, this is exactly what the information story would predict.

[22] If N firms find it profitable to lend to any type 2 firm, then C banks differ only by winnowing out 2B firms. If N firms stay away from type 2 firms altogether, then the same result requires that the typical 2A firm be more attractive than the typical type 1 firm.

6. Conclusions

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

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

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

Taken together, the evidence here identifies personal relationships as a technology that allows banks to excel in problems situations, in which a borrower's creditworthiness is difficult to evaluate or when active monitoring is required (Diamond, 1984, 1991; Fama, 1985). Examples of this technology at work are microcredit groups such as the Grameen Bank of Bangladesh. 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 or in renegotiation, we leave to future work.

Appendix A

Variable definitions and constructions are given in Table A1.

Table A1
Variable definitions and construction.

Variable name	Variable definitions and constructions	Source of data
All-in Drawn Spreads	All-in drawn spreads of each tranche	Dealscan
Capital Expenditure/Total Assets	*Capital Expense* (t)/*Total Assets* (t-1)	Compustat
CAPM Beta	Beta estimate from the capital asset pricing model (CAPM), using the past 36 months of monthly returns, with a minimum of 18 months of return data	CRSP
Change of Default Spread	Change of default spread between current month and prior month	Federal Reserve
Change of Term Spread	Change of term spreads between current month and prior month	Federal Reserve
Characteristics-Adjusted Return, 12 Months Ahead	Cumulative Daniels, Grinblatt, Titmann, and Wermers (1997; DGTW) characteristic-adjusted return 12 months ahead, beginning at the month immediately after the deal	CRSP, /Compustat
Characteristics-Adjusted Return, 24 Months Ahead	Cumulative DGTW characteristic-adjusted return 24 months ahead, beginning at the month immediately after the deal	CRSP, Compustat
Characteristics-Adjusted Return, 36 Months Ahead	Cumulative DGTW characteristic-adjusted return 36 months ahead, beginning at the month immediately after the deal	CRSP, Compustat
Deal in Past 1–3 Years Indicator	Dummy variable that takes the value of one if the firm borrows from a bank in the syndicate during the prior three years	Dealscan
Deal in Past 4–6 Years Indicator	Dummy variable that takes the value of one if the firm borrows from a bank in the syndicate during the four to 6 years before the current year	Dealscan
Deal in Past 7 Years or Earlier Indicator	Dummy variable that takes the value of one if the firm borrows from a bank in the syndicate more than 6 years before the current year	Dealscan
Default Spread	Difference between Moody's BAA corporate bond index yield and Moody's AAA corporate bond index yield	Federal Reserve
EDF Decile Fixed Effect	Dummy variable that equals one if the EDF value falls into one of the ten EDF deciles, in which EDF deciles are defined over the cross-sectional EDF values within the month	Moody's KMV
EDF Implied Spread (EIS)	Product of the estimated Expected Default Frequency and the estimated expected loss given default (LGD)	Moody's KMV
Expected Default Frequency (EDF)	Computed and calibrated to actual default events by Moody's KMV (see Crosbie and Bohn, 2003, for details)	Moody's KMV
Idiosyncratic Volatility	Residual standard deviation of the estimate from the capital asset pricing model using the past 36 months of monthly returns, with a minimum of 18 months of return data	CRSP
Industry Fixed Effect	Industry fixed effect, where the industry classification is defined by Fama and French (1997) 30-industry classifications	CRSP
Level of Term Spread	Difference between the ten-year Treasury yield and the three-month Treasury yield	Federal Reserve
Local Bank Indicator	Dummy variable that takes the value of one if a syndicate member bank is located within 100 km of the borrower's headquarters and zero otherwise	Hand-collected
Log*(Maturity)*	Logarithm of tenor length	Dealscan
Log*(Total Assets)*	Logarithm of *Total Assets* (AT) at (t)	Compustat
M/B	Market value of equity/book value of equity	CRSP, Compustat
Number of Lenders	Number of lenders within each syndicate	Dealscan
Number of Loans Offered by Syndicate Prior Year	Total number of nonoverlapping loans offered by syndicate members during the prior year	Dealscan
Profitability	Operating Income Before Depreciation (t)/*Total Assets* (t-1)	Compustat
Return [t-1, 0]	Cumulative past 12-month raw return	CRSP
Return [t-3, t-2]	Cumulative past 36-month raw return excluding the most recent 12-month return	CRSP
Seniority Fixed Effect	Dummy variable that takes the value of one if the loan is a senior loan, and zero otherwise	Dealscan
Tangibility/Total Assets	[Property, Plant, and Equipment (PP&E) + Inventory] (t)/*Total Assets* (t-1)	Compustat

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