The Role of Bankers in the U.S. Syndicated Loan Market*

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Abstract

I construct a novel dataset linking individual bankers to large borrowers in the U.S. syndicated loan market to analyze the impact of bankers for the largest, most transparent borrowers. Banker fixed effects explain a sizeable fraction of loan terms and exhibit more explanatory power than bank fixed effects. Bankers also form personal relationships with borrowers. Stronger personal relationships are associated with significantly lower interest rates, even after controlling for the endogenous nature of relationship-formation. Relationship loans are associated with fewer subsequent bankruptcies and no favorable modifications in renegotiations, suggesting personal relationships lead to superior information for banks rather than nepotism.

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1 Introduction

Syndicated loans, which are jointly funded by two or more lenders, amount to more than $4 trillion and are a primary source of capital for U.S. corporations (Dennis and Mullineaux, 2000; Sufi, 2007; Ivashina, 2009).[1] In syndicated loans, a lead bank negotiates the primary loan terms with the borrower and subsequently forms a syndicate of participating lenders who jointly provide the required funds. Syndicated loans share some characteristics with ordinary loans to small and medium sized borrowers, such as that lead banks and borrowers establish a relationship through repeated interaction. These relationships have both a significant financial and real impact on borrowers (Ivashina and Scharfstein, 2010; Chodorow-Reich, 2014). On the other hand, the large size of syndicated loans, the wide availability of information on borrowers, and the shared commitment by multiple lenders is different from loans to smaller, more opaque borrowers.

In this paper, I examine the role of individual bankers for setting loan terms, building relationships with clients, and matching borrowers to banks in the syndicated loan market. While the importance of individual bankers has been widely documented in the setting of small, opaque borrowers, there is no evidence that bankers play a role for large, transparent borrowers. The large amounts of publicly available information about borrowers in the U.S. syndicated loan market and their ability to access alternative capital markets would speak against individual bankers playing a big role in this segment. Yet, I find that individual bankers play a key role in the syndicated loan market.

I construct a novel, hand collected dataset from publicly available loan contracts linking individual bankers to specific loans to study the role of bankers in the U.S. syndicated loan market. The syndicated loan market is a particularly promising empirical laboratory in which to study the effect of individual bankers on bank loans since borrowers tend to be large firms with lots of publicly available data, such as audited financial statements or

[1] Shared National Credit Program report for the first quarter of 2016, available at goo.gl/NqZuZ6. The Shared National Credit Program covers loans which feature at least three supervised lenders and exceed $20 million.

analyst reports. The prior literature on the role of bankers in lending focuses on character loans made to small, opaque borrowers. For those character loans, individual bankers are often the only source of information available to banks. By examining large borrowers with ample publicly available information, I am able to pin down the personal effect of bankers. Bankers and their relationships with clients should matter least for borrowers with large amounts of public, high-quality information: Theory predicts that under perfect information, there should be a unique loan contract for each borrower, and, empirically, firms with more publicly available information are less likely to self-select into relationship lending (Sufi, 2007; Bharath, Dahiya, Saunders, and Srinivasan, 2011). I show that even for the largest, most transparent borrowers, individual bankers have a key role in setting loan terms.

I begin my analysis by investigating time-invariant banker effects on loan conditions. Regressions of loan characteristics on explanatory variables including banker and bank fixed effects reveal that banker fixed effects can explain a significant portion of the variation in loan terms, such as loan size and loan pricing. Banker fixed effects account for between 10 and 25 percent of the variation in various loan characteristics and explain up to two and a half times as much variation as do bank fixed effects.

Time-invariant banker fixed effects can stem from a number of channels: On the one hand, they could reflect individual skill, experience or preferences. On the other, banker fixed effects might simply reflect some unobservable bank internal matching of bankers to specific clients. To rule out that unobservable matching of bankers and borrowers is the sole driver of the role of bankers in lending, the second set of results in the paper focuses on one specific channel through which bankers impact loans: The time-varying effect of bankers on loans through their personal relationships with clients. As bankers and borrowers repeatedly interact they form relationships. I investigate the role of these personal relationships between bankers and borrowers, while carefully addressing the endogenous nature of personal relationships. Lower quality borrowers can self-select into relationships (Sufi, 2007), and personal relationships between bankers and borrowers develop simultaneously to institutional relationships between banks and borrowers which impact lending terms (Bharath et al., 2011). I therefore obtain identification of the impact of personal relationships on lending by exploiting shocks to those relationships from banker turnover. If a relationship banker leaves her employer, her clients experience a shock to their personal relationship strength, while the clients' institutional relationships remain intact. I find that loans granted by bankers with strong personal relationships with a borrower exhibit significantly lower interest rates than do comparable loans. I estimate that a one-standard-deviation increase in personal relationship strength, measured as the number of interactions between a banker and a borrower, is associated with 11 basis points lower interest rates. The economic magnitude of this effect is large and corresponds to annual savings of $275,000 at the median loan size of $250 million. These results demonstrate two things: First, they shed light on one specific economic channel through which bankers impact lending, namely through their personal relationships with borrowers. Second, they address the concern that the impact of bankers could be the result of unobservable, time invariant matching between bankers and borrowers. Since any such matching is stable over time, it cannot explain the effect of changing personal relationships.

Two economic channels could explain why loans with strong personal relationships feature lower interest rates. First, bankers could gather superior information throughout the course of their relationship with borrowers, thereby reducing information asymmetry and leading to a lower spread. Alternatively, those lower rates could be the result of agency conflicts between the bank and its employees. Borrowers might reward bankers for lower interest rates, either directly through monetary kickbacks or indirectly, for example through invitations to social events. If bankers received personal gains in return for granting cheap loans, lower interest rates would reflect nepotism, rather than superior information. The two competing explanations can be tested by comparing the subsequent performance of relationship loans with that of nonrelationship loans. If lower interest rates are the result of nepotism, loan performance should be worse for strong relationship loans. If, on the other hand, lower interest rates are the result of superior information, strong relationship loans will be associated with superior subsequent loan performance. As predicted by the information channel, I find that a one-standard-deviation increase in personal relationship strength is associated with a 20% relative reduction in bankruptcy likelihood when compared with the unconditional mean. One potential explanation for the lower bankruptcy likelihood of borrowers in relationship loans might be a tendency of banks to inefficiently roll over loans instead of pushing borrowers into bankruptcy. I analyze the effect of personal relationships on loan renegotiations and find no increase in loan size or maturity for high personal relationship loans. If anything, relationship loans are being reduced in size and maturity upon renegotiation when compared to non-relationship loans. While this result does not rule out nepotism, it at least suggests that the information channel dominates in the aggregate.

The final result of my paper underscores the importance of bankers in the syndicated loan market. When a banker with a strong personal relationship switches from one bank to another, her former clients are three times more likely to initiate a novel banking relationship with her new employer than otherwise.

The paper contributes to three strands of the literature: The literature on the role of individuals as opposed to institutions, the literature on loan officers in business lending, and the institutional relationship lending literature.

A few authors explicitly investigate the impact of connections between corporate executives and banks. Karolyi (2017) exploits unexpected executive turnover as a shock to the personal relationship between corporate executives and banks and finds that borrower executives with stronger personal relationships with lending banks obtain lower credit spreads, and that executives are more likely to borrow from banks with which they have interacted in the past. My findings add to his in two ways. First, I analyze the importance of individuals in lending from the other side, that of bankers, rather than of executives. Second, I investigate a different economic channel through which personal relationships impact lending: Bankers impact lending through their personal characteristics and the information they gather about borrowers over time, whereas the economic channel proposed in Karolyi (2017) is that executives can commit to a behavior in different ways than their corporations, for example, due to reputation concerns.

Two other papers analyze the role of high-level social ties between bank and borrower executives in lending. Engelberg, Gao, and Parsons (2012) find that past social connections between executives, for example, from having attended the same university, lead to lower interest rates and larger loans. They find that firm performance increases after relationship loans, suggesting that high-level social connections can transfer useful information. Haselmann, Schoenherr, and Vig (2017) document a similar impact of personal connections on loan terms using data from German service clubs. They find that when bank and firm executives share social ties, loans tend to be larger and banks stand to earn less from those relationship loans due to higher rates of default. Unlike Engelberg et al. (2012), they do not find an effect on interest rates, but find rather that socially connected banks continue funding failing borrowers, suggesting a nepotism channel for social ties on lending.

Both sets of authors study social ties between high-level executives, either from shared past (Engelberg et al., 2012) or current (Haselmann et al., 2017) social interactions. I study personal interactions that are professional, rather than personal, in nature, that arise from current collaborations on loans, and that can be linked to precise transactions. These business relationships are less vulnerable to nepotism and, at the same time, might be more suitable than social ties to facilitate the transfer of business-related information. This is reflected in my finding that work-related personal ties are associated with fewer bankruptcies. I find that professional links between bankers and borrowers lower interest rates in a similar magnitude as do the past social ties studied in Engelberg et al. (2012), even after accounting for the endogenous nature of relationship formation. Unlike the high-level social relationships studied in Haselmann et al. (2017) that seem to foster nepotism, professional relationships are associated with fewer bankruptcies and seem to provide lenders with superior information about borrowers. That last finding suggests that the context in which personal relationships are formed plays a key role in determining whether they foster nepotism or superior information.

Other authors investigate the role of individual loan officers in the context of small business lending. Loan officers play a key role in determining loan terms for small, opaque borrowers (Drexler and Schoar, 2014; Behr, Drexler, Gropp, and Güttler, 2014; Agarwal and Ben-David, 2014), and their performance is impacted by bank-specific economic incentives (Qian, Strahan, and Yang, 2015; Cole, Kanz, and Klapper, 2015; Berg, Puri, and Rocholl, 2014; Hertzberg, Liberti, and Paravisini, 2010) and social characteristics (Fisman, Paravisini, and Vig, 2016). I add to this literature in two ways. First, the detailed microlevel data required in past studies of loan officers limited them to proprietary datasets obtained from a single lender. Data from a single bank cannot be used to disentangle the effects of individuals from that of institutions since, as those papers show, any individual effect strongly depends on the bank's respective structure and incentive system. Second, those papers study the interactions of loan officers with small borrowers, whereas the individuals in my study are commercial bankers that issue large, syndicated loans to multi-billion-dollar corporations. Compared with large borrowers in the U.S. syndicated loan market, small, opaque borrowers generally provide less and lower quality information, such as audited annual reports, credit ratings, or analyst reports. It is therefore not surprising that individual loan officers play a role in small business lending since often they are the bank's primary source of information. It is a novel finding that individual bankers have a large impact on the outcome of large syndicated loans to borrowers disclosing ample public information.

A number of authors have recently investigated the relative contributions of individuals compared with those of institutions, that is the firms employing those individuals, in a variety of contexts. Some authors find that executive-specific characteristics impact a wide range of corporate characteristics. Executive fixed effects can explain a significant fraction of management style (Bertrand and Schoar, 2003) and executive compensation (Graham, Li, and Qiu, 2012), as well as bank risk taking (Hagendorff, Saunders, Steffen, and Vallascas, 2015). Other authors document a significant contribution of individual fixed effects in the financial sector. Chemmanur, Ertugrul, and Krishnan (2014) find that investment bankers have a significant impact on the success of mergers and acquisitions, and Ewens and Rhodes-Kropf (2015) show that individual venture capitalists have significant explanatory power going beyond that of their venture capital funds. Mukharlyamov (2016) finds a relationship between banks' aggregate workforce composition and bank risk taking. I add to this literature by extending it to the setting of bank lending and by linking individual bankers to specific loans.

In concurrent work, Gao, Martin, and Pacelli (2017) use a similar dataset to mine to estimate fixed effects of loan officers. There are two main differences between their study and mine. First, I focus on lead banks and bankers working for lead banks, as those are the primary parties that negotiate loan contracts (for a detailed description of this process see, for example, Dennis and Mullineaux, 2000; Ivashina, 2009; Armstrong, 2003), whereas Gao et al. (2017) consider all lenders and their bankers equally. Second, I provide evidence that bankers form personal relationships with clients that lead to lower interest rates and superior subsequent loan performance, as well as that bankers impact lending on the extensive margin by matching borrowers with banks.

Finally, this paper adds to the large existing body of research on the role of institutional relationship lending. For a detailed review, see Boot (2000), Kysucky and Norden (2016) and Degryse, Kim, and Ongena (2009).

The remainder of the paper is organized as follows: Section 2 describes the data collection process on individual bankers and the resultant dataset. Section 3 presents the results of analyzing the impact of time-invariant banker fixed effects on loans. Section 4 focuses on the role of time-varying personal relationships between bankers and borrowers. Section 5 presents additional tests, and Section 6 concludes.

2 Data collection and sample

The analysis uses accounting data from Compustat North America for nonfinancial firms in the years 1996 to 2012. The starting year is the first year for which electronic SEC filings are widely available. Since part of the analysis focuses on the development of banking relationships over time, all sample firms are required to report at least four consecutive years of nonmissing data for assets, liabilities, EBITDA, and share price. The second dataset contains information on the pricing of syndicated loans from LPC DealScan. The DealScan-Compustat link is performed using the DealCcan-Compustat Linking Database from Chava and Roberts (2008). The third dataset contains bankruptcy data obtained from Audit Analytics. The final dataset consists of hand-collected data on interactions between commercial bankers and borrowers from loan agreements.

2.1 Data on bankers and borrowers

Data on the interactions between bankers and borrowers stem from the signature pages of publicly available loan contracts. Those signature pages contain information on both the banks involved in the deal and the bankers associated with each lender. Firms are generally required to publish their loan contracts with the SEC in accordance with item 601(b) of Regulation S-K. Item 601(b) requires firms to publish all "material events and contracts". Loan contracts generally qualify as material contracts and are therefore filed with the SEC. These filings also constitute a major source of the primary information in DealScan. Chava and Roberts (2008) report that more than half the entries in DealScan are based on such filings.

The data set is first constructed with all available 8-K, 10-K, and 10-Q filings for sample firms obtained from the SEC's EDGAR filing system. In the next step, I employ a text search similar to the method used in Nini, Smith, and Sufi (2009). The search program identifies those regulatory filings with an attached loan contract.

The search program then identifies the loan contract's signature page. Most documents contain a section that features the names and functions of all banks involved in the deal. In addition, the signature page usually contains names and titles of all bankers representing those banks. Once it has found the signature page, the program extracts the information on bankers and their respective banks.[2] Figure 1 gives an example of a signature page and the different items extracted.

[Figure 1 about here]

The circles mark the name of the bank (Wells Fargo), the bank's role (Administrative Agent), the banker's name (D. N.), and his title (Vice President).[3]

The resultant dataset contains information on both the institutions and persons involved in each deal. To confirm the text extraction program's efficiency, I randomly sample 100 of the potential contracts and compare the results from the text search with the actual contracts.

Not all contracts contain information that can be extracted. Manual inspection reveals that 35 percent, or about one-third of contracts do not contain information on the name of signers in the original document. Obtaining information on the bankers associated with the loan from those contracts is impossible. I therefore exclude all contracts from the sample that do not feature signatures. A lack of names in the original document can occur for one of two reasons: Either the contract does not contain a signature page or the signature page contains only the names of banking institutions, but not the officers representing them. In some cases, the personal signature is marked as "illegible"; that is, the original contract contains signatures that were not correctly converted into the electronic document in the initial filing process. In most cases, either all signatures are missing or all are present. In two contracts a subset of signatures was missing.

[2] A final step links bankers across different contracts and employers. This matching is necessary since the layout of signature pages is not uniform and names are sometimes spelled in different ways. Reasons for variations in spelling include both involuntary mistakes, such as typos, and intentional spelling choices, such as the omission of middle initials or the use of abbreviations.

[3] For the sake of privacy I removed the banker's full name from the picture.

For the remaining contracts that contain signatures, manual inspection finds that the text search correctly identified 80% of lead bankers.[4] The most frequent reason why no lead banker could be extracted is that the algorithm failed to capture any signature in the document (16%). Those loans contain no information on bankers and are dropped from the sample. In four percent of cases, the algorithm failed to extract the name of the lead banker, but succeeded in extracting the name of other bankers associated with syndicate participants. The high rate of correctly extracted signatures leads me to believe that the measure of relationship strength derived from these data is accurate. The noise from the text extraction is unlikely to be systematic and should, therefore, if anything, attenuate the results.

2.2 Measuring personal relationship strength

While the estimation of banker fixed effects requires only the identification of a banker's presence on a loan contract, the analysis of a time-varying impact of personal banking relationships requires a measure of personal relationship strength. Since relationship lending relies on the collection of information through repeated interaction over time (Petersen, 2004; Berger and Udell, 2006), there are two natural proxies for the strength of relationships between bankers and borrowers.

The first measure is the number of signed loan contracts, or interactions, between a given banker-firm pair; this measure is reported as the variable *Personal count*. *Personal count* measures the number of repeated interactions between a banker and a borrower, without regard to the banking institution employing the banker. *Personal count* therefore purely measures the relationship strength of the banker, not that of the bank.[5] Since the loan

[4] In addition to the names of lead bankers, the search also extracts the names of bankers associated with nonlead banks. The rate of successful extractions for the sample of all lenders is slightly lower at 76%. The text search is more precise for lead banks since the signatures of lead banks are easier to extract due to their structure, place of appearance in the contract, and name of the bank.

[5] The following example illustrates that point: A banker who was involved in three deals with a borrower when working for *Bank A* and another two deals when working for *Bank B* will be assigned a relationship count of five with this borrower for the final loan. The development of *Personal count* in this example is,

terms generally are negotiated between the lead bank and the borrower prior to syndication, I only consider interactions between bankers and borrowers if the banker acted for one of the syndicate's lead banks. For cases in which more than one banker from a lead bank has a prior relationship with the borrower, I follow Bharath et al. (2011) and assign the highest value of the relationship measure to the loan. Considering only the highest relationship value among all lead bankers is equivalent to assuming that bankers share their knowledge with other lead arrangers. Lead bankers have strong incentives to utilize and hence share their soft information since each lead bank retains on average almost 30% of the loan amount, and syndicate members require this share to be higher for more opaque borrowers (Sufi, 2007). I analogously calculate a second measure of personal relationship strength, $Personal\ duration$, as the time since the first interaction between a lead banker and specific borrower. Appendix A provides a detailed example for one of the commercial bankers from the sample.

For the further analysis, I restrict the sample of loan contracts to those that I successfully matched to DealScan. This step is necessary to add key loan-level information, such as loan size and pricing. The final sample comprises 4,430 loans with available information on bankers, loan characteristics, and all control variables.

A key advantage of the empirical setup in this paper is the ability to distinguish between personal and institutional relationships, that is, the relationship between a borrower and a banker as opposed to that between a borrower and the bank. As a proxy for institutional relationship strength, I calculate the maximum number of interactions between the lead banks on each loan and the respective borrower, analogous to the construction of *Personal count*. Interactions are aggregated to the ultimate parent bank level to avoid cases in which banks lend through different subsidiaries. The corresponding variable is $Institutional\ count$. Section 2.4 provides detailed statistics on each of those variables.

therefore, a simple sequence from 1 to 5 without a break after the switch in employer.

2.3 Discussion of relationship measure

The key question to judge the validity of my measure of personal interactions is whether the person signing the loan contracts is, in fact, the person that sets the loan terms and holds the relationship with the borrower.

I therefore talked to several current and former employees of commercial lending divisions from different banks, both in the United States and Europe. All interview partners agreed that, as a general rule, the person signing the contract on behalf of the bank is the banker most involved in negotiating the deal. At the very least, they argued that the signatory will have had some exposure to the deal and therefore knows what she is signing. At the same time they all confirmed that cases in which the signatory is not the person holding the relationship occur occasionally. One of the main reasons for the later situation is that the actual relationship banker is traveling when the contract needs to be signed. Manual inspection of the data and a comparison with publicly available data from professional networks reveal that most signatories are employed in banking divisions, although at least one person is reportedly employed in his bank's legal division. That the signatory of loan contracts is sometimes not the person holding the relationship introduces noise in my measure of personal relationship strength. Yet there is little reason to fear that any noise in the signing of loan contracts systematically biases my measure of personal relationship strength. If anything, this noise should attenuate my results.

2.4 Sample characteristics

Table 2 displays summary statistics for the main sample. All variables are winsorized at the 1% level.

[Table 2 about here]

The first set of variables describes the measures of personal relationships. There are 2,981 unique lead bankers in the sample. The key variable of interest is *Personal count*, the measure of personal relationship strength between commercial bankers and firms derived in Section 2.2. The average loan has a personal count of 1.43. Note that this is the average relationship strength per loan and that personal count is bounded from above by the total number of loans taken out by a borrower (for example, any borrower's first loan in the sample always will be assigned a *Personal count* equal to one). On the relationship level, the average number of interactions *per relationship* is 2.88. This means the average banker-firm pair interacts almost three times during the sample. For those firms that, at some point during the sample period, have a repeat interaction with any banker, the average number of personal interactions increases to 3.52 per relationship.[6]

An alternative measure of personal relationship intensity is the relationship's duration, rather than the number of prior interactions (Petersen and Rajan, 1994). I therefore assign to each loan a measure of *Personal duration*, corresponding to the time since the first loan contract signed between the borrower and the banker. The duration of personal relationships exhibits a pattern similar to that of the number of personal interactions. The average personal duration associated with loans is 0.63 years. The median loan is the first interaction between a borrower and banker, and hence *Duration* takes the median value of 0. The distribution is highly skewed: The maximum value of *Personal duration* is more than 14 years. The average number of loans on which bankers are reported as lead bankers is 2.27, with the most represented banker holding the lead relationship on 13 loans.

The average firm issues four loans during the sample period, or roughly one loan every four years. The maximum number of loans taken out during the sample period is 10, or roughly one every 2 years.

Institutional count is on average 1.68, which is slightly larger than personal count, at an average of 1.43 interactions. Since bankers leave the sample, for example, due to retirement

[6]To illustrate these numbers, suppose the sample consisted of two firms, one of which issues one loan and the other issues two loans. The second firm issues both loans with the same banker. Then the personal count on those three loans will take the values of 1, 1 and 2. The average personal count per loan is $\frac{1+1+2}{3} = 1.33$. The average personal count for the two relationships is $\frac{2+1}{2} = 1.5$. Finally the average relationship strength for the subsample of firms with at least one repeat relationship is 2.

or a career switch, it is not surprising that the number of personal interactions is smaller than that of institutional interactions.

The indicator variable *Banker left* marks a loan issued after a banker has left the bank. To construct this indicator, I first identify for each loan the banker with the strongest relationship to the borrower, the "lead banker". I then identify and mark the next loan of that borrower if that lead banker is no longer among (any of the) bank representatives; that is, her employment with the bank has ended. The identification therefore stems from the next loan taken out between a firm and its bank, after the loan officer with the strongest personal relationship to the borrower has left the bank. I require that the predeparture count between the banker and the firm is at least two; that is,a relationship did in fact exist.[7] In the final sample, about eight percent of loans, or 350 individual contracts, are identified in this way.

The next variables describe firm characteristics. The average firm in my sample is large with a mean (median) of \$4.1 billions (\$1.1 billions) in assets. The sample contains some very small firms with the minimum amount of assets at just \$19 million. Leverage is calculated as the book value of liabilities over the book value of assets. The mean ratio of liabilities to assets is 61%, and the market-to-book ratio is, on average, 1.05. Sample firms exhibit an EBITDA-to-assets ratio (*Profitability*) of, on average, 12%. The average firm has a fraction of 20% of its assets in intangibles, with the most opaque firm having as much as 80% intangibles.

The final set of variables describes loan characteristics. The all in spread drawn, a loan's spread above LIBOR, measures loan price. It is provided by DealScan, which adds loan spreads and annual fees for the total cost of credit. All in spread drawn varies a lot in the sample. The average loan is priced at 182 basis points above LIBOR, with the minimum spread being just 20 basis points and the maximum spread standing at 591 basis points. A

[7] One potential pitfall with this identification strategy is that a banker might not sign a particular loan (or my algorithm failed to extract a signature), although the banker was actually involved in some future deal. To avoid such situations, I require that commercial bankers do not appear again on any loans between their current employer and the borrower.

similarly large range of loan sizes is present in the sample. Whereas the average loan is $586 million, the smallest loans are just $5 million. The *Financial covenants indicator* takes the value of one if a loan contains a restrictive financial covenant. About three out of four loans in the sample feature at least one such covenant. Average loan maturity is 3.75 years, with the shortest loans running for just a month and the longest for twenty years. Finally, about half of each loan package is secured (51%). The sample contains both fully secured loans and those that are completely unsecured. All loan characteristics are comparable to those used in other papers, for example Engelberg et al. (2012).[8]

Since the instrumental variable analysis in Section 4 uses the binary instrument *Banker left*, there is the potential concern that borrowers who experience the departure of a relationship banker are fundamentally different from those who do not. Panel B of Table 2 therefore tests for differences in means in the firm-level variables between those loans issued after a relationship banker departs and the rest of the sample. Panel B of Table 2 shows that borrowers are very similar across the two groups. Two variables are marginally statistically different for firms that experience banker turnover: Treated firms have slightly higher institutional relationship strength and are five percent more likely to have a credit rating compared with control firms. Since both institutional relationships and the presence of a credit rating should lead to lower interest rates among the group of treated firms (Bharath et al., 2011), these differences should, if anything, bias against finding an attenuating effect of personal relationships on interest rates. Panel B of Table 2 therefore shows that treatment and control firms do not exhibit meaningful differences in observable variables.

[8]The only exception is that I report larger loan sizes, stemming from different treatments of DealScan data: DealScan's basic unit of observation is a loan facility, which corresponds to a single loan. Multiple loan facilities are usually bundled into a so-called "package". A single loan contract (package) can contain, for example, a term loan, as well as a revolving credit facility. Many papers use loan facilities as their unit of observation. But since the explanatory variable in this paper is personal relationship intensity, which is collected from loan contracts, that is, on the package level, all analyses are conducted on the package level and the relevant loan characteristics, such as interest rates, correspond to the value-weighted averages of the individual loan facilities.

3 Time-invariant impact of bankers on loans

A large literature is concerned with separating the effects of individuals from those of institutions. The most direct approach to estimating individual fixed effects is the inclusion of individual and institution fixed effects in the regressions. The drawback from this approach is that the indicator for individuals who never switch employers will be perfectly collinear with that of their institution. Papers that utilize this approach therefore limit their sample to individuals who work for more than a single employer (the so-called "switchers") and estimate the fixed effects associated with those individuals (e.g., Bertrand and Schoar, 2003). Since no individual fixed effect can be identified without a person switching employers, the switcher approach generally significantly reduces the available sample.

A number of authors in the finance literature have recently employed the methodology of Abowd, Kramarz, and Margolis (1999) (AKM method), which is a refined version of the approach of Bertrand and Schoar (2003) and is the methodology I utilize for the analysis. The so-called "connectedness" approach first sweeps out individual fixed effects by subtracting the mean of the dependent variable for each individual, before estimating the remaining model including the institution fixed effects. In a final step, individual fixed effects are recovered. Individual fixed effects are identified as long as at least one individual at a given institution is a switcher. Authors have used the AKM methodology to disentangle the impact of individuals from that of institutions in the context of CEO compensation (Graham et al., 2012), bank risk taking (Hagendorff et al., 2015), innovation (Liu, Mao, and Tian, 2016), or mergers and acquisitions (Chemmanur et al., 2014).

Formally, the full model is

$$y_j = \alpha_i + \phi_t + \theta_m + \kappa_q + \delta' X_{i,t} + \gamma' X_j + \epsilon_j, \tag{1}$$

where y_j is a loan characteristic of a loan package j obtained by borrower i in year t with bank m and banker q. $X_{i,t}$ is a vector of time-varying firm control variables, and X_j

is a vector of loan control variables. Firm-level control variables include rating, firm size, leverage, market to book, profitability, and tangibility. Loan control variables include the borrower's rating, the number of previous interactions with the lead bank and the loan type. The coefficients of interest are therefore θ and κ, which measure the time invariant bank and banker fixed effects. The sample is limited to bankers and banks which are associated with at least two different loans.

Table 3 presents the results from estimating the resulting high dimensional fixed effects model in equation 1.

[Table 3 about here]

The prior literature suggests three dimensions along which the explanatory power of the individual fixed effects can be evaluated (e.g., Graham et al., 2012; Hagendorff et al., 2015; Chemmanur et al., 2014; Liu et al., 2016). First, the degree to which inclusion of institution (bank) and individual (banker) fixed effects increases the model fit (R^2). Second, whether an F-test can reject the null hypothesis of joint statistical significance of all individual fixed effects, and, third, the relative contribution of individual fixed effects to the model's explanatory power.

Panel A of Table 3 presents the results from estimating regressions of five loan characteristics on control variables, with and without banker and bank fixed effects. The five loan characteristics are the loan price measured as the all in spread drawn over LIBOR; loan size measured as the logarithm of the loan amount in U.S. Dollars; loan maturity; the fraction of the loan secured with collateral; and the number of covenants associated with the loan.

For each dependent variable, even-numbered columns present estimates without banker and bank fixed effects and odd-numbered columns present estimates including those fixed effects. There are 614 individual bankers classified as movers; movers are associated with loans featuring at least two lead banks. Some bankers are only associated with loans from the same lead bank.[9] The inclusion of high-dimensional banker and bank fixed effects signifi-

[9]The number of movers is high since bankers can be associated with banks other than their employing

cantly increases the model's explanatory power. The model's adjusted R^2 for the loan pricing regressions increases from 64% in Column 1 to 87% in Column 2, a 38% relative increase in explanatory power. Adding banker and bank fixed effects leads to a similar increase in the explanatory power for the loan amount with an increase in relative explanatory power by 27%. Effects on loan maturity (49.7%) and the fraction of the loan that is secured (66.0%) are significantly larger. For the presence of financial covenants, the relative explanatory power more than doubles after the inclusion of banker and bank fixed effects, albeit from a lower baseline level of explanatory power. The absolute increase in explanatory power is relatively even across specifications, at around 20% to 40%. Adding banker and bank fixed effects, in addition to standard control variables, hence greatly increases the explanatory power of models of bank loan characteristics.

Panel B of Table 3 tests whether the joint explanatory power of banker fixed effects is statistically significant. I report the F-statistics associated with both bank and banker fixed effects (line two), just banker fixed effects (line three), and just bank fixed effects (line four). The critical F-values to reject the null hypothesis that fixed effects are jointly zero with a 99% confidence interval are F(1008, 1226) = 1.15, for the test of both banker and bank fixed effects, F(588, 1226) = 1.18, for the test of banker fixed effects and F(420, 1226) = 1.20, for bank fixed effects only. Since all estimated F-values range from 1.93 to 3.45, none of those tests fails to reject the null of joint insignificance of any set of individual or joint banker and bank fixed effects.

The final three lines of Panel B report the relative contribution of banker and bank fixed effects to the model R^2, respectively. As in Graham et al. (2012) and Ewens and Rhodes-Kropf (2015), the relative explanatory power of each set of fixed effects is calculated as $\frac{Cov(FE,y)}{Var(y)}$ where y is the dependent variable and FE is the corresponding banker or bank fixed effect. Banker fixed effects explain a sizable part of the variation in loan characteristics.

bank on a loan if another bank has a stronger institutional relationship with the borrower than does their own bank. In addition, the banking sector underwent widespread consolidation during the sample period. For example, consider a banker who worked for Bank One in the late 1990s and kept her job after Bank One was acquired by J.P. Morgen Chase in 2004. This banker would be classified as a "mover".

Banker fixed effects account for about 20% to 25% of the variation in the variables *spread*, *maturity*, *secured*, and *financial covenant* and about 15% for loan size. The contribution of banker fixed effects is notably larger than that of bank fixed effects for the interest rate spread, loan maturity, and the fraction of the loan that is secured. It is only slightly smaller than that of bank fixed effects for the presence of financial covenants.[10]

Table 3 therefore provides strong evidence that banker fixed effects have significant explanatory power for a wide range of loan characteristics. It is, however, not straightforward to evaluate whether the estimated contributions are potentially mechanical. Including a large number of fixed effects will mechanically lead to an increase in model fit and the standard F-test is unreliable as a measure of joint statistical significance for very large degrees of freedom (Fee, Hadlock, and Pierce, 2013). I therefore test the significance of banker fixed effects using a simulation approach similar to that of Fee et al. (2013). The simulations randomly assign the existing bankers and banks across all sample loans and then estimate the models presented in Table 3. Each simulation then saves the resultant R^2, F-statistic, and relative contributions of banker fixed effects. I then repeat the simulation 1,000 times and record the 90th, 95th, and 99th percentile of resultant values for the three variables. Table 4 reports the simulated values and compares them to the sample estimates.

[Table 4 about here]

Table 4 reports four columns each for the adjusted R^2, F-value, and relative contribution. The first column contains the estimated value from the actual sample. The following columns contain the 90th, 95th, and 99th percentile for the corresponding value obtained from the simulated sample.

[10] In unreported analyses, I investigate whether the importance of banker fixed effects exhibit cross sectional patterns. Specifically, I follow Bharath et al. (2011) and split borrowers into opaque and transparent groups based on whether they have a credit rating, whether their debt is rated investment grade, and whether their assets exceed $1 billion. I find that across all five loan outcome variables, banker fixed effects can explain a larger fraction of the observed variation for small borrowers than for large borrowers, for those firms without a credit rating compared to those with a credit rating, and for those firms whose debt is rated non-investment grade than for those whose debt is not. These results suggest that bankers play a bigger role when there is fewer publicly available information available.

The simulations largely corroborate the results obtained from the actual sample estimates. The overall model fit from the actual sample exceeds the 99th percentile obtained from the 1,000 simulations for all five loan outcome variables. In terms of joint significance, the F-statistics associated with the banker fixed effects exceeds the 99th percentile for simulated values for the interest rate spread, the loan size, the fraction of the loan that is secured, and the presence of financial covenants. The F-value for banker fixed effects for regressions of loan maturity is only slightly less significant and a bit lower than the 95th percentile for the simulated F-values.

The relative contribution of banker fixed effects from the actual sample only exceeds the 99th percentile for simulated values for the loan price regressions. Their relative contributions to *maturity* and *secured* is slightly less significant, with the sample values exceeding the 95th and 90th percentile for simulated values, respectively. The relative contributions of banker fixed effects to the model's explanatory power with respect to loan size and the presence of financial covenants is lower in the actual sample than the 90th percentile in the simulations. Hence, while banker fixed effects for these two outcome variables are jointly statistically significant and significantly contribute to model fit jointly with bank fixed effects, the sample values for their relative explanatory power might be slightly higher than their actual explanatory power.[11]

The results from Table 4 confirm that banker fixed effects have significant explanatory power for a number of loan characteristics and that the results from Table 3 are not due to randomness or the mechanical effect of including a large number of fixed effects.

While the preceding analysis shows that banker fixed effects can add to the explanatory power of models for a variety of individual loan characteristics, there remains the question whether these fixed effects exhibit meaningful patterns. An example of one such pattern is that bankers who tend to issue larger loans also prefer to add financial covenants as additional

[11] In unreported results, I repeat the simulations, but instead of randomizing both bankers and banks, I only randomly assign bankers and keep banks as in the actual sample. In those simulations, the relative explanatory power of banker fixed effects in the actual sample exceeds the 99th percentile of the simulated values for all five loan outcome variables.

safeguards. In the final set of analyses for the banker fixed effects, I therefore investigate whether banker fixed effects exhibit stable patterns, or styles. Importantly, the fixed effects estimated through the connectedness method are only identified relative to other bankers in the same group of connected bankers (Abowd et al., 1999). The correlations are therefore calculated only with respect to the largest connected group, which includes almost 90% of the sample. Table 5 presents the correlations of banker fixed effects for the various loan outcomes analyzed in Table 3.

[Table 5 about here]

Table 5 shows that banker fixed effects are strongly correlated. Bankers who tend to issue loans with higher interest rates also issue smaller loans. Bankers who issue these loans also tend to secure a larger fraction of them, but make less use of financial covenants. And bankers who tend to issue larger loans are indeed more likely to insist on financial covenants.

Taken together, the results in Section 3 suggest that banker fixed effects can explain a significant fraction of the different loan characteristics. These individual banker fixed effects are not random, but bankers rather exhibit consistent patterns across loan terms. One potential interpretation of these patterns is that they reflect individual preferences, or "styles". There is, however, the alternative explanation that they merely reflect some unobservable organizational characteristics. A banker might be a specialist for a certain group of borrowers, and the patterns observed in Table 5 reflect the common nature of her clients rather than styles. While the regressions control for many observable borrower characteristics, such as industry, firm size, and financial health, there might be other unobservable borrower characteristics that drive banker fixed effects. Assume, for example, that a banker is an expert for "tough" clients, and this leads her to prefer loans with high collateral over those with covenants, since covenants are associated with negotiating with the tough clients. If observable data cannot pick up on this "toughness", the banker fixed effects would falsely indicate a personal preference for securing loans, when it actually reflects internal bank structures.[12]

[12] One specific concern I can rule out is that banker fixed effects could proxy for their employers' industry

I will now investigate whether bankers exhibit not just time-invariant, but also time-varying impact on loan terms. Unobservable matching between bankers and borrowers might drive the time-invariant effects observed in this section, but such a matching should not impact the time-varying role of increased banker-borrower relationships.

4 Time-varying impact of bankers on loans

This section analyzes the impact of banker-borrower personal relationships on initial interest rates, subsequent firm performance, and the matching of banks and borrowers. The tests corroborate the earlier finding that bankers play a key role in the lending process by controlling for unobserved, time-invariant banker-borrower matching and focusing on the time-varying impact of bankers on loans and borrowers.

4.1 Personal relationships and interest rates

Whereas unobservable bank or borrower characteristics might explain the time-invariant effect of bankers on loan characteristics, any such factors should be static, that is, stable over time. A test of the impact of personal relationship formation on loan terms therefore achieves two goals: First, it investigates a specific economic channel through which bankers can impact lending: information gathering through repeated interaction. Second, unobservable matching between bankers and borrowers cannot explain dynamic changes in the impact of bankers as relationships become stronger. There are, however, three challenges in identifying the role of personal relationships between bankers and borrowers on bank lending. Figure 2 visualizes them.

[Figure 2 about here]

preferences: Since a single bank will employ multiple industry teams, the banker fixed effect might inadvertently proxy for a bank-industry effect which would necessarily have higher explanatory power than a pure bank fixed effect, and could not be captured by a pure industry fixed effect. In unreported analyses I therefore repeat the regressions but with a bank-industry fixed effect instead of a pure bank fixed effect. The explanatory power of banker fixed effects in this setting remains robust to this change.

The first challenge, depicted in Panel A, is that relationships form endogenously. Not all firms engage in relationship lending. For example, Sufi (2007) finds that more opaque firms are more likely to repeatedly borrow from the same lender. Since borrower quality is not perfectly observable, the selection of worse borrowers into relationships would counteract the dampening effect of personal relationships on interest rates in an ordinary least-squares (OLS) estimation.

A second identification challenge is survivorship bias, as shown in Panel B. Healthy, well-managed firms will survive longer and default on loans less frequently. Survivorship leads to a mechanical association between (potentially unobservable) financial health and more interactions between borrowers and banks.

Panel C illustrates that personal relationships develop in lockstep with institutional relationships. Interactions between a banker and a firm necessarily coincide with interactions between the employing bank and the borrower. As long as bankers do not switch employers, disentangling the impact of personal and institutional relationship strength is not feasible.

I propose an instrumental variable approach to tackle these three challenges. The instrumented variable is personal relationship intensity. The proposed instrument consists of an indicator variable equal to one if a banker switches his employer, and zero otherwise. Figure 2 illustrates this approach. It depicts a situation in which *Firm 1* and *Bank 1* have previously interacted four times. After the fourth loan, the banker in charge of managing the relationship, *Banker A*, leaves *Bank 1* to join *Bank 2*. If his replacement, *Banker B*, has no prior interactions with *Firm 1*, the next loan between *Bank 1* and *Firm 1* will have an institutional relationship count of five, but a personal relationship count of only one.

The instrument is therefore *Banker left*, an indicator variable that takes the value of one for the first loan between a borrower and a lender after the borrower's main relationship banker has left the bank. Since the loss of a relationship banker is a firm-level event, I include firm fixed effects in all specifications to control for unobservable firm characteristics that might drive both interest rates and the loss of a banker.

The departure of a relationship banker from a lending institution is likely to fulfill the relevancy condition. While it is possible that a borrower sustains personal relationships with more than a single banker, the departure of the main relationship banker should still lead to a drop in personal relationship strength as long as secondary relationship bankers have weaker relationships to the borrower.

The departure of a relationship banker also fulfills the exclusion condition, as long as banker turnover is unrelated to other factors impacting loan terms. Banker turnover can result from various causes, many of which are plausibly exogenous to the performance of bankers and their relationship borrowers, such as death, illness or retirement. The challenge to the exclusion restriction stems from cases of endogenous banker turnover: Poor financial performance by borrowers might cause both a deterioration of loan terms and banker turnover. While I can control for observable borrower quality and time specific shocks through firm and year fixed effects, there might be unobservable shocks. I undertake a number of tests to verify that my results are not driven by such unobservable shocks in Section 5. Specifically, I control for a common unobservable quality of a banker's portfolio by adding banker fixed effects to the regression: If a banker persistently gives out loans that are too cheap, the banker fixed effect soaks up this effect. Alternatively, I add bank-year joint fixed effects to control for bank specific shocks. All results are robust to these changes in specification, which gives me confidence that the results are not driven by unobservable shocks driving both banker turnover and loan terms.[13]

More formally, the model is

$$Spread_j = \alpha_i + \phi_t + \theta_m + \beta \widehat{Personal\ count}_{i,j,t} + \gamma' X_{i,j,t} + \epsilon_j, \tag{2}$$

[13] In a final, unreported robustness test, I repeat the analysis using only cases of banker turnover where the banker subsequently signs a loan for a different bank. That test addresses concerns that bankers are fired for giving cheap loans, leading to a subsequent increase in interest rates. If bankers that get fired for giving loans too cheaply have a lower likelihood of getting hired subsequently, this test alleviates concerns that low rates and banker turnover are driven by an unobservable banker characteristics. Results retain both their statistical and economic significance.

where $Spread_j$ is the all in spread drawn of loan package j, taken out by firm i in year t with lead bank m. The main variable of interest is $\widehat{Personal\ count}$, the instrumented number of interactions between the banker and the borrower. $X_{i,j,t}$ denotes a vector of time-varying firm and loan controls.

The estimation is performed using a two-stage least-squares regression. In the first-stage equation, $\widehat{Personal\ count}$ is instrumented for with *Banker left*. The specification is

$$Personal\ count_j = \alpha_i + \phi_t + \theta_m + \rho \mathbb{1}_{Banker\ left} + \gamma' X_{i,j,t} + u_j, \tag{3}$$

where $\mathbb{1}_{Banker\ left}$ is an indicator variable that marks a borrower's first loan after her relationship banker left her relationship bank.

Table 6 presents the results of estimating equations 2 and 3 using two steps least squares. The key explanatory variable of interest is $Personal\ count$, the measure of personal relationship strength between commercial bankers and firms.

[Table 6 about here]

Column 1 of Table 6 reports the results of estimating the first stage, equation 3, a regression of personal relationship count on control variables and the instrument $\mathbb{1}_{Banker\ left}$. The point estimate on $\mathbb{1}_{Banker\ left}$ is -0.67, meaning that the departure of a banker with a strong personal relationship leads to a significant reduction in personal relationship strength on the next loan. The drop in personal relationship strength is economically significant and corresponds to 40% of the mean and 69% of a standard deviation of personal relationship strength. Personal relationship count does not necessarily drop to one after a banker's departure. Since borrowers can have personal relationships with multiple bankers simultaneously, a departing lead banker can sometimes be replaced with a different banker.[14]

[14] Imagine a borrower who interacted two times in the past with Banker A and three times with Banker B. If Banker B retires, but Banker A stays, the next loan after the departure of Banker B will have the same personal relationship strength as the previous loan. For slightly less than ten percent of cases, the departure of a lead banker is not associated with a drop in personal relationship strength to 1.

The large economic and statistical significance of the estimated coefficient suggests that the instrument indeed fulfills the relevancy condition: When a banker holding a personal relationship leaves a lender, there is a significant and negative effect on the personal relationship strength of the next loan. The coefficient estimate is highly statistically significant, both individually and in terms of the joint first-stage Cragg-Donald F-statistic, which is 123.1, well above the corresponding Stock and Yogo (2005) critical value of 16.38 for a maximum 10% bias in the single instrument case.[15] Taken together, the high statistical significance of both the instrument individually and the first stage jointly alleviates concerns of a weak instrument issue.

Columns 2 to 5 of Table 6 report the results from the second-stage estimation. The estimated impact of *Personal count* on interest rates is economically large at -55 basis points and statistically significant at the 1% level.[16] Column 3 adds controls for year and firm fixed effects. The estimated coefficient shrinks to -16 basis points, but retains it statistical significance. The same is true for Column 4, which replaces the firm-level controls through loan-level control variables and bank fixed effects to account for unobservable time-invariant bank characteristics impacting interest rates. The resultant coefficient estimate of *Personal count* is -34 basis points and highly statistically significant. Finally, Column 5 combines all firm- and loan-level control variables. The resultant coefficient estimate is -12 basis points and remains statistically significant at the 10% level. The estimated impact is economically sizable: A one-standard-deviation increase in personal relationship strength is associated with a reduction in interest rates of about 10.5 basis points, or 6.6% of the unconditional mean spread. For the median loan size of $250 million, a one-standard-deviation increase in personal relationship strength leads to an annual interest rate savings of $275,000.

[15] The large F-statistic is partly driven by the inclusion of firm fixed effects. Section 5 presents a robustness exercise with industry, rather than with firm fixed effects. The corresponding F-statistic drops by one-third to about 80, but is still very high.

[16] All standard errors are clustered at the borrower and year level to account for arbitrary error correlation within borrowers and years as suggested in Petersen (2009) and implemented in Karolyi (2017). All results are robust to clustering errors on the borrower level or simply using robust standard errors without clustering.

The estimated effect of these personal relationships between bankers and borrowers is similar in magnitude to the effect of high-level social ties in Engelberg et al. (2012), who find an effect of 28 basis points on interest rates when banks and borrowers share a board level social connection.[17]

To get an impression of the magnitude of the biases from the endogenous nature of personal relationships, I show in Table 7 OLS regressions of equation 2.

[Table 7 about here]

The estimated impact from personal relationships on loan terms in these panel regressions is generally negative and significant. But the estimates are smaller than in the instrumental variable results, and the point estimate on personal relationship count in the most complete specification (Column 4) is only about one-quarter of that from the instrumental variable specification. These results suggest that the bias from worse borrowers self-selecting into relationship lending biases the panel estimates upward.

While the results in this section provide evidence that personal relationships between bankers and borrowers lead to a time-varying impact of bankers on interest rates, there is no robust evidence for a similar impact on the other loan characteristics analyzed in Section 3. Neither loan size nor the fraction of the loan secured, nor the presence of financial covenants vary with personal relationship strength. Bankers therefore exhibit both time-variant and time-invariant preferences regarding the pricing of loans, but only time-invariant preferences regarding the other characteristics. One potential interpretation of this result is that loan size, collateral requirements, and covenants are impacted more by time-invariant banker characteristics, whereas interest rates also are strongly driven by information and personal relationships.[18]

[17] One of the advantages of my measure of personal relationship strength is that, unlike the analysis in Engelberg et al. (2012), my analysis can use an ordinal measure of relationship strength. In unreported results, I collapse this ordinal measure into a single indicator as in Engelberg et al. (2012). All results retain both their economic and statistical significance in this specification.

[18] In unreported results, I find evidence that while loan *size* is not statistically significantly affected by personal relationships, loan *availability* is. I use a firm-month panel to estimate regressions of an indicator

4.2 Efficiency or nepotism?

The lower interest rate associated with loans in which bankers have lots of prior experience with borrowers could be due to either nepotism or superior information. On the one hand, commercial bankers might extend favorable loans to managers they have been interacting with in the past in exchange for personal monetary or social favors. Under this nepotism hypothesis, loans granted by bankers with many prior interactions with borrowers should be associated with worse loan performance afterward. If, on the other hand, bankers learn valuable information about borrowers over the course of their relationship, loans granted by bankers with strong prior experience should be associated with better loan performance.

To test these hypotheses, I construct an indicator variable *Bankruptcy*, which takes the value of one if the borrower of a given loan files for bankruptcy at any point of time during the maturity of the loan.[19] The unconditional likelihood of default for any loan in the sample is 3.16% and is comparable to other studies (e.g. Engelberg et al., 2012). Table 8 presents results from estimating a linear probability model in which the dependent variable is *Bankruptcy* and the explanatory variables include *Personal count*, *Institutional count*, and the loan and firm controls from Table 7.[20]

[Table 8 about here]

The results from Table 8 show that loans granted by bankers with lots of prior experience with the borrower are associated with a significantly lower likelihood of default. The point estimates of *Personal count* range from -0.67% to -0.75% across the various specifications and are robust to a wide range of firm- and loan-level controls. Personal relationship intensity is about 70% more effective at reducing bankruptcy likelihood than institutional relationship

variable whether a borrower obtained a loan in a given month on personal relationship strength of the borrower's last loan. I find that the likelihood of obtaining a loan increases significantly for borrowers with strong personal relationships. Firm fixed effects make sure that the effect is not driven by the mechanical association of stronger relationships with more loans.

[19] If a borrower has more than one outstanding loan at the time of bankruptcy, I assign the bankruptcy event only to the last loan.

[20] Note that due to the rare occurrence of bankruptcies and renegotiations, there are too few observations to estimate an instrumental variable specification analogous to Section 4.

intensity. The estimated reduction in bankruptcy likelihood is economically large: Compared with the unconditional bankruptcy rate of 3.16%, a one-standard-deviation increase in personal relationship strength is associated with a 21% relative reduction in bankruptcy likelihood, after controlling for a wide variety of firm and loan characteristics.

The estimated reduction in bankruptcies is not just economically significant but can also explain why banks are willing to grant loans at lower interest rates to relationship borrowers. Khieu, Mullineaux, and Yi (2012) report average recovery rates of bank loans ranging from 60% to 80%. A back-of-the-envelope calculation using the estimated reduction of bankruptcy likelihood of 75 basis points in Column 4 of Table 8, a recovery rate of 70% and loan maturity of 3 years implies annual savings of about 7 basis points for a one standard deviation increase in personal relationship strength. Compared to the lower interest rates of 11 basis points as a result of this increase in personal relationship strength estimated in Section 4, savings from lower bankruptcy rates can make up around two thirds of the reduced interest rates.[21]

The results from Table 8 indicate that loans granted by commercial bankers with many prior interactions with borrowers are associated with a significantly lower likelihood of bankruptcy. A lower bankruptcy likelihood by itself is, however, not necessarily the result of superior information: Haselmann et al. (2017) find that when CEOs of borrowers and banks share a social relationship, banks are more likely to extend loans to borrowers instead of pushing them into bankruptcy. If personal work relationships between bankers and borrowers had a similar nepotism effect, high personal relationship loans should exhibit a pattern of modifications advantageous for borrowers at times of renegotiation.

To test this hypothesis, I obtain data on loan renegotiations as used in Roberts (2015) from Michael Robert's website. I classify all events as renegotiations which are neither originations nor maturing of credit agreements and match them to my data based on DealScan

[21]These calculations form the lower bound of the benefits for the bank from personal relationships. Since bankruptcies are clustered in economic downturns when recovery rates are low and capital is scarce, the economic benefits of reduced bankruptcy rates likely exceed the nominal impact. In addition, banks can use personal lending relationships to cross sell a variety of other products or services, see Drucker and Puri (2005) or Neuhann and Saidi (2017).

package ID.[22] The resulting dataset includes 493 renegotiation events. I then test the hypothesis that loans associated with stronger personal relationships between bankers and borrowers exhibit a pattern of positive modifications.

The dependent variables in Table 9 are the change in the loan's Dollar amount (*Del. amount*, columns 1 and 2) and the change in its maturity (*Del. maturity*, columns 3 and 4). The explanatory variables include personal and institutional relationship strength, as well as an indicator that takes the value of one if a borrower's debt is junk rated, and its interaction with personal relationship strength. If personal relationships between bankers and borrowers lead to nepotism similar to that caused by social connections between high level bank and corporate executives documented in Haselmann et al. (2017), stronger personal relationships should be associated with an increase in loan amount and maturity upon renegotiation, an effect that should be stronger for firms that are closer to bankruptcy, i.e. have a worse credit rating.

Column 1 of Table 9 shows that personal relationship strength is not associated with an increased loan volume in renegotiations: The estimated coefficient of *Personal count* on *Del. amount* is \$-3.48 million and statistically insignificant. Column 2 tests whether the effect of *Personal count* is different for firms that are junk rated. While the interaction *Personal count* $\times$ *junk* is 0.18 and statistically insignificant, the estimated coefficient on *Personal count* doubles in size to -8.64 and becomes statistically significant at the one percent level. Since the null hypothesis is that stronger personal relationships should be associated with higher loan amounts, this is direct evidence against a nepotism effect.

Columns 3 and 4 repeat the analysis with *Del. maturity*, the change in loan maturity as the dependent variable. The estimated coefficient of *Personal count* on changes in maturity in column 3 is negative and statistically insignificant. Once the interaction *Personal count* $\times$ *junk* is added in column 4, the coefficient of *Personal count* turns positive to 0.43 months, but remains statistically insignificant. The coefficient on the interaction *Personal count* $\times$

[22] Due to the cost of data collection, Roberts (2015) only obtained data on 114 randomly chosen borrowers.

junk, however, is ten times as large at -4 months and statistically significant at the 10% level.[23] As before, this result goes directly against the null hypothesis is that stronger personal relationships lead to more advantageous modifications in renegotiations.

Taken together, these results suggest that stronger personal relationships, if anything, lead to less favorable loan modifications for borrowers upon renegotiation. The finding that personal relationships formed through business interactions lead to different outcomes then personal relationships formed through social interactions underscores that the nature of personal relationship formation plays a key role in determining whether they increase information or nepotism. Personal relationships formed in a business context are likely to be associated more with work related incentives and less with social incentives and hence better suited to transmit information.[24]

4.3 Do borrowers follow their bankers?

The previous sections show that commercial bankers with strong borrower-specific relationships allow firms cheaper access to loans. If bankers are truly important in the syndicated loan market and help reduce the asymmetric information issues between banks and borrowers, I would expect borrowers to follow their relationship bankers after relationship bankers switch employers. I now test the hypothesis that borrowers follow their bankers to new lenders. I construct a panel that features one observation for each potential borrower-bank pair in each year and estimate the following specification:

$$Initiation_{i,j,t} = \phi_j + \rho_t + \beta Personal\ Relationship\ Acquired_{i,j,t} + \epsilon_{i,j,t}, \tag{4}$$

The dependent variable $Initiation_{i,j,t}$, is an indicator variable that takes the value of one in year t if firm i took out a loan with bank j as one of the lead underwriters in year t but did

[23]In un-tabulated results I investigate whether personal relationships impact pricing modifications. I find no connection between personal relationship strength and interest rate adjustments.

[24]In that sense, my result is similar to the findings in Haselmann et al. (2017) that stronger work related incentives reduce the nepotism effect of social connections.

not do so in the two preceding years. To control for potential bank- or year-specific shocks, the full model controls for bank- and year fixed effects in the form of ϕ_j and ρ_t.

The main explanatory variable in this analysis is $Personal\ relationship\ acquired_{i,j,t}$, and indicator variable that identifies firm-bank pairs in which a personal relationship was recently acquired by the bank. To construct this variable, I first check whether bank j acted as lead arranger for one of firm $i's$ loans during the preceding two years. I am interested in whether hiring a banker with a prior personal relationship to a specific firm will allow the bank to initiate a lending relationship with that firm. I therefore verify whether any banker that was involved in any loan of bank j in year t has acted as banker for a different bank, $k \neq j$ on a loan to firm i during the past two years. $Personal\ relationship\ acquired_{i,j,t}$ takes the value of one if no institutional relationship previously existed and a personal relationship was acquired. Since the number of bankers with preexisting relationships who switch employers is small, the analysis does not require that the banker who brought the personal relationship to bank j signs the loan personally. If commercial bankers take their clients with them after switching employers, we expect $Personal\ Relationship\ Acquired_{i,j,t}$ to have a positive impact on $Initiation_{i,j,t}$, the initiation of a new banking relationship.

Table 10 presents the results of estimating different specifications of Equation 4.

[Table 10 about here]

Columns 1 to 3 estimate a linear probability model. The sign of the coefficient of $Personal\ relationship\ acquired_{i,j,t}$ is positive and statistically significant. The point estimate of 0.2%, albeit small, is economically significant when compared with the unconditional probability of a relationship initiation: Absent a personal relationship, the probability of initiating a loan with any new bank is 0.1%. The point estimate of 0.2% therefore represents a 200% increase in the likelihood of initiating a relationship after having acquired a personal relationship.

One potential explanation for this finding could be that banker turnover is associated with the state of the macroeconomy. If, for example, commercial bankers are more or less likely to switch employers in economic upswings, *Personal relationship acquired* will be correlated with the macroeconomic state. If firms are more (less) likely to switch lenders in good years, the effect if *Personal relationship acquired* on relationship initiation will be biased upward (downward). I therefore repeat the estimation in Column 2 while adding year fixed effects. The estimated coefficient on *Personal relationship acquired* not only remains positive and statistically significant at the 1% level but also becomes larger as well with a value of 0.04. This result suggests that banker turnover is higher in years in which switching between lenders is lower.

Another concern is that bank characteristics could drive the association of personal relationships and loan initiations. If larger, expanding, or healthy lenders issue more loans and also hire more employees from competing lenders, this could explain the earlier finding. I therefore add bank fixed effects (of the acquiring bank) to the estimation in Column 3. The estimated coefficient for *Personal relationship acquired* shrinks slightly to 0.023, but remains statistically highly significant.

Columns 4 to 6 repeat the analysis using logit regressions rather than linear probability models. The coefficient for *Personal relationship acquired* remains positive and statistically significant in all specifications. The changed magnitude of the coefficient stems from the different interpretation in the context of logit regressions in which it represents the marginal contribution of the factor to the likelihood. The average marginal effects are smaller than in the linear probability model regressions and equal to about 0.1% across the three specifications. The implied economic magnitude is nonetheless large and implies borrowers are more than twice as likely to initiate a new lending relationship with a bank that hired their relationship banker than with other banks.

The results presented in Table 10 provide evidence that commercial bankers do indeed take their clients with them when switching banks and is evidence that personal lending relationships are of value to both banks and borrowers. This finding adds to that of Karolyi (2017), who finds that corporate executives who get appointed at a new firm tend to continue borrowing from the same banks as before. While Karolyi (2017) finds that the client side of a relationship matters, my findings suggest that the bank side of the relationship is equally important for explaining the endogenous formation of bank-borrower relationships.

5 Additional tests

The preceding analyses made a number of assumptions regarding the data and model specification. This section provides additional tests that corroborate the earlier findings and show that the results are robust to various alternative specifications.

5.1 Additional results on the time-invariant effect of bankers on loans

The first additional test concerns the banker fixed effects results. One potential concern is that these results might be driven by serial correlation of dependent variables inside a specific bank as discussed by (Fee et al., 2013). Table A2 therefore presents results from tests of the persistence of banker fixed effects across multiple banks. The dependent variable is the average residual of each loan outcome variable at a banker's second associated bank. The explanatory variable is the same banker's fixed effect from her previous bank. If bankers do indeed posses time-invariant styles, the residuals should exhibit significant, positive serial dependence. Table A2 shows that the banker-specific fixed effects from one bank are indeed a strong predictor of the banker-specific fixed effects at another bank: For each dependent loan outcome variable, the estimated relationship of banker fixed effects across banks is positive and highly statistically significant.[25] In unreported results, In unreported tests, I also repeat the fixed effects regressions using the same collapsed residuals, similar to the tests in Bertrand and Schoar (2003) and Fee et al. (2013). The results remain robust to this

[25]The sample size shrinks significantly since I both collapse observations on the banker-bank level, and limit the sample to bankers which work for two distinct banks.

alternative specification. These results provide further evidence that the observed banker fixed effects are indeed meaningful.

5.2 Additional results on the effect of relationships on interest rates

The second set of additional results concern the analysis of personal relationships in Section 4. First, the analysis of personal relationships in the preceding sections measures personal relationship strength as the number of previous interactions between bankers and borrowers. One potential concern could be that the number of interactions is an imperfect proxy for the strength of personal relationships. Table A3 presents the results from estimating the instrumental variable regression from equations 2 and 3 with personal relationship strength measured as *Personal duration*, the time since the initial loan between a banker and a borrower, rather than *Personal count* as used in the original estimation. The estimated specifications correspond to those in Table 6, with the first column presenting the results from the two-steps least-squares estimation's first stage. As in the case of *Personal count*, *Personal duration* significantly drops after a relationship banker leaves her old bank, showing that most firms do not have a second personal relationship of equal strength to a different banker which could replace their primary relationship. The next loan by the borrower features a personal relationship that is about one year shorter than the previous one. The first stage shows that *Banker left* is statistically highly significant. The first stage is also jointly highly with a Cragg-Donald F-statistic of 84.88. The high statistical and economic significance of the estimates alleviate concerns of weak instrument issues.

The subsequent second-stage regressions produce point estimates that are both economically and statistically comparable to those using *Personal count* as the measure of personal relationship strength. Personal relationship duration reduces interest rates by between 8 and 45 basis points for each additional year since the beginning of the relationship. The estimates are robust to a wide variety of controls and economically significant: A one-standard-deviation increase in *Personal duration* is associated with a 12.5 basis point lower interest rate in the most complete specification in Column 5. This estimated effect is slightly larger than that for *Personal count*, which was 10 basis points for a one standard deviation increase, but overall the robustness of the estimated effect confirms the earlier findings that personal relationship strength is associated with lower interest rates.

It might be that the results are driven by the inclusion of firm fixed effects. Table A4, therefore, presents the results from estimating Equations 2 and 3 using industry fixed effects rather than firm fixed effects. Industry fixed effects are assigned based on the two-digit primary SIC codes. Column 1 presents the results from the first-stage estimate of *Personal count* on the instrument banker left. The impact of losing a relationship banker is negative and significant as before. The first-stage Cragg-Donald F-statistic is lower than in the firm fixed effects regressions of Table 6, but still very high at 70.48. The following second-stage estimates are comparable to the ones featuring firm fixed effects in both economic and statistical significance.[26]

Another concern might be related to the suitability of the instrumental variable approach. If *Banker left* was a weak instrument for personal relationship strength, the resultant second stage estimates might be biased upward. I therefore repeat the analysis of personal relationships and interest rates using a difference-in-differences (DiD) approach.

Table A5 presents the results from a DiD regression of the loan spread on *Personal count*, the number of prior interactions between bankers and borrowers; *Banker left*, an indicator that takes the value of one for the first loan after a relationship banker leaves the sample; and *Personal count* $\times$ *banker left*, the interaction of *Banker left* and the value of *Personal count*, lagged by one period. The specifications in Columns 1 to 4 correspond to those from Table 6. The coefficient of interest is that on the interaction *Personal count* $\times$ *banker left*. If stronger personal relationships are associated with lower interest rates,

[26] Note that the sample is slightly larger in the industry fixed effects regression, as it now includes singleton observations, that is, firms which are linked only to a single loan. Those singleton borrowers were omitted in the specification with firm fixed effects since they can lead to biased estimates.

the loss of a relationship banker with a strong relationship should be associated with an increase in interest rates. The point estimate of the impact of *Personal count* × *banker left* on interest rates is indeed positive, statistically significant, and economically comparable to the results from the instrumental variable setup. The coefficient of about 12 basis points on the interaction implies that a one-standard-deviation increase in personal relationship strength is associated with a reduction in interest rates by about 10 basis points, or about 6% of the unconditional mean spread. The estimated impact is robust across the various specifications.

Columns 5 replaces the firm fixed effects with industry fixed effects. The DiD estimates become statistically more significant under the inclusion of industry fixed effects. The estimated coefficient on the interaction of the lagged personal relationship strength with the indicator of whether the lead banker left the bank is 11.8 basis points, meaning interest rates rise by about 12 basis points for each additional interaction of the gone banker with the firm. A final concern might be that contemporaneous industry-wide shocks affect both banker turnover and interest rates. Column 6 replaces the separate industry and year fixed effects with joint industry-year effects. The resulting estimate of the interaction term is 13 basis points, even stronger than before.

Since the sample starts in 1996, due to the lack of available data in earlier years, left censoring is a potential issue. If a banker has interacted with a borrower earlier in the 1990s and then again in the late 1990s or early 2000s, I could incorrectly classify the interaction as a novel relationship. To alleviate concerns that left censoring drives the results of personal relationships on interest rates, I present in Table A6 results with various burn-in periods. During an initial part of the sample, the so-called burn-in period, I track the personal relationships between bankers and borrowers but do not estimate any regressions. The longer the burn-in period, the more precise the measure of personal relationship strength becomes. Columns 1 and 2 burn in two years of data, 3 and 4 burn four years of data and the last two columns burn in six years of data. The instrumental variable regressions reveal that the estimated impact of personal relationship strength on interest rates is both economically and statistically *more* significant in the truncated samples. The point estimates range from -23 to -36 basis points, more than double their original magnitude. It is not the case that a weakening of the first stage estimation is driving this result: The Cragg-Donald F-statistic for the first stages remains at almost 30, even in the case of a 6-year initial burn-in period. The results from Table A6 therefore confirm that left censoring does not drive the attenuating impact of personal relationships on interest rates.

Table A7 provides two additional robustness checks. First, Columns 1 and 2 replace the bank and year fixed effects of the main analysis with combined bank-year fixed effects. The rationale behind this test is that banker turnover might coincide with unusual conditions at the employing bank, such as particularly strong exposure to the financial crisis of 2008. Bank-year combined fixed effects account for this possibility. As Table A7 shows, the results remain both economically and statistically very comparable to those in the main analysis once bank-year fixed effects are added. The first stage remains equally economically strong.

The last two columns in Table A7 replace bank with banker fixed effects. If banker turnover was driven by, for example, poor performance by individual bankers, increased rates after banker turnover might reflect a correction of the previous banker's bias rather than a shock to personal relationship intensity. The inclusion of banker fixed effects would remedy such a bias. As can be seen from Column 4 of Table A7, the results are robust to this change in specification. Once banker fixed effects are included in the model, the estimated impact of personal relationship strength on interest rates is -31 basis points and statistically significant at the 5% level. The first stage remains statistically and economically significant. The results in Table A7 reconfirm the interpretation that personal relationship strength indeed causally reduces interest rates on loans.

6 Conclusion

I construct a new dataset linking commercial bankers and borrowers to investigate the role of individual bankers in the market for large syndicated U.S. loans. I find that bankers play a key role in setting loan terms. Commercial bankers exhibit a significant, time-invariant impact on loan terms, such as the interest rate, loan size, loan maturity, whether a loan is secured, or whether it features a financial covenant. Banker fixed effects can explain about 1.5 times as much of the variation in those variables as bank fixed effects.

Individual bankers impact commercial bank lending not just through time-invariant factors, which could reflect tastes, innate aptitude, or idiosyncratic preferences, but they also form personal relationships with borrowers over time. I find that bankers with strong personal relationships to borrowers grant loans at lower interest rates. A one-standard-deviation increase in personal relationship strength is associated with annual savings of $275,000 for the median borrower. Those loans are associated with lower borrower bankruptcy likelihood, which suggests that the reduced credit spread is due to bankers' ability to collect information about borrowers over time. Having an experienced relationship banker benefits not just banks but also borrowers; this benefit is reflected in the finding that bankers who switch to another bank take their former clients with them.

My results shed light on the economic process through which banks make lending decisions. Banks and borrowers in the syndicated loan market are large, sophisticated institutions and have access to high-quality, publicly available information. Nonetheless, individual bankers play a key role in forming lending relationships and setting loan terms.

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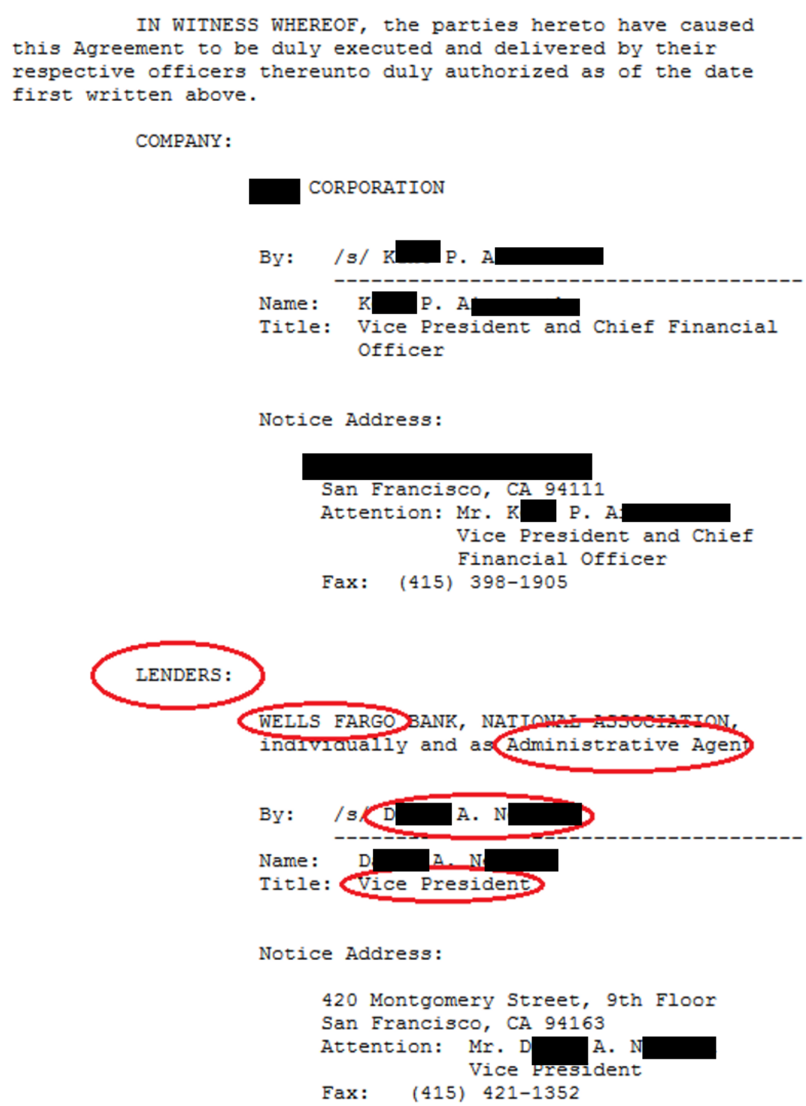
Figure 1: Example of simple signature page with a single bank.
The red circles indicate information extracted by the text search algorithm. This information includes the name and role of the bank, as well as the name and title of the signatory. The names of the banker, corporation, and corporate executive are anonymized for the sake of privacy.



IN WITNESS WHEREOF, the parties hereto have caused this Agreement to be duly executed and delivered by their respective officers thereunto duly authorized as of the date first written above.

COMPANY:

[redacted] CORPORATION

By: /s/ K[redacted] P. A[redacted]

Name: K[redacted] P. A[redacted]
Title: Vice President and Chief Financial Officer

Notice Address:

[redacted]
San Francisco, CA 94111
Attention: Mr. K[redacted] P. A[redacted]
Vice President and Chief Financial Officer
Fax: (415) 398-1905

LENDERS:

WELLS FARGO BANK, NATIONAL ASSOCIATION, individually and as Administrative Agent

By: /s/ D[redacted] A. N[redacted]

Name: D[redacted] A. N[redacted]
Title: Vice President

Notice Address:

420 Montgomery Street, 9th Floor
San Francisco, CA 94163
Attention: Mr. D[redacted] A. N[redacted]
Vice President
Fax: (415) 421-1352

Figure 2: Identification Challenges
This figure illustrates three challenges in estimating the impact of personal relationships on lending in Panels A to C. Panel A illustrates the question of selection into a single or multiple relationships. Panel B visualizes the challenge posed by survivorship bias. Panel C depicts the simultaneous development of personal and institutional relationships. Panel D illustrates the instrumental variable setup I propose to overcome these challenges.

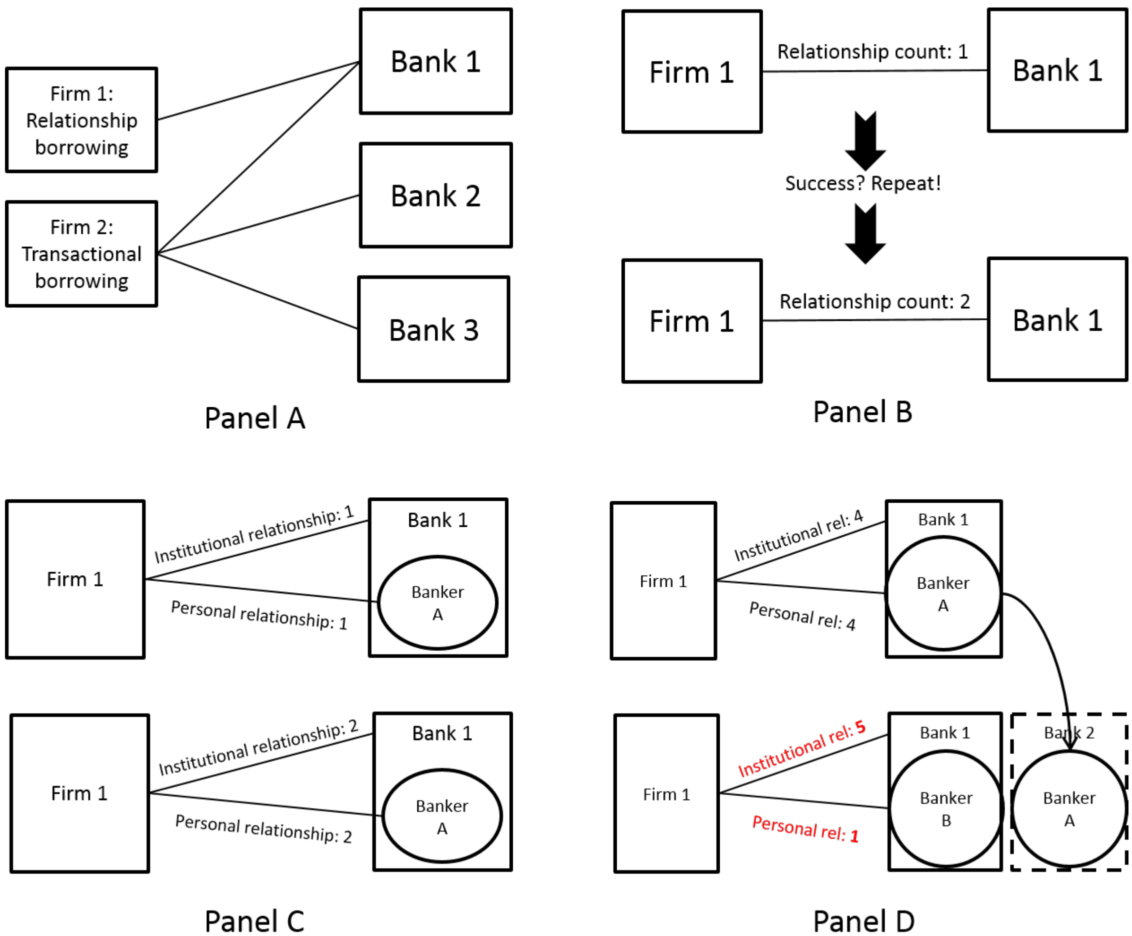


Table 1: Variable Descriptions

Variable name	Description
Firm characteristics	
Assets	Total assets
Leverage	Total liabilities, divided by total assets
Market to book	Shares outstanding times stock price, divided by book value of assets
Profitability	EBITDA/Assets
Rating	Long-term S&P credit rating
Industry fixed effects	Two-digit primary SIC code
Loan characteristics	
All in spread drawn	All in spread drawn above LIBOR
$\mathbb{1}_{Banker\ left}$	Indicator taking the value of one for the first loan after the lead banker with strongest repeated interaction left the bank
Bank fixed effect	Lead bank with highest institutional relationship with clients
Bankruptcy	Indicator equal to one if a borrower filed for bankruptcy at any point during the maturity of a loan
Financial covenants	Indicator of presence of at least one financial covenant
Institutional count	Number of interactions between lead bank and firm
Institutional duration	Duration in years since first interaction between lead bank and firm
Loan size	Total loan size
Loan type	Indicator for one of: *Revolver*, *Term Loan*, or *Other*
Maturity	Maturity of loan in years
Personal count	Number of interactions between a specific banker and borrower
Personal duration	Time since first interaction between a specific banker and borrower
Secured	Fraction of loan package secured (weighted by relative facility amounts)

Table 2: Summary Statistics

This table displays summary statistics for the main explanatory variables used in the paper. The sample consists of loans taken out by U.S. nonfinancial firms from DealScan, which is linked to machine-collected data described in Section 2.4. The sample period is 1996 to 2012. Panel B presents the results from comparing covariates of treated loans from loans in the control group. Treated loans are those issued after the departure of a relationship banker, that is, those for which the indicator variable *Banker left* takes the value of one. The control group is formed by all other loans. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Sample Characteristics

Variable	Mean	SD	25%	Median	75%	Min	Max	Observations
Personal count	1.43	0.87	1.00	1.00	2.00	1.00	5.00	4,430
Personal duration (years)	0.63	1.58	0.00	0.00	0.08	0.00	14.25	4,430
Num. relationships/banker	2.27	2.23	1.00	1.00	3.00	1.00	13.00	4,430
Number loans per firm	3.54	2.17	2.00	3.00	5.00	1.00	10.00	4,430
Institutional count	1.68	1.08	1.00	1.00	2.00	1.00	6.00	4,430
Banker left	0.08	0.27	0.00	0.00	0.00	0.00	1.00	4,430
Assets	4,079	7,884	374	1,128	3,633	19	48,375	4,430
Leverage	0.61	0.23	0.47	0.60	0.73	0.14	1.00	4,430
Market to book	1.05	0.98	0.43	0.75	1.32	0.01	5.81	4,430
Profitability	0.12	0.10	0.08	0.12	0.17	-0.28	0.45	4,430
Intangibles to assets	0.20	0.20	0.03	0.13	0.32	0.00	0.79	4,430
All in spread	181.55	121.52	87.50	160.00	250.00	20.00	590.71	4,430
Loan size (USD million)	586	944	100	250	650	5	6,000	4,430
Financial covenants indicator	0.76	0.43	1.00	1.00	1.00	0.00	1.00	4,430
Maturity (years)	3.75	1.71	2.71	4.00	5.00	0.08	20.00	4,430
Secured	0.51	0.49	0.00	0.70	1.00	0.00	1.00	4,430

Continued on next page

Panel B: Covariate Balance

This table presents the results from comparing covariates of treated loans from loans in the control group. Treated loans are those issued after the departure of a relationship banker, that is, those for which the indicator variable *Banker left* takes the value of one. The control group is formed by all other loans. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Banker left = 0	*Banker left* = 1	Differrence
Institutional count	1.673	1.788	-0.116*
Assets	4,046.013	4,474.389	-428.377
Leverage	0.61	0.63	-0.02
Market to book	1.052	0.979	0.074
Profitability	0.123	0.129	-0.007
Intangibles to assets	0.201	0.182	0.018
Credit rating	0.545	0.597	-0.052*
N	4,085	345	

Table 3: Banker Characteristics and Loan Outcomes

This table presents the results of high dimensionality fixed effects regressions of loan outcomes on firm control variables, as well as banker and bank fixed effects. Estimations are performed using the Abowd et al. (1999) methodology as implemented in Cornelissen (2008). Panel A presents results from regressions with and without banker and bank fixed effects as well as the corresponding model fit. Panel B reports F-statistics for tests of individual and joint statistical significance of the respective fixed effects, as well as a decomposition of their relative contribution in explaining variation in the respective dependent variables. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses and clustered by firm. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Main Regressions

	Spread		Ln amount		Ln maturity		Secured (%)		Financial covenants	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Institutional count	-1.448	-0.856	0.005	0.032	-0.016*	-0.014	0.018*	0.021*	-0.002	-0.007
	(1.553)	(1.921)	(0.018)	(0.024)	(0.009)	(0.014)	(0.010)	(0.011)	(0.008)	(0.011)
Log(Assets)	-16.942***	-14.028***	0.661***	0.579***	-0.036**	-0.066***	-0.081***	-0.079***	-0.119***	-0.122***
	(2.444)	(2.968)	(0.026)	(0.034)	(0.016)	(0.023)	(0.012)	(0.016)	(0.012)	(0.015)
Leverage	68.078***	68.373***	0.651***	0.436***	-0.089	-0.057	0.084	-0.035	-0.147***	-0.056
	(11.692)	(12.505)	(0.105)	(0.156)	(0.065)	(0.087)	(0.052)	(0.067)	(0.055)	(0.068)
Market to book	-6.658***	-3.042	-0.031	-0.005	-0.037**	-0.076***	-0.000	-0.001	-0.008	-0.008
	(2.530)	(3.373)	(0.032)	(0.034)	(0.016)	(0.023)	(0.013)	(0.018)	(0.013)	(0.016)
Profitability	-170.248***	-107.348***	0.222	-0.369	0.670***	0.869***	-0.467***	-0.378**	0.484***	0.427**
	(28.914)	(36.340)	(0.268)	(0.368)	(0.162)	(0.256)	(0.119)	(0.172)	(0.131)	(0.179)
Intangibles to assets	20.241*	29.861**	0.396***	0.118	0.156**	0.051	0.056	0.123	0.010	0.059
	(11.035)	(14.579)	(0.147)	(0.185)	(0.068)	(0.107)	(0.061)	(0.077)	(0.058)	(0.077)
N	2164	2164	2164	2164	2164	2164	2164	2164	2164	2164
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loantype FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Banker FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Bank FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Num. movers		614		614		614		614		614
Num. stayers		103		103		103		103		103
Adjusted R^2	0.637	0.871	0.675	0.859	0.501	0.750	0.462	0.767	0.275	0.701

Continued on next page

Panel B: F-Statistics and Explanatory Power

	Spread	Ln amount	Ln maturity	Secured (%)	Financial covenants
	(1)	(2)	(3)	(4)	(5)
N	2,164	2,164	2,164	2,164	2,164
F-Statistic joint F(1008,1226)	3.45	2.60	2.09	2.63	2.82
F-Statistic banker F(588,1226)	2.81	2.41	2.09	2.68	2.25
F-Statistic bank F(420,1226)	2.64	2.51	1.93	2.28	2.78
R-Squared of:					
Control variables	0.557	0.626	0.471	0.457	0.286
Bankers	0.200	0.151	0.232	0.236	0.230
Banks	0.150	0.121	0.117	0.139	0.269

Table 4: Simulation Results

This table presents the results for R-squared, F-statistics and individual contribution of banker fixed effects when estimating regressions of loan outcome variables on control variables, bank fixed effects and banker fixed effects. Bankers and banks are randomly assigned to loans in a simulation approach which iterates the random assignment 1,000 times and then reports the 90th, 95th, and 99th percentile of resultant simulated R squared, F-value of a joint test of significance of banker fixed effects, and the contribution of banker fixed effects to overall variation in the dependent variable. Statistical significance is based on whether sample values exceed the 90th, 95th, and 99th percentile for the simulated values. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Adjusted R^2				F-Value (Bankers only)				Relative contribution			
		Simulations				Simulations				Simulations		
	Sample	p90	p95	p99	Sample	p90	p95	p99	Sample	p90	p95	p99
Spread	87.1%***	79.22%	79.55%	80.40%	2.81***	1.99	2.06	2.24	20.0%***	16.04%	17.00%	18.93%
Amount (log)	85.9%***	76.31%	76.71%	77.59%	2.41***	1.99	2.04	2.16	15.10%	18.22%	19.09%	21.25%
Maturity (log)	75.0%***	72.15%	72.63%	73.92%	2.09*	2.01	2.09	2.28	23.2%**	21.19%	22.14%	24.23%
Secured (%)	76.7%***	67.95%	68.33%	69.23%	2.68***	1.95	2.00	2.12	23.6%*	23.47%	24.76%	26.53%
Financial Covenants	70.1%***	60.77%	61.29%	62.62%	2.25***	1.95	2.02	2.16	23.00%	28.40%	29.65%	32.00%

Table 5: Banker Styles

This table presents correlations between banker fixed effects regarding various loan dimensions. Fixed effects stem from the regressions presented in Table 3. Since fixed effects are calculated relative to the respective group of connected individuals, correlations are only based on the largest group of connected bankers covering about 90% of the sample. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses and clustered by bank. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)				
	Spread FE	Amount FE	Maturity FE	Secured FE	Financial covenants FE
Spread FE	1				
Amount FE	-0.158***	1			
Maturity FE	-0.0719**	0.227***	1		
Secured FE	0.235***	0.0815***	0.0618**	1	
Financial covenants FE	-0.0989***	0.361***	0.0180	0.0730***	1

Table 6: Interest Rates and Relationships: Instrumental Variable Regressions

This table presents the results of instrumental variable regressions in which the dependent variable is *Spread*, the all in spread drawn between the loan and LIBOR at initiation. The explanatory variable is *Personal count*, the number of previous interactions between the loan officer and the borrower. The instrument used for *Personal count* is *Banker left*, an indicator variable that takes the value one for the first loan of a firm after its lead banker left the relationship bank. Institutional count measures the number of interactions between the lead bank (as opposed to *banker*) and the borrower. The Cragg-Donald first-stage F-statistic refers to the most complete model specification in Column 5. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. *Loan controls* include *loan size*, *secured*, and *Financial covenants indicator*, as well as indicators for *loan type*. Controls never contain the respective dependent variable. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by firm and year. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	First Stage	Spread			
	(1)	(2)	(3)	(4)	(5)
Banker left	-0.671***				
	(0.060)				
Personal count		-55.125***	-16.203**	-33.892***	-12.144*
		(14.474)	(6.809)	(12.153)	(6.830)
Institutional count	0.072***		-1.164	6.181**	-0.811
	(0.021)		(2.178)	(2.722)	(2.044)
N	3473	3473	3473	3473	3473
Firm FE		Yes	Yes	Yes	Yes
Bank FE		No	No	Yes	Yes
Year FE		No	Yes	No	Yes
Rating FE		No	No	Yes	Yes
Firm controls		No	Yes	No	Yes
Loan controls		No	No	Yes	Yes
Cragg-Donald F-statistic	123.1				

Table 7: Interest Rates and Relationships: Panel Regressions

This table presents the results of panel regressions in which the dependent variable is *Spread*, the interest rate spread between the loan and LIBOR at initiation. The explanatory variable is *Personal count*, the number of previous interactions between the banker and the borrower. Institutional count measures the number of interactions between the lead bank (as opposed to *banker*) and the borrower. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. *Loan controls* include *loan size*, *secured*, and *financial covenants indicator* as well as indicators for *loan type*. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by firm and year. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Spread			
	(1)	(2)	(3)	(4)
Personal count	1.440	-3.976**	-1.264	-3.683**
	(2.252)	(1.618)	(2.652)	(1.552)
Institutional count		-2.185	1.689	-1.443
		(2.070)	(2.769)	(1.943)
N	3,473	3,473	3,473	3,473
Firm FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	Yes	Yes
Year FE	No	Yes	No	Yes
Rating FE	No	No	Yes	Yes
Firm controls	No	Yes	No	Yes
Loan controls	No	No	Yes	Yes

Table 8: Personal Relationships and Bankruptcy

This table presents the results of linear probability regressions in which the dependent variable is an indicator taking the value one if the borrower went bankrupt during the maturity of the loan. The main explanatory variable is *Personal count*, the number of previous interactions between the banker and the borrower. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. *Loan controls* include *loan size*, *secured*, and *financial covenants indicator* as well as indicators for *loan type*. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by firm and year. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Bankruptcy*100			
	(1)	(2)	(3)	(4)
Personal count	-0.673***	-0.666***	-0.727***	-0.754***
	(0.061)	(0.111)	(0.125)	(0.117)
Institutional count		-0.466**	-0.420**	-0.441**
		(0.199)	(0.201)	(0.202)
N	4,367	4,367	4,367	4,367
R^2	0.001	0.064	0.076	0.079
Industry FE	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	Yes
Rating FE	No	Yes	Yes	Yes
Firm controls	No	No	Yes	Yes
Loan controls	No	Yes	No	Yes

Table 9: Personal Relationships and Renegotiations

This table presents the results of regressions of loan renegotiations outcomes on measures of personal relationship intensity. The main explanatory variable is *Personal count*, the number of previous interactions between the banker and the borrower. The dependent variables *Del. amount* and *Del. maturity* are the changes in loan amount and loan maturity, respectively. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by industry. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Del. amount		Del. maturity	
	(1)	(2)	(3)	(4)
Personal count	-3.478	-8.636***	-0.128	0.428
	(3.076)	(1.751)	(0.703)	(0.529)
Institutional count	17.938***	11.570***	-0.294	-0.980
	(2.427)	(3.148)	(0.845)	(0.702)
Personal count × junk		0.179		-4.141*
		(8.620)		(2.256)
Junk rated		9.175		11.357***
		(13.946)		(3.854)
N	493	493	493	493
R^2	0.009	0.008	0.046	0.046
Industry FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Rating FE	Yes	No	Yes	No
Firm controls	Yes	Yes	Yes	Yes

Table 10: Personal Relationships and Lender Choice

This table presents the results of linear probability and logistic regressions in which the dependent variable is initiation, an indicator variable taking the value one if a bank is lead agent for a loan for a borrower it has not been lead agent for in the preceding two years. The main explanatory variable is *Personal relationship acquired*, an indicator variable taking the value one if a bank hired a banker with a previous personal relationship with a client. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses and clustered by bank. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	LPM			Logit		
	(1)	(2)	(3)	(4)	(5)	(6)
Personal Rel. Acquired	0.002***	0.040***	0.023***	3.727***	3.710***	0.870***
	(0.000)	(0.002)	(0.003)	(0.122)	(0.123)	(0.222)
N	31,022,779	31022779	31,022,779	31,022,779	29,200,000	29,106,560
R^2	0.000	0.003	0.001			
Year FE	No	Yes	Yes	No	Yes	Yes
Bank FE	No	No	Yes	No	No	Yes

Appendix for
"The Role of Bankers in the U.S. Syndicated Loan Market"

A Data Example

This section presents a detailed example of the banker-borrower-matched dataset used in the paper and described in Section 2. I will explain the data using the example of loan officer number 171. Although all information used in this section, as well as in the rest of the paper, is public, I will anonymize information that could identify officer 171. According to his profile on an on-line career network and a separate profile on Bloomberg Business, officer 171 obtained his MBA from a U.S. business school in the mid-1980s before joining his first employer, which I will refer to as *Bank A*. In the mid-1990s he was promoted to team leader in *Bank A's* media division.

The first time I record a signature from officer 171 is in 1996, when he signs a loan contract for a firm in the printing and publishing industry. Officer 171 continues to appear in the dataset until 2012, signing a total of 38 individual loans, or about 2.2 loans per year. Thirty-three of those deals are in the communications industry (SIC: 48); two loans are in the business services industry (SIC: 73); and three remaining loans are in related industries. Officer 171 represented (one of) the lead bank(s) in 16 of those cases. During that time, he arranged loans for 27 different borrowers. The maximum number of repeated loans with the same customer is four loans over the course of 9 years, from 1996 to 2005, with a client in the media industry.

Between the fall of 2004 and spring 2005, officer 171 switches to a different bank, *Bank B*. Of his 38 loans, 12 are signed while he is working for *Bank B*. Out of the four loans with his most intensive borrower, three are signed while at *Bank A* and one at *Bank B*. Officer 171 also makes significant career progress: When he signs his first contract in my sample in 1996, he holds the title of assistant vice president. After his change to *Bank B* he is named General Manager, before he gets promoted to Director in 2006.

This example illustrates how the data collection process identified and tracked commercial bankers over long periods of time and multiple employers.

Table A1: Persistence of Banker Effects

This table presents regressions of average banker-bank residuals for various loan characteristics in one firm on the residuals of the same banker at previous firms, similar to Bertrand and Schoar (2003) and Fee et al. (2013). Residuals stem from regression of loan outcomes on various control variables and bank fixed effects. The resultant residuals are then averaged at the banker-bank level. The table presents the coefficients from regressions of those average residuals on banker fixed effects. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Spread	Ln amount	Ln maturity	Secured (%)	Financial covenant
	(1)	(2)	(3)	(4)	(5)
Spread	0.392***				
	(0.0303)				
Ln amount		0.552***			
		(0.0572)			
Ln maturity			0.551***		
			(0.0528)		
Secured				0.541***	
				(0.0442)	
Financial covenant					0.643***
					(0.0714)
N	419	419	419	419	419

Table A2: Sample Splits fixed Effects

This table presents sample splits for banker fixed effects regressions. The table reports the fraction of the observed variation in the outcome variable that can be explained by banker fixed effects. The dependent variables in columns 1,2,3,4, and 5 are loan *spread*, *amount*, *maturity*, *secured* and *financial covenant present*. Rows one and two present a sample split for firms below (above) $ 1bn in assets, rows three and four present results for firms without (with) a credit rating, and rows five and six present results for loans which are junk rated (not junk rated). The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Spread	Ln Amount	Ln Maturity	Secured (%)	Financial Covenant
	(1)	(2)	(3)	(4)	(5)
N	2272	2272	2272	2272	2272
sample small	0.309	0.279	0.427	0.385	0.369
sample large	0.184	0.237	0.201	0.268	0.254
sample unrated	0.312	0.254	0.406	0.401	0.391
sample rated	0.189	0.223	0.212	0.219	0.239
sample junk	0.259	0.286	0.289	0.389	0.259
sample notjunk	0.189	0.193	0.187	0.284	0.222

Table A3: Interest Rates and Relationships: Instrumental Variable Regressions Relationship Duration

This table presents the results of instrumental variable regressions in which the dependent variable is *Spread*, the interest rate spread between the loan and LIBOR at initiation. The explanatory variable is *Personal duration*, the time since the first interaction between the loan officer and the borrower. Institutional count measures the number of interactions between the lead bank (as opposed to *banker*) and the borrower. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. *Loan controls* include *loan size*, *secured*, and *Financial covenants indicator* as well as indicators for *loan type*. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by borrower and year. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	First stage	Spread			
	(1)	(2)	(3)	(4)	(5)
Banker left	-1.032***				
	(0.141)				
Personal duration		-45.283***	-10.570**	-25.926***	-7.912*
		(13.211)	(4.747)	(9.925)	(4.663)
Institutional count	0.055		-1.645	7.058**	-1.250
	(0.039)		(2.088)	(3.198)	(1.942)
N	3,473	3,473	3,473	3,473	3,473
Firm FE		Yes	Yes	Yes	Yes
Bank FE		No	No	Yes	Yes
Year FE		No	Yes	No	Yes
Rating FE		No	Yes	Yes	Yes
Firm controls		No	Yes	No	Yes
Loan controls		No	No	Yes	Yes
Cragg-Donald f-statistic	84.85				

Table A4: Interest Rates and Relationships: Instrumental Variable Regressions Industry Fixed Effects

This table presents the results of panel regressions in which the dependent variable is *spread*, the interest rate spread between the loan and LIBOR at initiation. The explanatory variable is *Personal count*, the number of previous interactions between the loan officer and the borrower. Institutional count measures the number of interactions between the lead bank (as opposed to *banker*) and the borrower. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. *Loan controls* include *loan size*, *secured*, and *Financial covenants indicator* as well as indicators for *loan type*. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by industry and year. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	First stage	Spread			
	(1)	(2)	(3)	(4)	(5)
Banker left	-0.404***				
	(0.041)				
Personal count		-50.520**	-16.258	-27.070**	-7.789
		(23.101)	(12.100)	(12.465)	(11.590)
Institutional count	0.138***		2.372	5.659**	1.846
	(0.005)		(2.230)	(2.631)	(1.845)
N	4,430	4,430	4,430	4,430	4,430
Industry FE		Yes	Yes	Yes	Yes
Bank FE		No	No	Yes	Yes
Year FE		No	Yes	No	Yes
Rating FE		No	No	Yes	Yes
Firm controls		No	Yes	No	Yes
Loan controls		No	No	Yes	Yes
Cragg-Donald F-statistic	70.48				

Table A5: Interest Rates and Relationships: Difference in Differences Regressions

This table presents the results of cifference-in-differences regressions in which the dependent variable is *spread*, the interest rate spread between the loan and LIBOR at initiation. The explanatory variables are *Personal count*, the number of previous interactions between the loan officer and the borrower, *Banker left*, an indicator taking the value one for the first loan issued by a borrower after its relationship banker left the sample, and the interaction of the (lagged) *Personal count* and *Banker left*. Institutional count measures the number of interactions between the lead bank (as opposed to *banker*) and the borrower. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. *Loan controls* include *loan size*, *secured*, and *Financial covenants indicator* as well as indicators for *loan type*. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by firm and year (columns 1 to 4) and industry and year (columns 5 and 6). *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Spread					
	(1)	(2)	(3)	(4)	(5)	(6)
Personal count	3.744*	-1.075	0.879	-2.377	-0.366	-1.168
	(2.208)	(1.645)	(2.367)	(1.663)	(1.265)	(1.165)
Banker left	1.535	-11.943	-5.860	-16.238	-16.571**	-17.923***
	(14.866)	(11.649)	(14.914)	(11.969)	(6.468)	(4.422)
Personal count X Banker left	14.825**	11.789*	13.898*	12.315*	11.829***	13.108***
	(7.253)	(6.622)	(7.632)	(7.466)	(3.120)	(1.754)
N	3,473	3,473	3,473	3,473	4,430	4,231
Firm FE	Yes	Yes	Yes	Yes	No	No
Industry FE	No	No	No	No	Yes	No
Industry × year FE	No	No	No	No	No	Yes
Bank FE	No	No	Yes	Yes	Yes	Yes
Year FE	No	Yes	No	Yes	Yes	No
Rating FE	No	No	Yes	Yes	Yes	Yes
Firm controls	No	Yes	No	Yes	Yes	Yes
Loan controls	No	No	Yes	Yes	Yes	Yes

Table A6: Interest Rates: Truncated Sample

This table presents the results of instrumental variable regressions in which the dependent variable is *Spread*, the interest rate spread between the loan and LIBOR at initiation. The explanatory variable is *Personal count*, the time since the first interaction between the loan officer and the borrower. Institutional count measures the number of interactions between the lead bank (as opposed to *banker*) and the borrower. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. *Loan controls* include *loan size*, *secured*, and *Financial covenants indicator* as well as indicators for *loan type*. The sample consists of U.S. nonfinancial firms. To account for left censoring of the sample, it starts in 1998, 2000 and 2002 in the three pairs of columns, respectively. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by borrower and year. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Starting 1998		Starting 2000		Starting 2002	
	First stage (1)	Spread (2)	First stage (3)	Spread (4)	First stage (5)	Spread (6)
Banker left	-0.988***		-1.069***		-1.064***	
	(0.184)		(0.190)		(0.208)	
Personal count		-23.092***		-28.547***		-36.531**
		(8.433)		(10.243)		(15.300)
Institutional count	0.065	0.928	0.066	-0.303	0.019	-3.576
	(0.050)	(2.746)	(0.060)	(3.101)	(0.077)	(3.873)
N	2,996	2,996	2,614	2,614	2,081	2,081
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Cragg-Donald F-statistic	44.8		43.5		29.8	

Table A7: Interest Rates: Bank-Year and Banker Fixed Effects

This table presents the results of instrumental variable regressions in which the dependent variable is *Spread*, the interest rate spread between the loan and LIBOR at initiation. The explanatory variable is *Personal count*, the time since the first interaction between the loan officer and the borrower. Institutional count measures the number of interactions between the lead bank (as opposed to *banker*) and the borrower. Columns 1 and 2 control for joint bank-year fixed effects. Columns 3 and 4 control for banker fixed effects. *Firm controls* include *firm size*, *leverage*, *profitability*, *intagibles to assets*, and *market to book*. *Loan controls* include *loan size*, *secured*, and *Financial covenants indicator* as well as indicators for *loan type*. The sample consists of U.S. nonfinancial firms from 1996 to 2012. All variables are explained in Table 1. Standard errors are reported in parentheses, robust to heteroscedasticity, and clustered by borrower and year in Columns 1 and 2, and banker and year in Columns 3 and 4. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	First stage	Spread	First stage	Spread
	(1)	(2)	(3)	(4)
Banker left	-0.555***		-0.922***	
	(0.088)		(0.240)	
Personal count		-15.900*		-31.011**
		(9.507)		(12.332)
Institutional count	0.144***	-0.286	0.097**	-1.658
	(0.037)	(2.943)	(0.048)	(3.717)
N	3,468	3,468	2,166	2,166
Bank × year FE	Yes	Yes	No	No
Bank FE	No	No	No	No
Year FE	No	No	Yes	Yes
Banker FE	No	No	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes
Cragg-Donald F-statistic	36.3		27.9	