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

Christoph Herpfer†

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Abstract

I construct a novel dataset of individual bankers in the U.S. syndicated loan market to analyze the impact of bankers for the largest, most transparent borrowers. I exploit within-firm variation in personal relationship strength from banker turnover and find that stronger relationships lead to significantly lower interest rates. Relationship loans are associated with fewer bankruptcies and fewer favorable modifications in renegotiations. Lower rates therefore derive from increased lending efficiency, rather than nepotism. While personal relationships generally increase credit availability, during the financial crisis these relationships locked in borrowers with affected banks. Bankers also exhibit time-invariant preferences for specific loan characteristics.

Keywords: Asymmetric information; Bank lending; Cost of debt; Professional connections; Lending outcomes; Bankers

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†Emory University, Goizueta Business School, 1300 Clifton Road, 30322 Atlanta, GA, USA; christoph.herpfer@emory.edu; www.herpfer.com.

1 Introduction

Syndicated loans are jointly funded by two or more lenders. These loans amount to more than $4 trillion, and they 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 that are provided to small- and medium-sized borrowers. In both types of loans, 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, syndicated loans are larger, involve a wider availability of information on borrowers, and are based on a shared commitment by multiple lenders. This makes them significantly different from loans to smaller, more opaque borrowers.

In this paper, I examine the role of individual bankers in 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 less evidence that bankers play a role in the setting of large, transparent borrowers. I find that individual bankers impact lending even for large, transparent borrowers that interact with sophisticated U.S. lenders, where their role should be smallest due to the large amounts of hard, publicly available information. I find that even in this setting, commercial bankers impact lending on three dimensions:

First, they form personal relationships with clients over time. These relationships lead to lower interest rates as bankers gather soft information about borrowers and build trust, which can lower monitoring and contracting costs. Second, I show that bankers play an important role in matching banks to borrowers, which is consistent with banker relationships being

[1] Shared National Credit Program report for 2016Q1, available at goo.gl/NqZuZ6. The Shared National Credit Program covers loans that feature at least three supervised lenders and exceed $20 million.

valuable to customers. Finally, I show that bankers exhibit time-invariant preferences for specific loan characteristics.

The syndicated loan market is a particularly promising empirical laboratory for testing the effect of individual bankers on bank loans. This is because borrowers in this market tend to be large firms with large amounts of publicly available data, such as audited financial statements or analyst reports. The prior literature on the role of loan officers in lending focuses on *character loans* made to small, opaque borrowers. For these character loans, individual bankers are often the only source of information available to banks. In contrast, bankers and their relationships with clients should matter least for borrowers with large amounts of available hard information: Under perfect information, there should be a unique, optimal loan contract for each borrower. And, empirically, firms that have more publicly available information are less likely to self-select into relationship lending (Sufi, 2007; Bharath, Dahiya, Saunders, and Srinivasan, 2011). I find that, despite all the publicly available information, individual bankers play an important role in the lending process even for the largest borrowers.

The first set of results in this paper focuses on how personal relationships between bankers and clients impact loan pricing. Bankers and borrowers form relationships as they interact repeatedly. I investigate the role of these personal relationships, and I carefully address the endogenous nature of these personal relationships: lower quality borrowers can self-select into relationships (Sufi, 2007), and personal relationships between individual bankers and borrowers develop simultaneously to institutional relationships between banks and borrowers, which impact lending terms (Bharath et al., 2011).

Therefore, I identify 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 who have a strong personal relationship with the 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 (bp) lower interest rates. The economic magnitude of this effect is large and corresponds to an annual savings of $275,000 at the median loan size of $250 million. Importantly, this result is equivalent to interest rates *increasing* after a banker leaves a bank and the borrower returns for another loan. This result alleviates concerns that the correlation between stronger relationships and lower rates is driven by survivorship bias, since survivorship bias would predict lower interest rates with each additional loan.

Two economic channels can explain why loans with strong personal relationships feature lower interest rates. First, bankers can gather information and develop trust throughout the course of their relationship with borrowers, thereby reducing information asymmetry and monitoring costs.[2] Alternatively, lower rates might result from agency conflicts between the bank and its employees; borrowers can reward bankers for lower interest rates, either directly (i.e., through monetary kickbacks) or indirectly (e.g., through invitations to social events). If bankers receive personal gains in return for granting cheap loans, these lower interest rates would reflect nepotism rather than superior efficiency.

These two competing explanations can be tested by comparing the subsequent performance of relationship loans with that of nonrelationship loans, as in Bolton, Freixas, Gambacorta, and Mistrulli (2016) and Gopalan, Nanda, and Yerramilli (2011). 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 or the ability to contract, strong relationship loans will be associated with superior subsequent loan performance. As predicted by the monitoring and information channel model, I find that a one standard deviation increase in personal relationship strength is associated with a 20%

[2]For ease of exposition I use the term *information* broadly. Stronger personal relationships (a) can make monitoring borrowers less costly, (b) can make negotiating new contracts easier, or (c) can be associated with more trust between bankers and clients. All of these result in lower interest rates.

relative reduction in bankruptcy likelihood when compared to the unconditional mean.[3]

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 forcing borrowers into bankruptcy (Haselmann, Schoenherr, and Vig, 2018). To rule out that the inefficient rolling over of loans drives my findings, I also analyze the effect of personal relationships on loan renegotiations, and I find no increase in loan size or maturity for high personal relationship loans. If anything, relationship loans are *reduced* in size and maturity upon renegotiation when compared to nonrelationship loans.

Compared to the literature on lending to small, opaque clients, in which researchers have access to internal bank data or are allowed to randomize loan officer assignment, the public data used in this study complicates identification. There might be an unobservable shock that simultaneously explains (a) why interest rates rise after a banker with strong personal relationships leaves the bank, (b) why this effect is stronger for opaque borrowers, and (c) why these relationship loans are associated with fewer bankruptcies and a lack of favorable renegotiations. I do however, not see a plausible alternative mechanism that can explain the combination of those findings.

In the second set of results, I investigate how bankers match lenders to borrowers. Since strong relationships with a specific banker lead to lower interest payments, borrowers should be willing to follow their bankers when they switch employers. I show that 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 new banking relationship with her new employer.

[3]Banker turnover is potentially not exogenous to interest rates. It may be the case that banker turnover and the subsequent rise in interest rates are both driven by banks terminating bankers for granting loans that are too cheap. To alleviate these concerns, in unreported results, I consider only those loans affected by banker turnover if the banker in question completely leaves my sample; i.e., she does not sign any loans in the future for any bank. Since even bankers who are associated with adverse loan outcomes often find employment with another bank (Gao, Kleiner, and Pacelli, 2018b), those bankers who ultimately leave the sample are likely to have either retired or died. These shocks are less likely to be driven by a concurrent firm-level shock; despite a reduction in the number of treated observations, these results are economically and statistically very similar to those from the main analysis. In another robustness test, I find that results are robust to including banker fixed effects in the estimation to control for a potential tendency of some bankers to grant cheaper loans.

There is a downside for borrowers to commit to a lending relationship with a single banker. I find that, during the financial crisis, borrowers with strong personal relationships were significantly less likely to obtain a loan from a new lender compared to borrowers with weak personal relationships. It was not the case that relationship borrowers didnt switch lenders because they had no need for funding: During normal times, borrowers with strong personal relationships have easier access to credit, but during the crisis, these borrowers were significantly less likely to obtain a loan. Consistent with relationship borrowers being locked into their lender, I find that the restriction on access to new loans was more severe for clients who borrowed from banks that were more affected by the crisis. These results form an important microfoundation for the established fact that bank-dependent borrowers suffer real consequences during financial crises (Chodorow-Reich, 2014; Chava and Purnanandam, 2011).

Finally, I provide evidence that bankers have time-invariant preferences for loan characteristics. In fixed effects estimations, I find that banker fixed effects explain between 10 and 25 percent of the variation in various loan characteristics, and they explain up to two and a half times as much variation as do bank fixed effects. These results are more pronounced for more opaque borrowers, and they are robust to various estimation techniques.

This 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 small business lending, and the literature on institutional relationship lending.

A few authors explicitly investigate the impact of connections between corporate executives and banks. Karolyi (2018) exploits unexpected executive turnover as a shock to the personal relationship between corporate executives and banks. Karolyi finds that borrower executives who have stronger personal relationships with lending banks obtain lower credit spreads, and executives are more likely to borrow from banks they have interacted with in the past. My findings add to this finding in two ways. First, I investigate a different economic channel through which personal relationships impact lending: Bankers impact lending through their personal characteristics and through the relationships they form with borrowers over time, whereas the economic channel proposed in Karolyi (2018) is that executives can commit to a behavior in different ways than their corporations (e.g., due to reputation concerns). Second, I analyze the importance of individuals in lending from the other side (i.e., that of bankers) rather than from the side of borrower-executives.

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 (e.g., 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 et al. (2018) 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; instead, they find 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 a shared *past* (Engelberg et al., 2012) or from *current* (Haselmann et al., 2018) *social* interactions. I study personal interactions that (a) are professional rather than social in nature, (b) arise from current collaborations on loans, and (c) 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 decrease interest rates by a similar magnitude as do past social ties, as 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. (2018) that seem to foster nepotism, professional relationships are associated with fewer bankruptcies and seem to increase lending efficiency. This 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.[4]

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, 2018), 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, 2017). I add to this literature in two ways. First, the detailed microlevel data required in past studies of loan officers limits them to proprietary datasets obtained from a single lender. Data from a single bank cannot be used to disentangle the effects of individuals from the effects of institutions since, as those papers show, any individual effect strongly depends on banks respective structures and incentive systems.

Second, those papers study the interactions between loan officers and small borrowers, whereas the individuals in my study are commercial bankers that issue large, syndicated loans to large corporations. The prior literature stresses that soft information plays a larger role in smaller banks Berger, Miller, Petersen, Rajan, and Stein (2005). I show that bankers matter even for large borrowers and sophisticated lenders.[5]

Compared to large borrowers in the U.S. syndicated loan market, small, opaque borrowers generally provide both less information and lower quality information such as audited annual reports, credit ratings, or analyst reports. It is therefore not surprising that individual loan

[4]Another potential explanation for this difference in findings could be the different samples used. This paper and Engelberg et al. (2012) study large borrowers and private banks in the U.S. (average loan size: $586mn), whereas Haselmann et al. (2018) study smaller borrowers in Germany (average loan size: EUR 9mn) and find that nepotism is strongest for state-owned banks.

[5]Average loan size in the above studies ranges from $500 to $600,000. By comparison, the average loan size in my sample is $586,000,000, larger by a factor of 100 than the largest average loan size in the previous studies. One notable exception is Liberti, Seru, and Vig (2015), who use data from a large U.S. commercial lender. The average loan size in their sample is $3,000,000, which is still only about 1% of the average loan size in my sample.

officers play a role in small business lending, since they are often 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.[6]

Other authors have investigated the relative contributions of individuals compared to those of institutions (i.e., the firms employing those individuals) in a variety of contexts.[7] I add to this literature by extending it to the setting of bank lending and by linking individual bankers to specific loans.

This paper also adds to the emerging literature on the importance of bankers in various aspects of the syndicated loan market. Frattaroli and Herpfer (2018) find that different bankers collaborate to use their knowledge of borrowers to broker strategic alliances between their clients; i.e., there are spillovers of information gathered during the lending process. In concurrent work, Gao, Martin, and Pacelli (2017) conduct a study similar to the fixed effects portion of this paper. A key difference between the two papers is that I focus on lead banks and the bankers who work for lead banks, since these 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); in contrast, Gao et al. (2017) consider all lenders and their bankers equally and focus on the differential impact of bankers across organizations. In another paper, Gao et al. (2018b) find that bankers face career consequences when one of their borrowers defaults. Finally, Gao, Karolyi, and Pacelli (2018a) find that when bankers are distracted, their ability to screen and monitor loans is impaired. All of these papers rely on the assumption that bankers causally impact lending

[6]While the novelty of these results makes them interesting, it of course also reduces their transferability to the prior literature on small opaque borrowers.

[7]Some authors find that executive-specific characteristics impact a wide range of corporate characteristics. Executive fixed effects can explain a significant fraction of the variations in 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 (2018) 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 that exceeds that of their venture capital funds. Mukharlyamov (2016) finds a relationship between banks' aggregate workforce composition and bank risk-taking and Berger and Udell (2004) show on the aggregate level that, as banks' loan officer pool deteriorates, lending decisions become worse.

decisions and possess soft information about borrowers. This paper provides direct evidence of this fact.

Finally, this paper adds to the large existing body of research on the role of institutional relationship lending by showing that it is the individual banker who allows banks to collect, store, and interpret information.

The remainder of this paper is organized as follows: Section 2 describes the data collection process on individual bankers and the resultant dataset. Section 3 focuses on the role of time-varying personal relationships between bankers and borrowers on loan pricing and performance. Section 4 documents the role that bankers play in forming and maintaining bank-borrower relationships. Section 5 presents the results of analyzing the impact of time-invariant banker fixed effects on loans. 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 DealScan-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. These signature pages contain information on both the banks involved in the deal and the bankers associated with each lender. Firms are generally required to submit their loan contracts to the SEC in accordance with item 601(b) of Regulation S-K.[8]

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. I start with all available 8-K, 10-K, and 10-Q filings for sample firms, which are obtained from the SEC's EDGAR filing system and employ a text search similar to Nini, Smith, and Sufi (2009) to identify regulatory filings with attached loan contracts.

The search program then identifies the signature page, which contains the names and titles of all bankers representing lenders in the deal. Once it has found the signature page, the program extracts the information on bankers and their respective banks.[9] Figure 1 gives an example of a signature page and the different items extracted.

The circles mark the name of the bank (Wells Fargo), the bank's role (Administrative Agent), the banker's name (D. N.), and the banker's title (Vice President).[10]

The resultant dataset contains information on both the institutions and persons involved in each deal. Appendix A.1 contains further details of the data as well as quality checks. To confirm the text extraction program's efficiency, I randomly sampled 100 of the potential contracts, and I compared the results from the text search to the actual contracts.

Not all contracts contain information that can be extracted. In a random sample of 100 contracts, manual inspection revealed that 35, or about one-third of contracts, do not

[8]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.

[9]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 (e.g., typos) and intentional spelling choices, such as the omission of middle initials or the use of abbreviations.

[10]For the sake of privacy, I removed the banker's full name from the image.

contain information on the name of signers in the original document. I exclude contracts from the sample that do not feature signatures. Manual inspection finds that the text search correctly identified 80% of lead bankers.

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, at most, only attenuate the results.

2.2 Measuring personal relationship strength

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 interactions over time (Liberti and Petersen, 2017; Berger and Udell, 2006), there are two natural proxies for the strength of relationships between bankers and borrowers: (a) the number of prior interactions and (b) the duration since the first interaction. My main results use the number of prior interactions between a banker and a borrower, measured as the number of signed contracts (*Personal count*). I use all contracts available through the SEC to measure relationship strength, not just those I can link to DealScan. In Appendix A.3, I provide more detailed discussions of these measures and Appendix B.1 provides various robustness tests, such as using duration rather than count and measuring relationship strength only within the DealScan linked sample rather than using all contracts.

2.3 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 personal count is bounded from above by the total number of loans obtained by a borrower (for example, any borrower's first loan in the sample will always be assigned a *Personal count* equal to 1). 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.[11]

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* that corresponds to the time since the first loan contract was 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.[12] 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 1.68, on average, which is slightly larger than personal count, at an average of 1.43 interactions. Since bankers leave the sample, for example, due to retirement or a career switch, it is not surprising that the number of

[11]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.

[12]The median loan is the first interaction between a borrower and banker, hence *Duration* takes the median value of 0. The distribution is highly skewed: The maximum value of *Personal duration* is more than 14 years.

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 who has the strongest relationship to the borrower, i.e., 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, if 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.[13] 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 borrower in my sample is large, with a mean (median) of \$4.1 billion (\$1.1 billion) 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 1.05, on average. Sample firms exhibit an EBITDA-to-assets ratio (*Profitability*) of 12%, on average. The average firm has 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 (i.e.,

[13] I require that the pre-departure count between the banker and the firm is at least two; that is, I required that a relationship did in fact exist. This restriction is equivalent to requiring an actual personal relationship before the banker leaves. Otherwise, I could not differentiate between borrowers that are transactional in nature and borrowers that intended ex ante to form a relationship with the borrower but did not do so due to the banker leaving. To illustrate this, imagine two borrowers who receive their first loan from a specific banker and a specific bank in the same year. One of them is a transactional borrower who does not intend to return to the bank, the other is a relationship borrower who intends to return. Let us assume the banker leaves the bank after the initial loan. Both borrowers will have a relationship count of 1 on their next loan. However, for one borrower, this is the outcome of being a transactional borrower, while for the other borrower, it is the result of a prematurely terminated relationship. Assigning both of these next loans the *banker switched* indicator will confound the effect of transactional borrowing with that of a shock to an existing relationship. Requiring a prior relationship count of at least two loans allows me to separate these two effects. A potential pitfall with this variable is that a banker might not sign a particular loan (or my algorithm might fail to extract a signature), although the banker was actually involved in some future deal. I therefore require that bankers do not appear again on any loans between their current employer and the borrower. I also exclude cases in which two banks merge, which mechanically creates a change in the banker's employer.

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 considerably in the sample. The average loan is priced at 182 bp above LIBOR, with the minimum spread being just 20 bp, and the maximum spread standing at 591 bp. A 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 1 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 20 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).[14]

Since the instrumental variable analysis in Section 3 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 and statistically different for firms that experience banker turnover: Firms associated with loans after a banker has left the bank have slightly higher institutional relationship strength and are 5% more likely to have a credit rating compared to the rest of the sample. Both institutional relationships and the presence of a credit rating should lead to lower interest rates among the

[14]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 (i.e., on the package level), all analyses are conducted on the package level, and the relevant loan characteristics (e.g., interest rates) correspond to the value-weighted averages of the individual loan facilities.

loans marked as the banker having left (Bharath et al., 2011). Therefore, 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 both the treatment and control firms do not exhibit meaningful differences in observable variables.

3 Time-varying impact of bankers on loans

This section analyzes the impact of banker-borrower personal relationships on initial interest rates and subsequent firm and loan performance.

3.1 Personal relationships and interest rates

A test of the impact of personal relationship formation on loan terms achieves two goals: First, it investigates a specific economic channel through which bankers can impact lending, forming relationships with borrowers through repeated interactions. 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 illustrates these challenges.

[Figure 2 about here]

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 borrow repeatedly 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 will default on loans less frequently. Survivorship leads to a mechanical association between (potentially unobservable) financial health and more interactions between borrowers and banks, which would bias estimates of relationship strength toward lower rates.

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, it is not feasible to disentangle the impact of personal and institutional relationship strength.

I propose an instrumental variable approach to overcome these three challenges. The instrumented variable is personal relationship intensity. The proposed instrument consists of an indicator variable equal to 1 if a banker switches her employer, and 0 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 her 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 5, but a personal relationship count of only 1.

The instrument is therefore *Banker left*, an indicator variable that takes the value of 1 for the first loan between a borrower and a lender after the borrower's main relationship banker has left the bank.[15]

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

[15] As a nonparametric first pass, I compare the last loan before a banker switches with the first loan after the switch. I find that the mean personal relationship strength falls by about 0.8 interactions, and about 1 year. The residuals of a regression of interest rates on the full set of firm and loan control variables is -1.5bp before departure, consistent with strong relationships being associated with relatively lower rates, and +3.8 bp on the first loan afterward. This simple example illustrates that personal relationships fall and interest rates rise after a turnover in bankers.

to a drop in personal relationship strength as long as secondary relationship bankers have weaker relationships with the borrower.[16]

The departure of a relationship banker also fulfills the exclusion condition as long as banker turnover is unrelated to other factors that impact 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 other unobservable shocks. I undertake a number of tests to verify that my results are not driven by such unobservable shocks in Appendix B.1.

Formally, I estimate

$$Spread_j = \alpha_i + \phi_t + \theta_m + \beta \widehat{Personal\ count}_{i,j,t} + \gamma' X_{i,j,t} + \epsilon_j, \tag{1}$$

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. The variable $X_{i,j,t}$ denotes a vector of time-varying firm and loan controls. 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 estimation is performed using a 2-stage least-squares fixed effects regression.[17] In the first-stage equation, *Banker left* instruments for $\widehat{Personal\ count}$. The specification is

[16] In about 10% of cases of banker turnover the borrower subsequently takes a loan from a banker with a pre-existing relationship.

[17] In unreported results, I repeat these tests using first differences rather than fixed effects. The estimated coefficients are both economically and statistically very similar to those from the fixed effects specifications. For example, the coefficient on *Personal count* in the most complete specification is -16 bp and significant at the 1% level, hence it is both statistically and economically more significant than the coefficient in the fixed effects estimates.

$$Personal\ count_j = \alpha_i + \phi_t + \theta_m + \rho\mathbb{1}_{Banker\ left} + \gamma' X_{i,j,t} + u_j, \tag{2}$$

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 3 presents the results of estimating equations 1 and 2 using 2-steps least squares. The key explanatory variable of interest is $Personal\ count$, which is the measure of personal relationship strength between commercial bankers and firms.

[Table 3 about here]

Column 1 of Table 3 reports the results of estimating the first stage, equation 2, a regression of personal relationship count on control variables and the instrument $\mathbb{1}_{Banker\ left}$. For the sake of exposition, I report only the first stage that corresponds to the most complete model (Column 4). Each estimation in columns 2, 3, and 4 is of course based on a separate 2SLS estimation.

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 1 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.[18]

The large economic and statistical significance of the estimated coefficient suggests that the instrument indeed fulfills the relevancy condition: When a banker who holds 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

[18]Let us 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 10% of cases, the departure of a lead banker is not associated with a drop in personal relationship strength to 1.

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.[19] 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 3 report the results from the second-stage estimation. The estimated impact of *Personal count* on interest rates is economically large (-55 bp) and statistically significant at the 1% level.[20] Column 3 adds controls for year and firm fixed effects. The estimated coefficient shrinks to -16 bp, but retains its 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 bp and is highly statistically significant. Finally, Column 5 combines all firm- and loan-level control variables. The resultant coefficient estimate is -12 bp and remains statistically significant at the 10% level. The estimated impact is economically sizable: A one standard deviation increase in personal relationship strength (0.87 interactions) is associated with a reduction in interest rates of about 10.5 bp, 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.

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 bp on interest rates when banks and borrowers share a board-level social connection.[21]

[19]The large F-statistic is partly driven by the inclusion of firm fixed effects. Appendix B.1 presents a robustness exercise with industry fixed effects rather than firm fixed effects. The corresponding F-statistic drops by one-third to about 80, but is still very high.

[20]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 (2018). All results are robust to clustering errors on the borrower level or simply using robust standard errors without clustering.

[21]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

Of course, an unobservable shared characteristic of a banker's complete portfolio might still cause bankers to lose their jobs. I therefore conduct additional analyses in Appendix B.1. 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 absorbs 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.[22]

I now show OLS regressions of Equation 1 in Table 4 to demonstrate the magnitude of the biases that result from the endogenous nature of personal relationships.

[Table 4 about here]

The estimated impact of 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), while statistically significant at the 5% level, 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 leads to higher interest rates for loans with stronger relationships, and biases the coefficient estimate on personal

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.

[22]In a final, unreported robustness test, I repeat the analysis using only cases of banker turnover in which the banker subsequently signs a loan for a different bank. That test addresses concerns that bankers are terminated for offering cheap loans, which leads to a subsequent increase in interest rates. If bankers who are terminated for issuing loans too cheaply have a lower likelihood of subsequently being hired, this test alleviates concerns that low rates and banker turnover are driven by unobservable banker characteristics. Results retain both their statistical and economic significance. A potential alternative explanation channel might be that relationship bankers have a *higher* hurdle to issue a loan to their clients to avoid tarnishing their reputation. They might therefore demand higher borrower quality for the same loan terms, which would explain the observed lower rates for relationship loans. That concern would, however, still require the relationship banker to possess private information, since interest rates are lower for relationship loans after controlling for a wide range of observable covariates. However, this alternative explanation for lower rates would not invalidate the finding that bankers possess soft information even for the largest, most transparent borrowers. It would simply connect the lower rates to this information in a different mechanism.

relationship strength downward compared to the IV results.[23]

3.2 Efficiency or nepotism?

The lower interest rate associated with loans in which bankers have substantial prior experience with borrowers could be due either to nepotism or to superior information. In the nepotism hypothesis, commercial bankers might extend favorable loans to managers they have interacted with in exchange for personal monetary or social favors. These loans granted by bankers with many prior interactions with borrowers should be associated with worse loan performance afterward. However, in the superior information hypothesis, bankers learn valuable information about borrowers over the course of their relationship, and 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 1 if the borrower of a given loan files for bankruptcy at any time during the maturity of the loan.[24] Table 5 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 4.[25]

[Table 5 about here]

Loans granted by bankers that have substantial prior experience with the borrower are associated with a significantly lower likelihood of default. The point estimates of *Personal*

[23]The firm fixed effects only control for time-invariant unobservable borrower quality. If borrowers are more likely to return to their relationship borrower after an unobservable adverse shock to their financial health, the panel regressions would underestimate how much personal relationships reduce interest rates.

[24]If a borrower has more than one outstanding loan at the time of bankruptcy, I assign the bankruptcy event only to the last loan. I would like to point out that the lower bankruptcy rate is therefore not the result of a short-term improvement in firm performance: Since bankruptcy is monitored during the entire maturity of the loan, the lower bankruptcy rate is inside the time horizon that matters for the lending bank. 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).

[25]Note that due to the rare occurrence of bankruptcies and renegotiations, there are too few observations to estimate an instrumental variable specification analogous to the specification in Section 3. However, the results from table 4 show that personal relationship strength seems to be higher for unobservable weaker borrowers. This bias should go against finding that stronger relationships are associated with fewer bankruptcies in the OLS setting.

count range from -0.67% to -0.75% across the various specifications, and they 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 intensity. The estimated reduction in bankruptcy likelihood is economically large: Using a back-of-the-envelope calculation, a one standard deviation increase in personal relationship strength (0.86 interactions) translates to a 0.65% reduction in the likelihood of bankruptcy. Compared to the unconditional bankruptcy rate of 3.16%, 0.65% constitutes a reduction of roughly 20%.[26]

The estimated reduction in bankruptcies is not just economically significant; it can also explain why banks are willing to grant loans to relationship borrowers at lower interest rates. Khieu, Mullineaux, and Yi (2012) report average recovery rates of bank loans ranging from 60% to 80%. A one standard deviation increase in personal relationship strength implies annual savings of about 7 bp, based on a back-of-the-envelope calculation using an estimated 75 bp reduction in the likelihood of bankruptcy (as in Column 4 of Table 5), a recovery rate of 70%, and a loan maturity of 3 years. The expected savings from lower bankruptcy rates can therefore make up for around two thirds of the reduced interest rates, compared to the lower interest rates (11 bp) as a result of this increase in personal relationship strength estimated in Section 3.[27]

The results from Table 5 indicate that loans granted by commercial bankers who have many prior interactions with borrowers are associated with a significantly lower likelihood of bankruptcy. However, a lower likelihood of bankruptcy alone is not necessarily the result of superior efficiency: Haselmann et al. (2018) find that when CEOs of borrowers and banks share a social relationship, banks are more likely to extend loans to borrowers instead of

[26]Note that I cannot directly observe recovery rates for the loans in my sample. If recovery rates were significantly higher for relationship loans, banks would recoup less of the lower interest rate from the lower bankruptcy rate.

[27]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 their 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).)

pushing them into bankruptcy, particularly if banks are state owned. 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.[28] 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 6 are the change in the loan's dollar amount (*change amount*, columns 1 and 2) and the change in its maturity (*change maturity*, columns 3 and 4). The explanatory variables include personal and institutional relationship strength, as well as an indicator that takes the value of 1 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, then stronger personal relationships should be associated with an *increase* in loan amount and *longer* maturity upon renegotiation, an effect that should be *stronger* for firms that are closer to bankruptcy, i.e., they have a worse credit rating.

[Table 6 about here]

Column 1 of Table 6 shows that personal relationship strength is not associated with an increased loan volume in renegotiations: The estimated coefficient of *Personal count* on *change amount* is actually negative at $-3.48 million, albeit statistically insignificant. Column 2 tests whether the effect of *Personal count* is different for firms that are junk rated. While the interaction *Personal count* × *junk* is 0.18 and statistically insignificant, the estimated coefficient on *Personal count* by itself doubles in this specification to -8.64 and becomes statistically significant at the 1% level. Since the null hypothesis is that stronger

[28] I classify all events as renegotiations when they are neither originations nor a maturing of credit agreements, and I match them to my data based on DealScan package ID. Due to the cost of data collection, Roberts (2015) only obtained data on 114 randomly chosen borrowers.

personal relationships should be associated with *higher* loan amounts, the negative coefficient estimates are evidence against a nepotism effect.

Columns 3 and 4 repeat the analysis with *change 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. The coefficient on the interaction *Personal count* $\times$ *junk*, however, is large at -4 months and statistically significant at the 10% level.[29] As before, this result is the exact opposite of the null hypothesis, which 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 nepotism hypothesis would predict the opposite. The less favorable conditions upon renegotiation cannot result from the the slightly more favorable conditions upon origination for three reasons. First, these regressions control for the initial interest rate on the loan. Second, since stronger personal relationships impact the initial interest rate but not size and maturity of loans where a rolling over of loans should manifest itself during renegotiations. Third, more lenient loan terms at origination should lead to a lower probability of distress or renegotiation ex ante but not lead to tougher renegotiation terms once the firm is in distress.

Why are these results different from those in Haselmann et al. (2018)? The key difference is that the connections in my sample are based on professional relationships, whereas those in Haselmann et al. (2018) are social in nature. In fact, Haselmann et al. (2018) find that stronger work-related incentives of bankers reduce the nepotism effect of social connections. My results therefore show that when personal relationships are predominantly formed in the work context, they have an aggregate positive effect of lending efficiency. One caveat is that, as in the case of bankruptcies, the small number of observations in this analysis does not allow me to use firm fixed effects or the instrumental variable approach from the main analysis.

[29]In untabulated results, I investigate whether personal relationships impact pricing modifications. I find no connection between personal relationship strength and interest rate adjustments.

4 Bankers and bank-borrower matching in the loan market

4.1 Do borrowers follow their bankers to new banks?

The previous sections show that commercial bankers with strong borrower-specific relationships allow firms cheaper access to loans. Bankers should therefore play an important role in matching banks and borrowers. I begin by showing that when bankers switch employing banks, they subsequently take their clients with them, which underscores that individual bankers have unique human capital linking them to borrowers.

I construct a panel that features one observation for each potential borrower-bank pair in each year and estimate the following specification:

$$Initiation_{i,j,t} = \phi_j + \rho_t + \beta Personal\ Relationship\ Acquired_{i,j,t} + X_{i,t} + \epsilon_{i,j,t}, \qquad (3)$$

The dependent variable $Initiation_{i,j,t}$, is an indicator variable that takes the value of 1 in year t if firm i originated a loan with bank j as one of the lead underwriters in year t but did not do so in the 5 preceding years.[30] 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 in addition to the same firm-level control variables $X_{i,t}$ used in the main analysis.

The main explanatory variable in this analysis is $Personal\ relationship\ acquired_{i,j,t}$, an indicator variable that identifies firm-bank pairs in which a personal relationship was recently acquired by the bank.[31]

[30] I choose the 5-year cutoff because it is just above the average loan maturity in my sample. In unreported results, I find that all estimated coefficients are robust to variations in this cutoff. I limit the sample to the 50 lenders with the highest number of loans in the sample to avoid outliers driving the results. In unreported analysis I verify that results are robust to using all possible bank-borrower pairs.

[31] To construct this variable, I first check whether bank j acted as lead arranger for one of firm $i's$ loans during the preceding five 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

If commercial bankers take their clients with them after switching employers, $Personal\ Relationship\ Acquired_{i,j,t}$ should have a positive impact on $Initiation_{i,j,t}$, the initiation of a new banking relationship.

Table 7 presents the results of estimating different specifications of Equation 3.

[Table 7 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 0.3% in Column 1, albeit small, is economically significant when compared to 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 0.3% therefore represents a 200% increase in the likelihood of initiating a relationship after having acquired a personal relationship. Columns 2 and 3 add industry and year fixed effects as well as the set of control variables from the main specification. The estimated coefficient of $Personal\ relationship\ acquired$ stays positive and significant throughout.

Columns 4 to 6 report the marginal coefficients of the same specifications using logit regressions rather than linear probability models. The coefficient for *Personal relationship acquired* remains positive and statistically significant in all specifications. Marginal effects are about 0.1% across the three specifications. The implied economic magnitude is nonetheless large and implies that borrowers are more than twice as likely to initiate a new lending relationship with a bank that hired its relationship banker than with other banks in each subsequent month.

The results presented in Table 7 provide evidence that commercial bankers do indeed take their clients with them when switching banks, and this is evidence that personal lending relationships are of value to both banks and borrowers. This finding adds to that of Karolyi

whether any banker who 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. The variable $Personal\ relationship\ acquired_{i,j,t}$ takes the value of 1 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.

(2018), who finds that corporate executives who are appointed at a new firm tend to continue borrowing from the same banks as before. While Karolyi (2018) 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.

4.2 Personal relationships during the crisis: Loan availability and lock-in effects

This section investigates the role of personal relationships between bankers and borrowers during the financial crisis. A strong relationship between a borrower and a bank can act as an insurance mechanism against firm-specific shocks (Bolton et al., 2016). On the other hand, having a relationship with only a single banker can expose borrowers to borrower-specific shocks. Chodorow-Reich (2014) finds that when a firm's lender was affected by the financial crisis, that bank-specific shock propagated to the firm level and had real effects on employment. The starting point in the analysis of Chodorow-Reich (2014) is that banking relationships are sticky even for large borrowers.[32] But why did these large, transparent borrowers not simply move to another bank when their relationship bank was affected? Given that the previous sections have shown that bankers have valuable soft information even for these borrowers, I conjecture that strong personal lending relationships might have locked in borrowers with their banks and led to the transmission of the shock from the financial to the real sector.

I empirically investigate this question by running LPM regressions of whether a loan obtained during the financial crisis was with a new or an old lead arranger.

$$\begin{aligned} Switched\ leadbank_j = \alpha + Crisis_t + \beta_1 Personal\ count\ last\ loan_{i,t} + \\ \beta_2 Personal\ count\ last\ loan_{i,t} \times Crisis_t + \gamma' X_{i,j,t} + \epsilon_j, \end{aligned} \tag{4}$$

[32] The average loan size in his sample is \$300 million, comparable to that in my sample.

The dependent variable is $Switched\ leadbank_j$, a loan-level indicator that takes the value 1 if firm i obtained a loan from a lead arranger bank with which it had no lead arranger relationship in the past 5 years. The main explanatory variable is $Personal\ count\ last\ loan_{i,t}$ and its interaction with $Financial\ crisis$, an indicator that takes the value of 1 during the financial crisis from September 2008 to June 2009. Parameter α denotes industry (columns 1 to 3) and firm (columns 4 to 6) fixed effects, respectively. Table 8 presents the results from estimating various specifications of Equation 4.

[Table 8 about here]

Columns 1 to 3 show results using industry fixed effects as well as the additional firm- and loan-level controls from the main analysis. The estimated coefficient on $Personal\ count\ last\ loan$ is economically small and statistically insignificant, meaning that firms with stronger personal relationships are neither more nor less likely to switch lead arrangers. The coefficient on $Financial\ crisis$ is around 9 in columns 1 and 2, and it is statistically significant. This suggests that borrowers who obtained a loan during the crisis were more likely to have received that loan from a new relationship bank. Importantly, the coefficient on the interaction term $Personal\ count\ last\ loan \times Financial\ crisis$ is negative in all three specifications and statistically significant in columns 2 and 3. Firms that had stronger personal relationships with their last bank were therefore less likely to have switched banks, conditional on having received a loan.

Columns 4 to 6 repeat the analysis using firm fixed effects. While the coefficients on the uninteracted measures of prior relationship strength and the crisis are economically and statistically insignificant, the interaction term between the two remains negative and statistically significant. Due to the collapse in the loan market during the crisis (Ivashina and Scharfstein, 2010), there are only 90 loans "treated", which means we should be wary of placing too much emphasis on the coefficient estimates.[33] The results are nonetheless

[33]The firm fixed effect specifications in particular draw inferences only from borrowers who received loans both during and outside the crisis and from variation in their personal relationship strength across those

indicative of the fact that borrowers with strong personal relationships were less likely to switch lenders during the financial crisis.

The last result shows that, *conditional on having received a loan*, borrowers with strong personal relationships were less likely to have switched lenders during the crisis. It is unclear, however, whether this result translates into stronger credit constraints or whether these borrowers relationship banks had simply taken care of all their financing needs. To test this conjecture, I run panel linear probability models in the form of Equation 5:

$$Loan\ received_{i,t} = \alpha_i + \phi_t + \theta_m + \beta Personal\ count\ last\ loan_{i,t} + \gamma' X_{i,t} + \epsilon_{i,t}, \qquad (5)$$

The model in Equation 5 is on the firm-month level, with the outcome variable *Loan received* being an indicator of whether a firm received a loan in a specific month. The main explanatory variable is $Personal\ count\ last\ loan_{i,j,t}$, the personal relationship strength associated with the last loan obtained by firm i. Bank fixed effects θ_m are defined as the last lead arranger.

[Table 9 about here]

The results in Column 1 of Table 9 show that borrowers with stronger personal relationships generally had a higher chance of receiving a loan. The estimated coefficient on the personal relationship strength of the last loan is 0.39 and is statistically significant at the 5% level. This results remains robust to the inclusion of the various firm-level controls from the main specification as well as bank fixed effects in Column 2. Column 3 adds an indicator of the financial crisis as well as its interaction with personal relationship strength.

two periods. This means that those specifications draw inferences from fewer than 20 observations. For the same reason, I cannot simultaneously include both personal and institutional prior count as well as their interactions. In untabulated results, I re-estimate equations 4 with institutional rather than personal count. While the coefficient estimates go in the same direction, they are economically much smaller and statistically insignificant, which I interpret as evidence that personal relationships are more important than institutional relationships in making banking relationships sticky.

Unsurprisingly, given the global slowdown in lending following the collapse of Lehman Brothers, the coefficient estimate on *Financial crisis* is -1.7 and is highly statistically significant. Importantly, this effect is particularly pronounced for borrowers who had strong personal relationships with their prior banks. This result shows that borrowers with strong personal relationships indeed received fewer loans during the crisis.

If the lower propensity to obtain loans was indeed due to financial constraints rather than choice, the lock-in effect from stronger personal relationships should be stronger when relationship banks were more affected by the crisis. To test this conjecture, I use data on the reduction in lending of the largest U.S. lenders used in Chodorow-Reich (2014), and I construct the indicator *Bank affected* based on whether a bank reduced its lending more than the median lender during the crisis. The triple interaction between *Personal count last loan*, *Bank affected*, and *Financialcrisis* then captures the differential impact on loan availability during the financial crisis between borrowers who had strong personal relationships with the affected banks versus borrowers who had weak prior relationships with affected banks. The negative and statistically significant coefficient of -0.39 suggests that it was specifically strong relationship borrowers who were unable to switch to a different bank and hence suffered more financial constraints.[34]

The results in this section show that borrowers who had stronger personal relationships with their bankers had higher loan availability during normal times. However, during the financial crisis, firms with stronger personal relationships were less likely to receive funding, in particular when their relationship bank was more affected. Consistent with the idea that their personal relationships tied those borrowers to banks and exposed them to a bank-specific shock, those banks with strong personal relationships that did receive a loan during the crisis were significantly less likely to have done so by switching to another lead bank.

The result that personal relationships lock in borrowers and expose them to bank-specific

[34]Unlike in the analysis on bank switching in table 8, I can use these triple interactions in this context, since the sample is larger because it includes not just borrowers who obtained a loan but also borrowers who did not obtain a loan.

shocks is an important microfoundation for the findings in Chodorow-Reich (2014). They further highlight the importance of the nature of shocks on their interaction with relationship lending: While relationships can act as an insurance mechanism against firm-specific shocks (Bolton et al., 2016), they expose the firm to bank-specific shocks. Finally, these results highlight how the different economic channels that underpin different types of personal relationships can impact results differentially: Karolyi (2018) finds that personal relationships between borrowers, executives, and banks allowed firms to better commit to contracts and receive superior access to finance during the crisis. The relationships between bankers and borrowers in my paper operate through superior information and monitoring, and these relationships did not provide better access to funding.

5 Time-invariant impact of bankers on loans

In this section, I investigate the time-invariant impact of bankers on loan terms, or "banker fixed effects." I employ the methodology developed in Abowd, Kramarz, and Margolis (1999) (the AKM method), which is similar to a standard fixed-effects estimation as used in Bertrand and Schoar (2003) but it allows broader econometric inference. Appendix B.2.1 provides a detailed description of this method, and all results are statistically and economically similar using standard fixed-effects estimates.

Formally, the full model is

$$y_j = \alpha_i + \phi_t + \theta_m + \kappa_q + \delta' X_{i,t} + \gamma' X_j + \epsilon_j, \tag{6}$$

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. The variable $X_{i,t}$ is a vector of time-varying firm control variables, and X_j is a vector of loan control variables. 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 that are associated with at least two different loans.

Table 10 presents the results from estimating the resulting high-dimensional fixed effects model in Equation 6.

[Table 10 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., 2018; Liu, Mao, and Tian, 2016). The first dimension is the degree to which the inclusion of institution (bank) and individual (banker) fixed effects increases the model fit (R^2). The second dimension is whether an F-test can reject the null hypothesis of joint statistical significance of all individual fixed effects. The third dimension is the relative contribution of individual fixed effects to the model's explanatory power.

Panel A of Table 10 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 (a) the loan price measured as the all-in spread drawn over LIBOR, (b) loan size measured as the logarithm of the loan amount in U.S. Dollars, (c) loan maturity, (d) the fraction of the loan secured with collateral, and (e) the number of covenants associated with the loan.

Even-numbered columns present estimates without banker and bank fixed effects, and odd-numbered columns present estimates that include those fixed effects.[35] The inclusion of high-dimensional banker and bank fixed effects significantly increases the model's adjusted

[35] There are 614 individual bankers classified as movers; movers are associated with loans featuring at least two lead banks. Some bankers are associated only with loans from the same lead bank. The number of movers is high, since bankers can be associated with banks other than their employing bank on a loan if another bank has an institutional relationship with the borrower that is stronger than the borrowers 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. Morgan Chase in 2004. This banker would be classified as a "mover" for the purpose of estimating the fixed effects model, since we can econometrically distinguish between her FE and her bank's FE. Note that the classification for movers in the relationship lending analysis is economic, and it requires that a banker actually left her bank. In addition, *Banker left*, the instrument in the main analysis, takes the value of 1 for the next loan of the old relationship customer. If the old relationship customer does not obtain another loan, a banker switching banks does not cause the instrument to vary. If a borrower is cut off from obtaining a new loan due to a severed relationship, this setup biases the analysis against finding an effect on interest rates that results from bankers leaving.

R^2 for the loan pricing regressions 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 (27%), maturity (49.7%) the fraction secured (66%), and the inclusion of financial covenants (155%).

The absolute increase in explanatory power is relatively even across specifications, at around 20% to 40%. Therefore, adding banker and bank fixed effects, in addition to standard control variables, greatly increases the explanatory power of models of bank loan characteristics.

Panel B of Table 10 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 2), banker fixed effects alone (Line 3), and bank fixed effects alone (Line 4). 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 these tests fails to reject the null hypothesis of joint insignificance of any set of individual or joint banker and bank fixed effects at the 1% level.

The final three lines of Panel B report the relative contribution of banker and bank fixed effects to the model R^2, respectively.[36] Banker fixed effects explain a sizable part of the variation in loan characteristics. Banker fixed effects account for about 20% to 25% of the variation in the variables (i.e., *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 not straightforward to evaluate whether the estimated contributions are potentially

[36] 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.

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). These simulations randomly assign the existing bankers and banks across all sample loans and then estimate the models presented in Table 10. 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 percentiles of the resultant values for the three variables. Table 11 reports the simulated values and compares them to the sample estimates.

[Table 11 about here]

Table 11 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.

These 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, the 95th percentile for *maturity*, and the 90th percentile for *secured*. The relative contributions of banker fixed effects for loan size and financial covenants is lower in the actual sample than in the 90th percentile of the simulations. Hence, banker fixed effects for these two outcome variables are jointly statistically significant, and they significantly contribute to model fit jointly with bank fixed effects, but the relative explanatory power of the sample values may slightly exceed their actual explanatory power.[37]

The results from Table 11 confirm that banker fixed effects have significant explanatory power for a number of loan characteristics, and they confirm that the results from Table 10 are not due to randomness and are not the mechanical effect of including a large number of fixed effects. Of course, these results cannot fully disentangle the effect of unobservable matching between bankers and borrowers from that of intrinsic banker preference. However, in combination with the results on relationship formation, these results broadly underscore the relevance of bankers in the lending process.[38]

6 Conclusion

I construct a new dataset that links commercial bankers and borrowers in order to investigate the role of individual bankers in the market for large syndicated loans. I find that bankers impact commercial bank lending through personal relationships with borrowers over time. 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 an annual savings of $275,000 for the median loan. These loans are associated with fewer borrower bankruptcies, which suggests that the reduced credit spread is due to bankers' ability to

[37] In unreported results, I repeat the simulations, but instead of randomizing both bankers and banks, I only randomly assign bankers and keep banks as they are 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.

[38] One specific concern I can rule out is that banker fixed effects could proxy for their employers' industry 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. Therefore, in unreported analyses, I 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.

increase the efficiency of the lending process. Those relationship loans are subsequently less likely to be modified in the borrower's favor, evidence that relationships, on the aggregate, do not lead to lower interest rates due to nepotism.

Having an experienced relationship banker benefits borrowers; as a result, bankers who switch to another bank take their clients with them. The dark side of this matchmaking is that relationship borrowers are more exposed to bank-specific shocks, such as the financial crisis. Borrowers with strong personal relationships were less likely to have switched lenders during the crisis, conditional on having received a loan. This result is not caused by the borrowers receiving loans from their relationship banks: Borrowers with stronger relationships were generally more likely to receive loans, but less likely to have access to credit during the crisis. The fact that this credit rationing was most pronounced for borrowers from more affected banks indicates a lock-in effect of personal relationships for borrowers.

In the final set of results, I show that commercial bankers exhibit a significant, time-invariant impact on loan terms, such as the interest rate or loan size. Banker fixed effects can explain about 1.5 times as much of the variation in those variables as bank fixed effects.

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, public information. Nonetheless, individual bankers play an important role in this market.

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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.

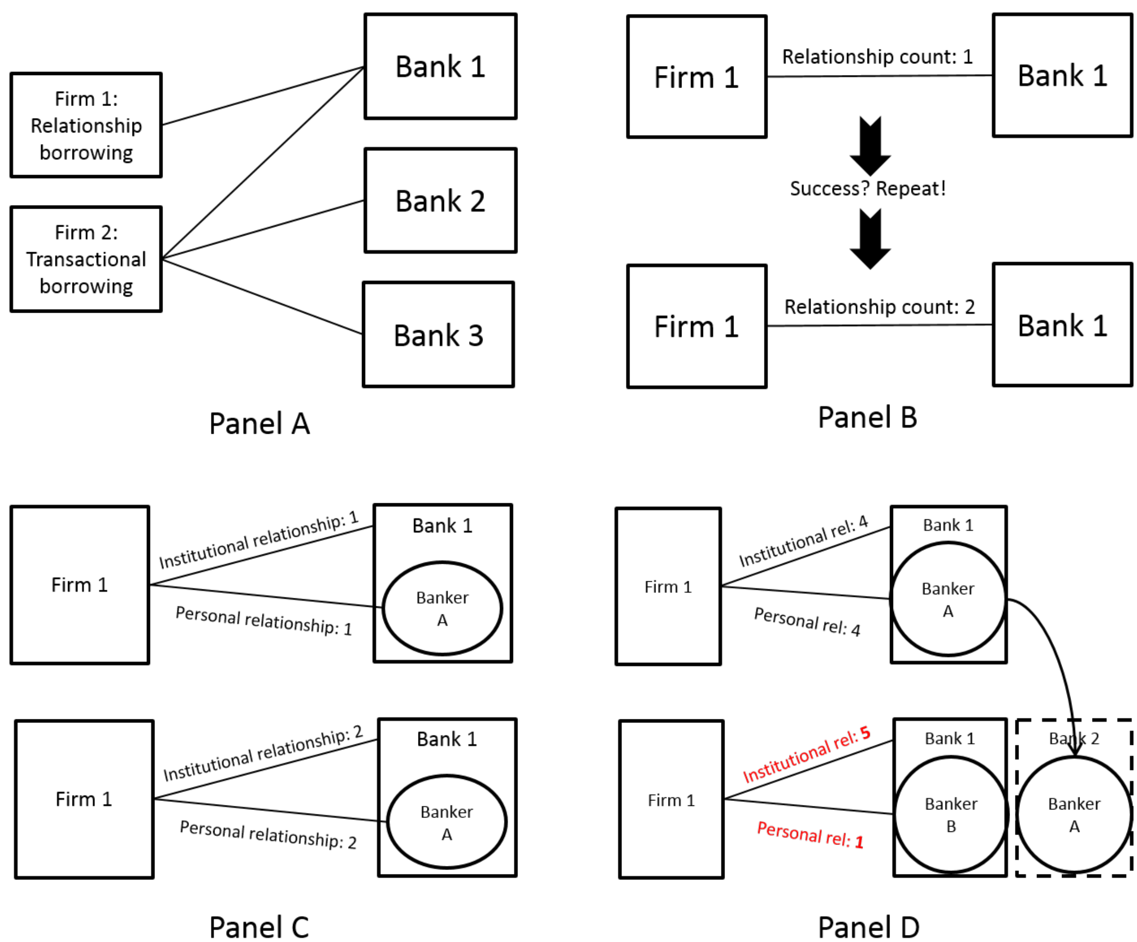

Table 1: Variable Descriptions

Variable name	Description
Firm characteristics	
Assets	Total assets
Bank affected	Indicator taking the value of one for lead banks that reduced their lending by more than the median bank during the financ
Financial crisis	Indicator taking the value of one during the financial crisis (September 2008 to June 2009)
Leverage	Total liabilities, divided by total assets
Market to book	Shares outstanding times stock price, divided by book value of assets
Personal relationship acquired	Indicator of whether bank hired banker with personal relationship with borrower in the last two years
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 2.3. 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: 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 banker 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. Column 1 presetns the first stage results corresponding to the most complete model specification in Column 5. For each of columns 2 to 5 a separate 2SLS estimation is performed. The first stage results for columns 2 to 4 are omitted for the sake of brevity. *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 4: 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 5: 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 6: 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.

	Change amount		Change 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 7: 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 2000 to 2012, relationships are measured starting 1999 to allow for burn in. 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	0.030***	0.011***	0.010***	0.012***	0.017***	0.005***
	(0.004)	(0.004)	(0.002)	(0.001)	(0.001)	(0.001)
N	1445100	1445100	1445100	1287650	1445100	1429295
R^2	0.002	0.019	0.022			
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls	No	Yes	Yes	No	Yes	Yes
Year FE	No	Yes	No	No	Yes	No
Bank × *Year FE*	No	No	Yes	No	No	Yes

Table 8: Personal Relationships and Lender Switching During the Financial Crisis

This table presents results of linear probability models where the dependent variable is or *Switched leadbank*, an indicator variable whether a borrower chose a new lead lender for a given loan. The main explanatory variables are *Personal count last loan*, a borrowers personal relationship strength with her last loan, *Financial crisis*, an indicator equal to one in the months between September 2008 and June 2009, and their interaction. 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 industry and year. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Switched leadbank*100					
	Industry FE			Firm FE		
	(1)	(2)	(3)	(4)	(5)	(6)
Personal count last loan	-0.605	1.158*	0.815	1.924	1.352	0.847
	(0.728)	(0.682)	(0.828)	(1.322)	(1.033)	(0.820)
Financial crisis	8.977*	9.470***	1.838	11.404	4.558	-5.081
	(4.753)	(3.476)	(5.547)	(11.391)	(10.476)	(9.240)
Personal count last loan × *Financial crisis*	-3.287	-5.348***	-4.000*	-6.513**	-5.180*	-2.028
	(2.542)	(1.956)	(2.206)	(3.053)	(2.867)	(2.224)
N	4430	4430	4430	3473	3473	3473
R^2	0.001	0.058	0.426	0.002	0.017	0.436
Industry FE	Yes	Yes	Yes	No	No	No
Firm FE	No	No	No	Yes	Yes	Yes
Firm controls	No	Yes	Yes	No	Yes	Yes
Loan controls	No	No	Yes	No	No	Yes

Table 9: Personal Relationships Credit Availability

This table presents results of linear probability models regressions where the dependent variable is either *Loan received*, an indicator equal to one if a borrower obtained a loan in a given month. The main explanatory variables are *Personal count last loan*, a borrowers personal relationship strength with her last loan, *Financial crisis*, an indicator equal to one in the months between September 2008 and June 2009, and their interaction. 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 industry and year. *, **, and *** mark statistical significance at the 10%, 5%, and 1% level, respectively.

	Loan received*100			
	(1)	(2)	(3)	(4)
Personal count last loan	0.388**	0.343**	0.250*	0.096
	(0.169)	(0.154)	(0.132)	(0.252)
Financial crisis			-1.724***	-1.418*
			(0.393)	(0.838)
Bank affected				-0.301
				(0.758)
Personal count last loan × *Financial crisis*			-0.483***	-0.232
			(0.102)	(0.300)
Personal count last loan × *Bank affected*				0.367
				(0.259)
Bank affected × *Financial crisis*				-0.500
				(0.414)
Personal count last loan × *Bank affected* × *Financial crisis*				-0.390**
				(0.161)
N	44662	44662	44662	44662
R^2	0.007	0.011	0.004	0.003
Industry FE	Yes	Yes	Yes	Yes
Bank FE	No	Yes	Yes	No
Firm controls	No	Yes	Yes	Yes

Table 10: 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 cirtical F-value to reject the null of no joint explanatory power for the three tests range between 1.15 and 1.20. Since all specifications are statistically significant at the 1% level I refrain from adding stars to denote significance for ease of readability. 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 11: 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%

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

A Data

A.1 Additional details data collection

About one-third of contracts do not contain signatures in the original documents. 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. In personal discussions with bankers, I was told that these missing signatures were likely due to problems in the conversion of contracts to electronic formats.

While the main analysis focuses on lead bankers, the search also extracts the names of bankers associated with non-lead 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, their location in the contract, and the name of the bank. The most frequent reason why no lead banker could be extracted is that the algorithm failed to capture any signature in the document (16%). These loans contain no information on bankers and are dropped from the sample. In 4% 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.

A.2 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. All information used in this section, as well as in the rest of the paper, is public; however, I will anonymize information that could identify Officer 171. According to his profile on an online 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 the 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 nine 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 while 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 is promoted to Director in 2006.

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

A.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 who sets the loan terms and holds the relationship with the borrower. I spoke with several current and former employees of commercial lending divisions from different banks, both in the U.S. and in 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. They argued that, at the very least, the signatory will have had some exposure to the deal and therefore knows what she is signing. At the same time, they all acknowledged 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 must 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.

A.4 Alternative measures of relationship strength

There are two potential measures of personal relationship strength: 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*, which 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.[39] Since loan terms generally are negotiated between the lead bank and the borrower before syndication, I consider interactions between bankers and borrowers only if the banker acted for one of the syndicate's lead banks. For cases in

[39]The following example illustrates this 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, therefore, a simple sequence from 1 to 5 without a break after the change of employers.

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 almost 30% of the loan amount, on average, 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 a specific borrower.[40]

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 consists of 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 2.3 provides detailed statistics on each of these variables.

B Additional results

B.1 Additional results on the effect of relationships on interest rates

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 A1 presents the results from estimating the instrumental variable regression from equations 1 and 2 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 3, with the first column presenting the results from the 2-steps least-squares estimation's first stage. As in the case of *Personal count*, *Personal duration* drops significantly 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 that could

[40] A second question is whether to construct these measures of relationship strength using all available contracts from the SEC archives or using only those contracts I can link to DealScan. DealScan focuses on novel loan originations, and many amendments are not captured. Since loan amendments involve interactions between bankers and borrowers, they deepen the relationship, which is why I measure relationship count based on all available contracts. All results stay statistically and economically similar, with count and duration measured exclusively within the DealScan matched sample.

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 significant, 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 bp for each additional year after the beginning of the relationship. The estimates are robust to a wide variety of controls and are economically significant: A one standard deviation increase in *Personal duration* is associated with a 12.5 bp 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 bp 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 A2, therefore, presents the results from estimating Equations 1 and 2 using industry fixed effects rather than firm fixed effects. Industry fixed effects are assigned based on the 2-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 3, but still very high at 70.48. The following second-stage estimates are comparable to those that feature firm fixed effects in both economic and statistical significance.[41]

The main results use all available contracts from the SEC to measure personal relationship counts, not just those contracts that can be matched to DealScan. The rationale behind this decision is that many renegotiations and amendments still strengthen personal relationships through the interaction between the banker and the borrower. However, one might be concerned that it biases the findings. Table A3 therefore repeats the main analysis using only contracts matched to DealScan to define personal relationship strength. All results are statistically and economically comparable to those from the main section.

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 A4 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 I 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, columns 3 and 4 burn in four years of data, and the last two columns burn in six years of data. The instrumental variable

[41] Note that the sample is slightly larger in the industry fixed effects regression, as it now includes singleton observations; that is, firms that 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.

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 bp, 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 A4 therefore confirm that left censoring does not drive the attenuating impact of personal relationships on interest rates.

Table A5 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 A5 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 A5 replace bank fixed effects 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 A5, 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 bp, and it is statistically significant at the 5% level. The first stage remains statistically and economically significant. The results in Table A5 reconfirm the interpretation that personal relationship strength indeed causally reduces interest rates on loans.

Table A6 tests whether there are cross-sectional differences in the impact of personal relationships on loan rates. If stronger personal relationships allow bankers to better understand borrowers, the impact of relationships should be stronger for more opaque firms. I therefore split the sample along two dimensions of opacity and estimate the same instrumental variable regressions as in Table 3. Columns 1 and 2 compare the effect of relationship strength between firms with and without a credit rating. The effect of stronger personal relationships is most pronounced when firms lack a rating (-37 bp, statistically significant at the 5% level) and this is economically and statistically insignificant for those with a rating. Columns 3 and 4 split the sample into those firms with above- and below-median intangibles. The coefficient estimate on high-intangibles firms is -24.7. While it is barely statistically insignificant, it is about 80% larger in magnitude than that for firms with above-median intangibles (Column 4). While the reduction in sample size weakens the statistical inference, these results indicate that information is indeed the economic channel that drives the connection between personal relationships and interest rates.

Finally, in unreported results, I investigate how the dispersion of interest rates changes over the course of relationships, similar to the exercise in Haselmann et al. (2018) on loan amounts. To motivate this test, consider the following simplified example: Assume that the interest rate for a borrower based on objective, hard information was r. A non-relationship lender can offer $r + \epsilon$, where ϵ is a mean zero error term. A relationship bank can obtain additional soft information about the borrower, s, which results in an offered interest rate of $r + s + \epsilon$. The soft information can lead the relationship bank to conclude that r is either too high or too low. Accordingly, the soft information adjustment s will be either positive (if the soft information reveals the borrower to be of low quality) or negative (if the soft information reveals the borrower to be of high quality). As a result, the *offered* interest rates for relationship loans should exhibit a larger degree of dispersion.

The data does, however, only allow me to observe the *realized* interest rates. But a borrower that is offered a high rate due to adverse soft information is likely going to seek alternative funding at rate r by pooling with transactional borrowers. Effectively, self selection by borrowers "cuts off" the lower tail of the distribution of interest rates in relationship loans. Therefore, with increasing relationship intensity, both interest rates and unexplained dispersion of rates should go down. In untabulated results I confirm this conjecture: I find that loans with higher personal relationship strength are associated with less dispersed interest rates.

B.2 Additional results and methodology for banker fixed effects estimation

B.2.1 AKM methodology

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 of 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 one 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 reduces the available sample significantly.

A number of authors in the finance literature have recently employed the methodology of Abowd et al. (1999) (the AKM method), which is a refined version of the approach of Bertrand and Schoar (2003). This is the methodology I utilize for the main analysis in this section. 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 et al., 2016), or mergers and acquisitions (Chemmanur et al., 2018). In untabulated results, I repeat my analysis using the switchers-only approach as in Bertrand and Schoar (2003), and I find the results to be economically and statistically very similar to those using the AKM approach.

B.2.2 Additional results on the time-invariant effect of bankers on loans

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 of 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 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 A7 presents the correlations of banker fixed effects for the various loan outcomes analyzed in Table 10.

Table A7 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 they make less use of financial covenants. And, bankers who tend to issue larger loans are indeed more likely to insist on financial covenants.

One potential concern is that fixed effects might be driven by serial correlation of dependent variables inside a specific bank, as discussed by (Fee et al., 2013). Table A8 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 A8 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.[42] In unreported results, 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 alternative specification. These results provide further evidence that the observed banker fixed effects are indeed meaningful.

In Table A9, 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 (a) whether they have a credit rating, (b) whether their debt is rated as investment grade, and (c) 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 (a) for small borrowers than for large borrowers, (b) for those firms without a credit rating compared to those with a credit rating, and (c) for those firms whose debt is not rated as investment grade than for those whose debt is investment grade. These results suggest that bankers play a larger role when there is less publicly available information.

The fixed effects regressions in Table 10 might suffer from the serial correlation of errors

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

in the dependent variables, similar to the analysis in Bertrand and Schoar (2003) and Fee et al. (2013).[43] I therefore follow those two sets of authors and repeat the analysis using collapsed banker-bank residuals. A first step creates those residuals through regressions of various loan characteristics on the set of explanatory control variables, including bank fixed effects, but not banker fixed effects. I then collapse the resulting residuals on a banker-bank level, creating an average residual for each banker and for each bank she worked for. I then estimate regressions of these average banker-bank residuals on banker fixed effects. Collapsing the residuals on the banker-bank level reduces concerns of serial correlations of residuals on the bank level. Table A10 presents the F-statistics of the combined explanatory power of all banker fixed effects. The joint explanatory power of banker fixed effects is statistically significant at the 1% level for regressions of the loan spread, and at the 5% level for regressions of the fraction of the loan that is secured. Those were also the two dependent variables for which inference was the most robust in the simulation exercise in Section 5. It is not clear whether the loss of statistical significance for the other dependent variables is due to the presence of bias in the original regressions or due to the reduction in sample size as a result of limiting the sample to switchers.

[43] The structure of my data is different due to a relatively low number of repeated individual-firm (i.e. banker-bank) observations, so autocorrelation of errors on the bank level is, ex ante, less of a concern.

Table A1: 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 column 5 only	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 A2: 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 column 5 only	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 A3: Interest Rates and Relationships: Instrumental Variable Regressions (all variables measured in sample)

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 banker and the borrower. The instrument used for *Personal count* (*DS only*) 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. Column 1 presetns the first stage results corresponding to the most complete model specification in Column 5. For each of columns 2 to 5 a separate 2SLS estimation is performed. The first stage results for columns 2 to 4 are omitted for the sake of brevity. *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 for col 5	Spread			
	(1)	(2)	(3)	(4)	(5)
Banker left	-0.195***				
	(0.033)				
Personal count (*DS only*)		-200.739***	-57.381**	-138.761***	-56.287**
		(53.147)	(24.789)	(37.777)	(25.505)
Institutional count	0.060***		0.211	13.790***	1.690
	(0.019)		(2.516)	(4.835)	(2.763)
N	3471	3473	3473	3471	3471
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 A4: 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	Spread	First stage	Spread	First stage	Spread
	(1)	(2)	(3)	(4)	(5)	(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 A5: 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 $\times$ 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	

Table A6: Interest Rates: Sample Splits

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 using sample splits for high and low opacity firms. 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 split the sample betwene firms with above and below median rates of intangibles to assets. Columns 3 and 4 split the sample into firms with and without a senior credit rating. *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.

	Spread			
	No rating	Rating	High intangibles	Low intangibles
	(1)	(2)	(3)	(4)
Personal count	-36.980**	6.553	-24.697	-13.537
	(15.723)	(10.387)	(15.439)	(13.250)
Institutional count	16.459***	-4.751**	0.614	-2.552
	(4.505)	(2.294)	(3.209)	(3.303)
N	1231	2053	1610	1571
R^2	0.626	0.701	0.716	0.653
Firm FE	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Rating FE	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Loan controls	Yes	Yes	Yes	Yes

Table A7: Banker Styles

This table presents correlations between banker fixed effects regarding various loan dimensions. Fixed effects stem from the regressions presented in Table 10. 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 A8: 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 A9: 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 A10: Banker Fixed Effects: Collapsed Banker-Bank Residuals

This table presents regressions of average banker-bank residuals for various loan characteristics on banker fixed effect 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 resulting residuals are then averaged on the banker-bank level. The table represents the F-statistics of regressing 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)
N	1,682	1,682	1,682	1,682	1,682
F(681, 1,000)	1.527***	0.904	0.947	1.144**	0.932