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Bank Loans, Bonds, and Information Monopolies across the Business Cycle

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ABSTRACT

Theory suggests that banks' private information about borrowers lets them hold up borrowers for higher interest rates. Since hold-up power increases with borrower risk, banks with exploitable information should be able to raise their rates in recessions by more than is justified by borrower risk alone. We test this hypothesis by comparing the pricing of loans for bank-dependent borrowers with the pricing of loans for borrowers with access to public debt markets, controlling for risk factors. Loan spreads rise in recessions, but firms with public debt market access pay lower spreads and their spreads rise significantly less in recessions.

THE IDEA THAT THERE ARE BOTH COSTS AND BENEFITS to relying on bank debt is an integral part of the modern theory of corporate finance. As formulated by Rajan (1992), the pros and cons are as follows. Being relatively concentrated, bank debt has more incentive to monitor the borrower than does dispersed "arm's length" debt. But then the private information that the bank gains through monitoring allows it to "hold up" the borrower—if the borrower seeks to switch to a new funding source, it is pegged as a lemon regardless of its true financial condition.[1] In this paper, we seek empirical evidence for this informational hold-up effect by contrasting the pricing of bank loans for bank-dependent borrowers

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[1] Rajan in turn builds on work by Diamond (1984), who models the monitoring advantages of bank loans over arm's-length debt, and Sharpe (1990), who models the informational hold-up aspect of bank loans.

with the pricing of bank loans for borrowers with access to public debt markets. Our evidence suggests that these costs do exist and are economically significant.

Previous work on this topic has focused on firms' choice of funding. In particular, a large literature has looked at whether firms that depend on banks seek one or more bank relationships, arguing that reliance on a single bank implies that the benefits of a single bank outweigh the informational costs, whereas reliance on multiple banks suggests the opposite. Empirical results in this area are mixed; we discuss some of this literature below.

Our paper takes a different approach, focusing on how the pricing of bank loans varies across the business cycle for firms that are more or less dependent on private finance. We begin with Rajan's (1992) theoretical prediction that firms with a higher probability of failure should suffer more from informational hold-up problems; intuitively, such firms are riskier, making the lemons problem that leads to hold-up more significant. Since firms are typically in greater risk of failure during recessions, it follows that, during recessions, banks that have an exploitable information advantage should be able to raise their rates by more than is justified by increased borrower default risk alone.

To test this, we compare bank loan spreads for bank-dependent borrowers with bank loan spreads for borrowers that have continuing access to public debt markets, controlling for a number of loan- and firm-specific factors. Our sample is drawn from the Loan Pricing Corporation's Dealscan database. Since this database focuses on large loans that are often, though not always, syndicated, and since large firms presumably suffer less from hold-up problems than smaller ones, use of this database should bias against finding evidence of such monopolistic loan pricing behavior. Furthermore, our results focus on firms that are also listed in Compustat, which requires that the firms have publicly traded equity—an even more stringent restriction. Nevertheless, we do find that, during recessions, banks raise their rates more for bank-dependent borrowers than for those with access to public bond markets. Further analysis suggests that much of this is due to informational hold-up effects rather than to greater risk of bank-dependent borrowers versus those with bond market access.

As one would expect, we consistently find that, all else equal, firms that have issued public bonds in the past tend to pay lower spreads on their bank loans. We also find that loan spreads rise in recessions, though significantly less so for firms that have previously borrowed in the public bond markets. This finding is robust to a number of loan- and firm-specific controls. Further, the economic magnitudes are significant. In our base specification, recent public bond market access decreases loan spreads by 95 basis points on average; recessions raise loan spreads by 28 basis points for firms without bond market access, but do not raise spreads for firms with market access. Of course, firms with market access may simply be less risky borrowers than firms without such access, but even when we add controls for firm-specific risk, recessions raise spreads by 33 basis points for firms without market access and by 11 basis points for firms with market access.

We measure public bond market access in two different ways. First, we determine whether the firm has *ever* borrowed in the public bond market; second, we ask whether the firm's *most recent* bond issue was in the public market (as opposed to a private placement). While these two measures yield similar results, the coefficients are consistently larger for the second measure. Moreover, the spread the firm pays is higher the longer it has been since the firm's last public bond issue. These results are consistent with informational effects; firms that have not issued public bonds recently may no longer have access to the public bond market, a suspicion that is even stronger if the firm's last bond issue was a private placement.

Our analysis relies on some important assumptions. One assumption is that the change in risk of bank-dependent firms in recessions is not significantly different from the change in risk of firms with access to public bond markets. However, it is possible that there are selection effects such that bank-dependent firms getting financing in recessions are riskier whereas firms with market access are safer. We address this concern in two steps. First, we examine whether seven key proxies for firm risk and quality vary across business conditions differently depending on whether the firm is bank dependent or not. To do so, we construct difference-in-differences tests for each proxy. The results generally suggest that firm selection is not a key driver of our results. Note that in our regressions, we include all of these proxies as explanatory variables; although they are often significant, our basic results do not change. Second, we return to Rajan's (1992) prediction that hold-up problems should increase with any increase in the firm's risk of failure. Although recessions are one proxy for failure risk, so are several other variables. Our model predicts that the interaction between bond market access and any one of these proxies should have a negative impact on spreads. Generally speaking, this is true: Of five alternative risk proxies, three have significantly negative coefficients for their interaction with market access, while the other two have insignificant coefficients.

Another assumption of our analysis is that access to public bond markets is exogenous. In reality, such access is likely to be endogenous, depending on firm-specific variables. To correct for this, we reestimate our equations using a two-step procedure. First, we regress bond market access on a number of exogenous variables including several that proxy for how well known the firm is. We then substitute the predicted value of market access into our loan spread regression. Our results hold without significant change.

In addition to these tests of our key assumptions, we also investigate whether our results are robust to a number of other concerns. These include other risk factors that influence loan spreads, the impact of prior lending relationships, issues involving our sample definition, firm fixed effects, and additional robustness checks. In all cases, our main results continue to hold.

The upshot is that our results suggest that, during recessions, banks do in fact charge significantly higher rates to customers with limited outside funding options. Since our study focuses on firms that are relatively large and have publicly traded equity, this suggests that smaller firms may face even higher costs from relying on bank finance.

In this respect, a recent paper by Schenone (2006) is complementary to ours. Examining a sample of firms with IPOs, she finds that the impact of lending relationship intensity on loan spreads declines after the IPO. This is consistent with a decrease in switching costs and bank information rents. Because pre-IPO firms have limited publicly available information, she is restricted in the controls she can impose, but her results do suggest that pre-IPO firms face significantly higher hold-up costs than those we identify in our sample of public firms.

Our paper is also related to the literature on the number of banks with which firms have relationships. Although the overall results of this literature are mixed, a few papers do suggest that the hold-up problem of Sharpe (1990) and Rajan (1992) is significant. If this problem is a concern, it is likely to be most costly for firms with many growth opportunities that need funding, and so these firms should use more public debt. Conversely, if the hold-up problem is not an issue, the advantages of relationship lending should make bank debt more attractive for firms with greater growth opportunities. Houston and James (1996) examine the mix of bank debt and public debt for a sample of publicly traded U.S. firms. They find that firms with a single bank relationship tend to rely less on bank debt as growth opportunities are higher, but the opposite is true for firms with multiple bank relationships. This is consistent with the notion that having multiple bank relationships mitigates the hold-up problem. Farinha and Santos (2002) examine Portuguese data and find that firms with greater growth opportunities, less liquidity, or greater bank dependence are more likely to switch to multiple bank relationships, all of which is consistent with reducing hold-up problems. For further references, see the surveys by Boot (2000) and Ongena and Smith (2000).

Another related paper is Faulkender and Petersen (2006), who show that publicly traded firms that do not have access to the bond markets have significantly lower leverage than publicly traded firms with such access. This suggests that, even for publicly traded firms, being dependent on bank financing induces significant credit constraints.

Our paper also adds to the literature that examines "bank channel" effects for business cycle transmission and monetary policy. This literature typically focuses on how banks' liquidity and capital concerns lead them to exacerbate downturns by reducing the amount of credit that they supply to borrowers.[2] By contrast, our paper suggests that some of the tightening of credit may be opportunistic, as banks exploit a reduction in competition for some borrowers by charging higher rates.

The remainder of the paper is organized as follows. The next section presents the theoretical basis for our hypothesis. Section II presents our data sources and characterizes our sample. This section also presents our methodology. Section III presents our results. Section IV presents robustness tests. Section V concludes.

[2] Bernanke and Gertler (1995) survey some of this literature. Other work includes theoretical models by Thakor (1996), Holmström and Tirole (1997), and Diamond and Rajan (2000), and empirical work by Kishan and Opiela (2000), Kashyap and Stein (2000), and Hubbard, Kuttner, and Palia (2002).

I. Model: Information Monopoly and Loan Pricing

A. Theory

As noted in the introduction, the theoretical basis for our paper comes from Rajan (1992). In part of his paper, Rajan examines competition between an informed "inside" bank that is already lending to a risky firm and an uninformed "outside" bank that is not currently lending to the firm. The inside bank knows whether the firm will succeed or fail, whereas the outside bank only knows that the firm will succeed with probability q. In this situation, if the outside bank makes a bid to lend to the firm, it faces the "Winner's Curse": Since the inside bank only bids for the loan when the firm will succeed, the outside bank is more likely to win the loan when the firm is failing. The greater this risk, the less aggressively it competes for the loan, allowing the inside bank to earn higher average profits. Effectively, the inside bank's informational advantage gives it limited monopoly power over the borrower, allowing it to hold up the borrower.

More specifically, Proposition 3 of Rajan (1992) shows that the probability that the outside bank makes a bid to lend to the firm is increasing in q, and that the expected profits of the inside bank are decreasing in q. The higher is q, the lower is the risk that the outside bank will make a loan to a failing firm, so the outside bank bids more aggressively, lowering the average risk-adjusted rate that the inside bank can charge. We emphasize "risk-adjusted" because the lower rate is not simply a reflection of the firm's lower default probability; even after adjusting for expected losses, the inside bank earns a lower expected profit. It follows that firms that are perceived by uninformed lenders to have higher chances of failure—lower q—receive less competitive lending rates and pay higher average lending rates, even after adjusting for their higher risk of default. This applies regardless of whether or not the inside bank or outside bank ends up winning the loan: Even if the outside bank wins, it has bid less aggressively, so its risk-adjusted lending rate is higher on average. Since most firms tend to perform worse in recessions, perceived q should fall in recessions, increasing the monopoly power of inside banks and thus the rates that borrowers pay.

We must augment Rajan's model to allow for differences between firms that depend on banks for financing and firms that have access to public debt markets. The model implies that risk-adjusted lending rates should be higher in recessions, assuming that inside banks are always fully informed about the borrower's situation whereas outside banks are always uninformed. But intuitively, inside banks should have a smaller information advantage for firms with access to public bond markets. These firms have a broader investment base, are usually rated by a major credit rating agency, and produce public signals not only through their rating but through the market price of their bonds and through analysis by their underwriters and bond market analysts.

To capture this in a simple way, we assume that outside lenders have a probability ϕ of knowing just as much about the firm as an inside bank (i.e., knowing whether the firm will succeed or fail), and a probability $1 - \phi$ of knowing only that the firm will succeed with probability q. It is easy to show that whenever outside lenders have as much knowledge as the inside bank, Bertrand competition causes the inside bank's profits to drop to zero, so that it breaks even on average. Whenever outside lenders are uninformed, the results are as in Rajan (1992), where inside banks earn informational rents that decrease with the firm's perceived chance of success q. The upshot is that average risk-adjusted lending rates are decreasing in ϕ.[3]

Assuming that ϕ is higher for firms with access to public bond markets, it follows that, all else equal, risk-adjusted lending rates should be lower for such firms than for similar firms that are bank-dependent. Moreover, since inside banks with an information advantage earn higher informational rents as q decreases, average risk-adjusted lending rates should increase more in recessions for bank-dependent firms (low ϕ) than for firms with public bond market access (high ϕ).

We can state these results as the following hypotheses.

Hypothesis 1: *Controlling for other firm and loan characteristics, loan spreads should be lower for firms with public bond market access than for firms that do not have such access.*

Hypothesis 2: *Controlling for other firm and loan characteristics, loan spreads should be higher in recessions than in expansions.*

Hypothesis 3: *Controlling for other firm and loan characteristics, the increase in loan spreads during recessions should be lower for firms with public bond market access than for firms without such access.*

These three hypotheses form the basis of our empirical work. Of these, the most critical are Hypotheses 1 and 3, since these directly focus on the impact of having access to public debt markets.

B. Discussion

Our hypotheses rest on a simple model in which banks have information about their borrowers that is to some extent unavailable to outside investors. We now discuss how well the key assumptions of this model apply to our sample, which (as discussed in the next section) consists of loans to publicly traded firms from single banks or syndicates of banks.

One critical assumption we make has to do with loan structure. Although the model assumes that a loan is made by a single bank, many of our loans are syndicated. This is less of a problem than might appear. More than 96% of the loans in our sample have a single lead arranger. As discussed by Dennis and Mullineaux (2000) and Sufi (2007), in a syndicated loan the bank that is the lead arranger sets the loan terms and rate on behalf of the rest of the syndicate. The

[3] Hauswald and Marquez (2003) present a more complex model of bank competition under asymmetric information that obtains similar qualitative results.

lead arranger is also responsible for ongoing monitoring of the loan terms and the borrower's condition. Finally, as we discuss below, Sufi (2007) finds evidence that loan terms are chosen so as to improve the lead arranger's incentives to monitor the borrower. In other words, the lead arranger of a syndicated loan plays the role of one of the single-bank lenders in our model. Since the vast majority of loans in our sample have only one lead arranger, this is a reasonable assumption.

On a related note, Rajan assumes single-period competition among banks that are exogenously informed or uninformed. Ruckes (2004) shows that when banks must acquire information about borrowers at a cost, competing banks do earn information rents that compensate for these costs.[4] Moreover, when banks compete over many periods, then, following Tirole (1988), tacitly collusive outcomes are possible, especially if the number of competing banks is small. This is especially relevant for syndicated loan settings, where the banks involved repeatedly interact with one another on similar deals.

The last critical assumption of our model is that the inside bank can in fact have private information about the borrower that is not easily available to outside investors. As we now discuss, a good deal of empirical evidence suggests that this assumption is reasonable even for publicly traded firms.

The earliest evidence comes from James (1987) and Lummer and McConnell (1989), who show that announcements of bank loans—particularly renewals of existing loans—convey new information to stock market investors: Renewals with positive revisions to loan terms lead to positive returns for the borrowing firms' shares, and vice versa for renewals with negative revisions. This is consistent with the lending bank having private information gleaned from its existing relationship with the borrower, which is the foundation for Rajan's (1992) model.

As mentioned in the introduction, Houston and James (1996) find evidence that is consistent with hold-up problems for firms that have long-term debt outstanding. These firms are drawn from the Compustat and CRSP databases, both of which we discuss in the next section; Thus, all are publicly traded.

In more recent work, Sufi (2007) examines the role of borrower opacity on syndicated loan structures. He notes that in a syndicated loan, the lead arranger structures the deal and monitors the borrower on behalf of the other syndicate members. If monitoring and the information it provides are private, there is a moral hazard problem between the lead arranger and the other members. Sufi finds that loans to more opaque borrowers give a larger percentage of the loan to the lead arranger and have a more concentrated syndicate. This and other evidence support the hypothesis that there is moral hazard between the lead arranger and the rest of the syndicate, so that when the borrower is harder to monitor, the lead arranger must be given more of the loan to give

[4] The banks mix their strategies of whether or not to become informed, so there is always a chance that one bank will be informed while its rival is not, allowing it to earn information rents. For further discussion of mixed strategy implications and evidence, see von Thadden (2004).

it incentive to monitor, and other syndicate members must be given more of the loan to give them incentive to monitor the lead arranger. This suggests that problems of lender private information are important even for syndicated loans. Although some of his tests involve privately held firms, Sufi also finds that increased opacity among publicly traded firms also leads to greater syndicate concentration, suggesting that private information is an important issue even for publicly traded firms.

We now turn to the data and methodology that we use to test these hypotheses.

II. Data and Methodology

A. Data

The data for this project come from several data sources, including Loan Pricing Corporation's Dealscan database (LPC), Securities Data Corporation's Domestic New Bond Issuances database (SDC), Center for Research on Securities Prices' stock prices database (CRSP), Stock–Watson recession indexes (Stock and Watson (1989)), Salomon Brothers' bond yields indices, and Compustat.

We use LPC's Dealscan database to identify the firms that borrowed from banks and when they did so. This database contains mainly information on syndicated loans, but it also reports information on some nonsyndicated loans. It goes as far back as the beginning of the 1980s. In the first part of that decade the database has a somewhat reduced number of entries but its comprehensiveness has increased steadily over time. It is for this reason that we begin our sample in 1987; as discussed in Section IV, we rerun our tests using only loans from 1990 on and verify that our results still hold. We also use the Dealscan database to obtain information on: individual loans, including the loan's spread over Libor, maturity, seniority status, purpose, and type; the borrower, including its sector of activity and its legal status (private or public firm); and finally, the lending syndicate, including the identity and role of the banks in the loan syndicate.

We rely on SDC's Domestic New Bond Issuances database to identify which firms in our sample issued bonds prior to borrowing in the syndicated loan market. This database contains information on the bonds issued in the United States by American nonfinancial firms since 1970. We also rely on this database to identify some features of the bonds issued by the firms in our sample, including their issuance date, their credit rating, and whether they were privately placed.

We use Compustat to get information on firms' balance sheets. Even though LPC contains loans from both private firms and publicly listed firms, given that Compustat is dominated by the latter we have to exclude from our sample the loans borrowed by privately held firms.

We rely on the CRSP database to link companies and subsidiaries that are part of the same firm, and to link companies over time that went through mergers, acquisitions, or name changes.[5] We then use these links to merge the LPC, SDC, and Compustat databases in order to find out the financial condition of the firm at the time it borrowed from banks and if by that date the firm had already issued bonds. We also use CRSP to determine our measure of stock price volatility.

We use Salomon Brothers' yield indices on new industrial long-term bonds to control for changes in the cost to access the bond market. We consider the indices on yields of AAA and BBB rated bonds because these go further back in time than say the indices on the investment-grade and below-grade bonds. We use the yields on 30- and 5-year Treasury bonds to determine the slope of the Treasury yield curve and therefore control for expected changes in the short-term interest rates.

Finally, we rely on the Stock–Watson Experimental Coincident Recession Index to define recessions and expansions. This is a monthly index that measures the probability with which the economy is in a recession. Because we want to identify important recessions, as opposed to just short periods of slow output growth, we identify recessions as any period of time of two (or more) consecutive quarters with a quarterly Stock–Watson index larger than the index's lifetime mean (this index started in 1959). This results in three recessions during our sample period. The first recession was from 1989:2 to 1989:3. The second recession was from 1990:3 to 1991:2. The third recession started in 2000:4 and ended in 2002:1.[6] According to this classification there are 52 expansion quarters and 12 recession quarters during our sample period 1987:1–2002:4.

The reason that we use the Stock–Watson Recession Index to determine recessions rather than NBER's definitions is that banks set their loan rates on the basis of what they know at the time they extend the loan. The Stock–Watson Experimental Coincident Recession Index is based on currently available information, whereas the NBER recession dates are set retroactively; thus, the Stock–Watson Index is a more accurate indicator of banks' concurrent information sets. In Section IV we use the NBER recession dates and show that our qualitative results do not change. Moreover, although the first of our recessions (1989:2–1989:3) does not overlap with the NBER recession dates, in Section IV we rerun our models dropping observations before 1990 (including this first recession) and find that our qualitative results do not change.

[5] The process we use to link LPC, SDC, and Compustat can be summarized as follows. The CRSP data is first used to obtain CUSIPs for the companies in LPC where this information was missing through a name-matching procedure. With a CUSIP, LPC can then be linked to both SDC and Compustat, which are CUSIP-based data sets. We proceed by using the PERMCO variable from CRSP to group companies across CUSIP, since that variable tracks the same company across CUSIPs and ticker changes. We adopt conservative criteria to solve the cases of multiple "cross-links" between companies we encounter and drop the companies that can not be reasonably linked.

[6] The two most recent recessions according to this classification overlap with the recessions that existed during our sample period according to the National Bureau of Economic Research Business Cycle Dating Committee.

B. Methodology

In order to test Hypotheses 1 through 3, we need to investigate loan spreads and determine the impact of borrower access to public bond markets and of recessions, controlling for various borrowing firm and loan-specific characteristics. We, therefore, estimate the following model of the loan credit spreads:

$$\begin{aligned} LOANSPREAD_{f,l,t} &= c + \delta \cdot PUBOND\,ACCESS_{f,t} + \zeta \cdot REC_t \\ &\quad + \eta \cdot REC_t \cdot PUBOND\,ACCESS_{f,t} + \sum_{i=1}^{L} \psi_i X_{i,f,t} \\ &\quad + \sum_{j=1}^{F} \nu_j Y_{j,l,t} + \epsilon_{f,t}. \end{aligned} \tag{1}$$

Here $LOANSPREAD_{f,l,t}$ is the spread over LIBOR of loan l of firm f taken out at date t (this is a standard measure of loan pricing), $PUBOND\,ACCESS_{f,t}$ is a dummy variable that takes the value one if firm f has access to public bond markets as of the date of the loan (as we discuss below, we use two different definitions of this variable), and REC_t is a dummy variable that takes the value one if there was a recession at this date. The $X_{i,f,t}$ represent firm-specific variables and the $Y_{j,l,t}$ represent various loan-specific variables that might be expected to affect the loan's credit risk (more on these variables below). Note that our three hypotheses are nested in this single equation: Hypothesis 1 is equivalent to δ being negative (firms with public market access pay lower spreads); Hypothesis 2 is equivalent to ζ being positive (recessions increase spreads); Hypothesis 3 is equivalent to η being negative (in recessions, spreads increase less for firms with public market access than for bank-dependent firms).

One concern in relation to our methodology is that spreads reflect firm-specific credit risk, and it is possible that firms with public bond market access differ in risk from firms without such access. We control for this possibility in two ways. The first is apparent from the estimation equation: We include controls that proxy for different aspects of credit risk. The second was alluded to in the introduction: Since bond market access itself is likely to be endogenous, in part depending on unobservable risk factors, below we use a two-stage procedure to estimate whether firms have access to the public bond markets.

On a related note, even if we find that the coefficient on the recession dummy is positive, it is possible that this simply reflects a premium for additional risk in recessions rather than increased hold-up power for banks during recessions. Also, the recession dummy may be positively correlated with other risk proxies. Thus, Hypothesis 2 is not identified cleanly—a positive coefficient here is consistent with the hypothesis but may indicate a risk premium; conversely, the coefficient may understate the amount of hold-up due to risk if some of this is incorporated in the coefficients of other risk proxies.

By contrast, the interaction between the recession and bond market access dummies identifies Hypothesis 3, subject to one caveat: It is possible that there is differential selection in recessions between bank-dependent firms and firms with market access. In particular, if the risk of bank-dependent firms seeking loans is higher during recessions, whereas the risk of firms with market access seeking loans is relatively lower during recessions, then this would provide an alternative explanation for finding a negative coefficient η on the interaction term.[7] To deal with this, we use three strategies. The first is that, as noted above, we do control for observable risk proxies. The second is to do difference-in-difference tests for observable risk proxies broken down by whether there is a recession and by whether the firm is bank-dependent; if these tests are not significant, then there is no significant difference in observable risk selection for the two types of firms. We do this in Section IV. Finally, in Section IV we also rerun our basic regression substituting observable risk proxies for the recession dummy in the interaction variable used in equation (1); if our model is correct, then the interaction between bond market access and any risk proxy should have a negative coefficient.

A critical part of our methodology is the definition of the public bond market access dummy. In our theory, this dummy should reflect whether the firm currently has access to the public bond markets, in the sense that the firm can tap into a large number of reasonably well-informed investors. We use two different definitions to capture firm access to the bond market. The first is the dummy variable *PBOND*, which equals one if the firm in question has ever issued a public bond and zero otherwise. However, one difficulty with this measure is that, if the bond was issued some time ago, it is possible that the firm's circumstances have changed in a way that makes continued access to the bond market harder or impossible. Our second definition is given by the dummy variable *MRPBOND*, which equals one if the firm's most recent bond issue was public and zero otherwise. Thus, this second variable focuses on more recent access to the bond market. Most of our reported results focus on this second measure, but we do examine the first measure as well in some of our tests.

We complement our definition of firms' access to the public bond market by controlling for the time that has elapsed since a firm issued its most recent public bond (prior to the loan). Specifically, we control for *LTIMRPBOND*, which measures the log of the number of months since the firm issued its most recent public bond. The more time that has passed since the bond issue, the less current is the information produced at that time, and so the greater the information advantage of inside banks over outside lenders. Thus, we expect that this variable should have a positive effect on spreads.

Note that we do not count privately placed bonds as a measure of public bond market access. We believe private placements are very different from public issues, reaching a smaller set of investors and thus not increasing informed competition as much as a public issue does. This is consistent with earlier work that considers private placements to be closer to syndicated bank loans than to public bonds. We do investigate the impact of having access to private placements but not to public issues by including an additional dummy variable, *BOND*, which equals one if the firm has ever issued a bond and zero otherwise.

[7] We thank the referee for suggesting this alternative explanation.

In some of our specifications, we break down the firms that have access to public bond markets by the credit rating of their bonds, adding two dummy variables, *MRPBONDblg* and *MRPBONDnr*, respectively. The first variable equals one if the firm's most recent bond (prior to the loan) was publicly issued and rated below investment grade; the second variable equals one if the firm's most recent bond was publicly issued and did not have a rating in the SDC database. We consider for this purpose the Moody's ratings. When the bond does not have a Moody's rating but it has a rating from the S&P, we consider this rating instead.

As noted above, we also include a number of firm-specific controls, X, and loan-specific controls, Y, that may affect a firm's risk. We begin by discussing the firm-specific variables that we use, since these are more likely to be exogenous to the loan rate that is set. Several of these variables are proxies for the risk of the firm. First, *LAGE* is the log of the firm's age in years. To compute firm age we proxy the firm's year of birth by the year of its equity IPO. Older firms are typically better established and hence less risky, so we expect this variable to have a negative effect on the loan spread. Next, *LSALES* is the log of the firm's real sales in hundreds of millions of 1980 dollars, computed with the CPI deflator. Larger firms are usually better diversified across customers, suppliers, and regions, so again we expect this to have a negative effect on the loan spread.

We also include variables that proxy for the risk of the firm's debt rather than that of the overall business. Specifically, *PROFMARGIN* is the firm's profit margin (net income divided by sales). More profitable firms have a greater cushion for servicing debt and so should pay lower spreads on their loans. A more direct measure of the ability to service debt is its interest coverage, which we measure by *LINTCOVERAGE*, the log of one plus the interest coverage ratio (EBITDA divided by interest expense) truncated at zero.[8] Again, a higher interest coverage ratio should make the firm's debt less risky. Next, *LEVERAGE* is the firm's leverage ratio (debt over total assets); higher leverage suggests a greater chance of default, so this should have a positive effect on spreads. We also attempt to account for the firm's probability of bankruptcy by controlling for its *ZSCORE*. This is an indicator of risk in the sense that it measures the number of standard deviations below the mean by which profits would have to fall in order to eliminate the firm's equity. Thus, the higher the *ZSCORE* the lower the firm's risk, so we expect this variable to have a negative effect on the loan spread.[9]

[8] For firms with no interest expense this variable is set equal to the log of one plus earnings before taxes and depreciation.

[9] Following Boyd and Runkle (1993), we estimate the *ZSCORE* for a firm using the following definition:

$$Z = \frac{1}{S_r}\left[\frac{1}{n}\sum_{j=1}^{n}\frac{2\tilde{\pi}_j}{A_j + A_{j-1}} + \frac{1}{n}\sum_{j=1}^{n}\frac{E_j + E_{j-1}}{A_j + A_{j-1}}\right],$$

where π is the firm's profits, A is its assets, E is its equity, and S_r is the estimated standard deviation of r, the firm's return on assets, computed with quarterly data over the past 3 years.

Another aspect of credit risk is losses to debt holders in the event of default. To capture this, we include several variables that measure the size and quality of the asset base that debt holders can draw on in default. In particular, *TANGIBLES* is the firm's tangible assets—inventories plus plant, property, and equipment—as a fraction of total assets. Tangible assets lose less of their value in default than do intangible assets such as brand equity, so we expect this variable to have a negative effect on spreads. Next, *ADVERTISING* is the firm's advertising expense divided by sales; this proxies for the firm's brand equity, which is intangible, so we expect this to have a positive effect on spreads.[10] Similarly, *R&D* is the firm's research and development expense divided by sales; this proxies for intellectual capital, which is intangible, and so we also expect this to have a positive effect on spreads. We also include *NWCDEBT*, the firm's net working capital (current assets less current liabilities) divided by total debt; this measures the liquid asset base, which is less likely to lose value in default, so we expect this to have a negative effect on spreads.[11] In addition, we include *MKTBOOK*, the firm's market-to-book ratio, which proxies for the value the firm is expected to gain by future growth. Although growth opportunities are vulnerable to financial distress, we already have controls for the tangibility of book value assets. Thus, this variable could have a negative effect on spreads if it represents additional value (over and above book value) that debtholders can access in part in the event of default.

Finally, we include dummy variables for single-digit SIC industry groups. A given industry may face additional risk factors that are not captured by the list of variables above, so this allows us to capture such risk at a very broad level.

By contrast with these firm-level variables, many of the loan-specific variables are endogenous, being jointly determined along with the lending rate. This can create a problem if both the feature and the loan spread are affected by an unobservable factor. For this reason, when we estimate our model of loan spreads we first account for firm-specific controls and then investigate what happens when we add the following loan-specific controls. We include dummy variables equal to one if the loan has restrictions on paying dividends (*DIVRESTRICT*), is senior (*SENIOR*), is secured (*SECURED*), has a sponsor (*SPONSOR*), and has a guarantor (*GUARANTOR*). All else equal, any of these features should make the loan safer, decreasing the spread, but it is well known that lenders are more likely to require these features if they think the firm is riskier (see, for example, Berger and Udell (1990)), so the relationship may be reversed. Loans with longer maturities (measured by the log of maturity in years, *LMATURITY*) may face greater credit risk, but they are more likely to be granted to firms that are thought to be more creditworthy; again, the effect on spread is ambiguous. Larger loans (measured by *LLOANAMT*, the log

[10] Firms are required to report expenses with advertising only when they exceed a certain value. For this reason, this variable is sometimes missing in Compustat. The same is true of expenses with research and development. In either case, when the variable is missing we set it equal to zero. We discuss in Section IV what happens when we drop these variables from our models.

[11] For firms with no debt, this variable is set equal to the difference between current assets and current liabilities.

of loan amount in hundreds of millions of 1980 dollars) may represent more credit risk, raising the loan rate, but they may also allow economies of scale in processing and monitoring the loan; again, the sign of this variable's effect on loan spreads is ambiguous.

Because the purpose of the loan is likely to impact its credit spread, we include dummy variables for loans taken out for corporate purposes (*CORPURPOSES*), to repay existing debt (*REFINANCE*), to finance takeovers (*TAKEOVER*), and for working capital purposes (*WORKCAPITAL*). Similarly, we include dummy variables to account for the type of the loan: lines of credit (*CREDITLINE*), term loans (*TERMLOAN*), and bridge loans (*BRIDGELOAN*).

We include an additional variable, *RENEWAL*, which is a dummy variable indicating whether the loan is a renewal of an existing loan. If lenders renew a loan, this may indicate that the firm is in relatively good shape. Since there is a chance ϕ that outside lenders have the same information at the time the existing bank is renewing the loan, on average, there would be more aggressive competition, leading to a negative effect on spread. Unfortunately, this variable is often missing from Dealscan, which limits our ability to test for this effect.

We also control for the size of the loan syndicate by including the log of the number of lenders in the syndicate, *LLENDERS*, and by including a dummy variable, *SINGLENDER*, to distinguish the loans with a single lender. Syndicate participants do not compete to extend the loan to the firm; instead, they act cooperatively. Thus, larger syndicates do not necessarily imply higher competition to extend a loan to a firm. In contrast, larger loans usually have larger syndicates. Thus, the effect of this variable on spreads is ambiguous for the same reasons discussed above regarding the effect of the size of the loan on spreads.

Lastly, in Section IV, we conduct a number of robustness tests. In addition to the robustness checks already mentioned (including a two-stage procedure to estimate whether firms have access to the public bond markets, and alternative versions of Hypothesis 3), we examine whether our main findings continue to hold when we control for (a) the firm's sales growth, *SALESGROWTH*, (b) other measures of firm risk, for example, the volatility of the firm's stock, *STOCKVOL*, and the firm's excess stock return, *STOCKRET*, (c) the slope of the yield curve, *TSPREAD*, and the difference in the yields of new bonds rated BBB and those rated AAA, *BBBSPREAD*, (d) the level of *LIBOR*, which is the interest rate over which we compute our loan spreads, (e) the borrower's lending relationships with the banks in the loan syndicate, and (f) the interaction of our recession dummy with measures of the firm's risk. Also, given that in several instances the loans to a firm in the LPC database are part of a deal package, and therefore have many features in common, we randomly select one loan per deal and rerun our models on the new sample. Because Dealscan's coverage improved significantly over the early 1990s, we rerun our tests using the subsample of loans made from 1990 to 2002, dropping the latter part of the 1980s. Because some loans are renegotiations of existing loans, which may indicate greater risk of failure, and the incidence of renegotiations might vary across bank-dependent and non-bank-dependent firms, we rerun our tests dropping loans that are renegotiations. Finally, we rerun our core regressions using firm fixed effects. In addition, we rerun these models clustering by time as well as by firm to account for possible errors in our measurement of recessions. In Section IV, we present these results as well those of some additional robustness tests with more complete motivation for each of the tests.

C. Sample Characterization

As we mentioned above, our data source for loans, the LPC database, includes loans taken out both by public firms and by privately held firms. In order to get additional information on firms, however, we need to merge this data set with Compustat. Because Compustat only contains information on publicly listed firms we have to drop from our original sample all of the loans taken out by private firms and by the public firms that are not in Compustat. This leaves us with a sample of 13,846 loans. These loans were taken out by 4,244 firms.

Table I presents the definitions of the variables we use in this study. Table II characterizes our sample of bank loans. The table compares the loans taken out by bank-dependent firms with those taken out by non-bank-dependent firms, that is, firms with access to the public bond market. We classify a firm as non-bank-dependent if the firm's most recent bond prior to its loan was issued in the public bond market ($MRPBOND = 1$), and as bank-dependent otherwise. As the first row of the table shows, on average, bank-dependent firms pay higher credit spreads on their loans than non-bank-dependent firms. Looking at the variables that we consider we find a pattern of results that is in general consistent with what one would expect. Our firm controls, for example, indicate that bank-dependent firms are younger, smaller, have fewer tangible assets, and spend more on advertising and R&D than do non-bank-dependent firms. The stock price of bank-dependent firms is on average more volatile but these firms are less highly levered and have greater interest coverage—both of which are consistent with the findings of Faulkender and Petersen (2006). Also, on average, these firms have higher market-to-book ratios, and they are less likely to borrow from banks they borrowed from in the last 3 years; however, this last point is somewhat misleading, as we will show shortly.

With regard to our loan controls, bank-dependent borrowers take out smaller and longer-maturity loans. Loans to bank-dependent firms are provided by syndicates with fewer banks and are more likely to be provided by a single lender. These firms are more likely to have a guarantor and to face dividend restrictions as a result of their loans. Their loans, in addition, are somewhat more likely to be senior and secured.[12] Finally, bank-dependent firms are more likely to borrow through term loans and to use their loans for working capital

[12] The low percentage of loans that are secured is most likely the result of the large number of missing observations this variable has in the LPC database. Throughout the paper we code missing observations of this variable as unsecured loans. In Section IV, we discuss what happens when we add a separate dummy variable for these missing observations.

Table I
Variable Definitions

Variable	Definition
LOANSPREAD	Loan spread over LIBOR at the time of the loan origination.
AGE	Age in years.
OLDERFIRMS	Dummy variable equal to one for firms older than 20 years.
SALES	Sales in hundreds of millions of dollars at 1980 prices.
SALESGROWTH	The firm's sales growth in the year prior to the loan.
TANGIBLES	Property, plant, and equipment plus inventories over assets.
TANGIBLES-B50	Dummy variable equal to one for firms with TANGIBLES below the sample median.
ADVERTISING	Advertising expenses over sales.
R&D	Research and development expenses over sales.
MKTBOOK	Market-to-book ratio.
PROFMARGIN	Net income over sales.
INTCOVERAGE	Earnings before taxes and depreciation over interest expenses. When the firm has no interest expenses this variable is set equal to earnings before taxes and depreciation.
INTCOVERAGE-B50	Dummy variable equal to one for firms with INTCOVERAGE below the sample median.
NWCDEBT	Current assets minus current liabilities over total debt. When the firm has no debt this variable is set equal to current assets minus current liabilities.
LEVERAGE	Total debt over assets.
LEVERAGE-A50	Dummy variable equal to one for firms with LEVERAGE above the sample median.
ZSCORE	Computed with quarterly data over the previous 3 years. See footnote (9) for the definition of the ZSCORE that we use.
ZSCORE-B50	Dummy variable equal to one for firms with a ZSCORE below the sample median.
STOCKVOL	Standard deviation of stock return over the year leading up to the loan date.
STOCKVOL-A50	Dummy variable equal to one for firms with STOCKVOL above the sample median.
STOCKRET	Return on the company's stock over the preceeding year minus the return on the equally weighted CRSP index of NYSE/AMEX stocks over the year.
RLENDINGnarrow	Dummy variable equal to one if the firm borrowed at least once from the lead underwriter(s) in the loan syndicate over the previous 3 years.
RLENDINGbroad	Dummy variable equal to one if the firm borrowed at least once from any bank in the loan syndicate over the previous 3 years.
CORPURPOSES	Dummy variable equal to one when loan is for corporate purposes.
REFINANCE	Dummy variable equal to one when loan is to repay existing debt.
TAKEOVER	Dummy variable equal to one when loan is for takeover purposes.
WORKCAPITAL	Dummy variable equal to one when loan is for working capital purposes.
CREDITLINE	Dummy variable equal to one for lines of credit.
TERMLOAN	Dummy variable equal to one for term loans.
BRIDGELOAN	Dummy variable equal to one for bridge loans.
LOANAMT	Loan amount in hundreds of millions of dollars at 1980 prices.
DIVRESTRICT	Dummy variable equal to one when borrower is subject to dividend restrictions.

(*continued*)

Table I—*Continued*

Variable	Definition
SENIOR	Dummy variable equal to one when loan is senior.
SECURED	Dummy variable equal to one when loan is secured.
RENEWAL	Dummy variable equal to one when loan is a renewal.
GUARANTOR	Dummy variable equal to one when borrower has a guarantor.
SPONSOR	Dummy variable equal to one when borrower has a sponsor.
LENDERS	Number of lenders in the loan syndicate.
SINGLELENDER	Dummy variable equal to one when the loan syndicate has a single lender.
MATURITY	Maturity of the loan in years.
BOND	Dummy variable equal to one for firms that issued bonds prior to their loan.
PBOND	Dummy variable equal to one for firms that issued public bonds prior to the loan.
PBONDblg	Dummy variable equal to one for those firms with PBOND=1 whose last public bond prior to the loan was rated below grade.
PBONDnrt	Dummy variable equal to one for those firms with PBOND=1 whose last public bond prior to the loan was not rated.
MRPBOND	Dummy variable equal to one for firms whose most recent bond prior to the loan was a public bond.
MRPBONDblg	Dummy variable equal to one for firms whose last bond prior to the loan was a public bond with a below grade rating.
MRPBONDnrt	Dummy variable equal to one for firms whose last bond prior to the loan was a public bond not rated.
REC	Dummy variable equal to one for loans borrowed during a recession as defined by the Stock–Watson Experimental Coincident Recession Index. See Section II.A for the dates of the recessions during our sample period.
NYSE	Dummy variable equal to one for firms that trade in the NYSE.
LEHMAN	Dummy variable equal to one for firms that issued bonds in amounts larger than the minimum necessary to be included in the Lehman Brothers' Corporate Bond Index.
S&P500	Dummy variable equal to one for firms that are members of the S&P500 index.
INDACCESSmrpbond	Percentage of the firms in the same industry (as defined by the two-digit SIC code) that have access to the bond market as defined by MRPBOND.
INDACCESSpbond	Percentage of the firms in the same industry (as defined by the two-digit SIC code) that have access to the bond market as defined by PBOND.
TSPREAD	Treasury yield curve slope computed as the difference between the 30- and 5-year Treasury bonds.
BBBSPREAD	Spread of new BBB rated bonds over new AAA rated bonds.

purposes, to fund takeovers, and to refinance; they are less likely to borrow through credit lines and to use bridge loans.

Figure 1 graphs the numbers of loans to bank-dependent firms and to firms with access to the bond market over time. As can be seen, there are many times more loans to bank-dependent borrowers than to non-bank-dependent

Table II

Sample Characteristics

Borrowers are defined as non-bank-dependent if their most recent bond prior to the loan was a public bond, otherwise they are defined as bank-dependent. See Table I for variable definitions.

	Firms			
Variables	Bank-Dependent	Non-Bank-Dependent	Difference	*t*-Statistic
LOANSPREAD	226.186	124.683	101.503	36.165***
FIRM CONTROLS				
AGE	14	30	−16	57.119***
SALES	6.308	36.689	−30.381	39.832***
TANGIBLES	0.676	0.748	−0.072	12.673***
ADVERTISING	0.028	0.014	0.014	2.231**
R&D	0.586	0.015	0.571	3.182***
MKTBOOK	2.150	2.032	0.118	3.429***
PROFMARGIN	−0.322	0.017	−0.339	1.726*
INTCOVERAGE	33.825	8.854	24.971	3.974***
NWCDEBT	18.660	7.571	11.089	1.061
LEVERAGE	0.313	0.360	−0.047	9.409***
ZSCORE	2.424	7.842	−5.418	1.412
STOCKVOL	0.043	0.012	−0.031	7.271***
RLENDINGnarrow	30.906	44.141	−13.235	13.539***
RLENDINGbroad	38.610	57.465	−18.855	18.544***
LOAN CONTROLS				
CORPURPOSES	22.779	25.478	−2.699	3.067***
REFINANCE	31.169	24.044	7.125	7.510***
TAKEOVER	13.354	12.534	0.820	1.165
WORKCAPITAL	16.688	7.104	9.584	13.099***
CREDITLINE	63.217	76.127	−12.910	13.171***
TERMLOAN	30.683	17.520	13.163	14.203***
BRIDGELOAN	1.795	2.766	−0.971	3.334***
LOANAMT	0.760	3.103	−2.343	34.634***
DIVRESTRICT	48.049	28.381	19.668	19.297***
SENIOR	95.613	94.877	0.736	1.698*
SECURED	9.132	4.304	4.828	8.520***
RENEWAL	1.466	0.684	0.782	3.320***
GUARANTOR	2.235	1.332	0.903	3.067***
SPONSOR	5.688	5.499	0.189	0.394
LENDERS	5	12	−7	39.440***
SINGLENDER	43.882	15.028	28.854	29.478***
MATURITY	4	3	1	5.821***
SECTOR OF ACTIVITY				
SERVICES	16.945	8.846	8.099	10.875***
TRADE	15.919	11.749	4.170	5.612***
TRANSPORTATION	11.193	19.741	−8.548	12.278***
MANUFACTURING	49.478	52.937	−3.459	3.325***
# Observations	10,918	2,928	–	–
# Firms	3,553	691	–	–

***,**, and * mean significant at the 1%, 5%, and 10% level, respectively.

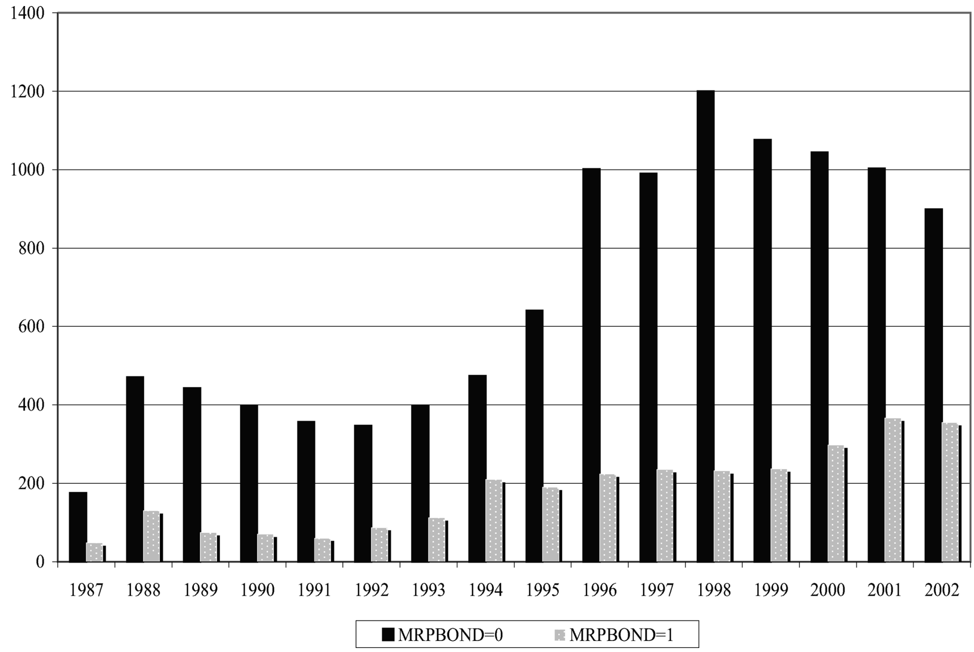

Figure 1. Time distribution of loans of bank-dependent and non-bank-dependent firms.

borrowers, and loans to bank-dependent borrowers seem to be more sensitive to credit conditions. For example, although both types of loans decrease in 1989 (the year of our "mini recession"), loans to bank-dependent firms continue to decline in the early 1990s whereas those to non-bank-dependent firms are relatively stable. A similar pattern occurs in the late 1990s, where loans to bank-dependent firms peak in 1998 (the year of the Russian and Long Term Capital Management debt crises) and then decline into the early 2000s, whereas loans to non-bank-dependent firms do not peak until 2001 and merely level off.

As a first step towards testing our model, we examine the extent to which firms switch to banks they do not have relationships with. As we noted above, according to Table II, bank-dependent firms are less likely than other firms to borrow from banks they borrowed from in the last 3 years. This result turns out to be an artifact of our relationship variable. Recall that this takes the value one if the firm borrowed at least once from its current lender in the previous 3 years. Given that we do not have information on firms' lending relationships prior to their first loan in the sample, this variable takes by definition the value zero for those loans. When we limit our computations to the second and all subsequent loans of each firm, as we report in Table III, 43.5% of bank-dependent firms borrow from a different bank, whereas 53.0% of the non-bank-dependent firms borrow from a different bank, and the difference is significant. Thus, bank-dependent firms switch from their relationship lenders less often than do non-bank-dependent firms; that is, bank-dependent firms are

Table III

Percentage of Loans Borrowed from a Bank Other than the Firm's Relationship Lender

Percentage of loans borrowed from a bank other than the firm's relationship lender. A bank is defined as a relationship lender if it acted as a lead underwriter at least once for the loan(s) the firm took out in the 3 years prior to the current loan (this definition matches our definition of RLENDINGnarrow). Percentages are computed after we exclude the first loan of each firm in the sample given that we do not have information on the firm's lending relationships prior to that loan. Borrowers are defined as non-bank-dependent if their most recent bond prior to the loan was a public bond, otherwise they are defined as bank dependent. Recessions are defined by the Stock–Watson Experimental Coincident Recession Index. See Section II.A for the dates of the recessions during our sample period.

	Bank-Dependent-Firms	Non-Bank-Dependent Firms	Difference	t-Statistic
		All loans		
	43.5	53.0	9.5	6.38
		Depending on the state of the economy		
Expansions	43.8	53.0	9.2	6.33
Recessions	42.3	53.3	11.0	3.57

Source: Authors' computations.

more likely to borrow from a relationship bank, which is what our model would predict.

In Table III, we also report the percentages of firms that switch banks in expansions versus recessions. Once again, we classify firms as non-bank-dependent if their most recent bond issue was public. Although bank-dependent firms remain less likely to switch, there is no clear-cut difference between the percentages of switchers in the two economic environments. Our model would predict that firms (especially bank-dependent firms) are more likely to switch in favorable economic times, but, although this is true for bank-dependent firms, it is not true for non-bank-dependent firms. On the other hand, it remains true that in both recessions and expansions, bank-dependent firms are significantly less likely to switch than non-bank-dependent firms.

III. Results

A. A Univariate Analysis of Loan Spreads

We begin our analysis of loan spreads by examining the impact of bank dependence and recessions, ignoring all other factors. The results of this comparison are presented in Table IV. As we now discuss, these results are consistent with our predictions.

In the table, Models 1 through 4 are univariate regressions of loan spread on the dummy variables for bond market access and for recessions. The terms in parentheses below the coefficient estimates are p-values. Throughout, standard errors are adjusted for clustering by firm. We see that firms that have issued

Table IV

Loan Spreads at Issue Date for Bank-Dependent and Non-Bank-Dependent Borrowers

The dependent variable is LOANSPREAD. See Table I for variable definitions. Models estimated with robust standard errors and clustered by firm to correct for correlation across observations of a given firm. Values in parentheses are p-values.

Variables	1	2	3	4	5	6	7
CONSTANT	237.990	230.747	226.186	200.831	233.190	224.845	220.722
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
BOND	−74.537				−74.386		
	(0.000)				(0.000)		
PBOND		−90.244				−85.745	
		(0.000)				(0.000)	
MRPBOND			−101.503				−95.263
			(0.000)				(0.000)
REC				19.523	26.247	31.079	27.797
				(0.000)	(0.000)	(0.000)	(0.000)
REC · BOND					−5.076		
					(0.545)		
REC · PBOND						−24.771	
						(0.005)	
REC · MRPBOND							−31.507
							(0.000)
Adjusted-R^2	0.0690	0.0840	0.0863	0.31	0.0736	0.0894	0.0912
# Observations	13,846	13,846	13,846	13,846	13,846	13,846	13,846

any bond typically pay 75 basis points less than other firms, firms that have issued public bonds pay 90 basis points less than other firms, and firms whose most recent bond was publicly issued pay 102 basis points less than other firms. All of these results are highly significant and consistent with Hypothesis 1. The larger effect for public bonds is consistent with our prediction that issuers of public bonds face smaller informational hold-up problems (in our model, higher ϕ) than do issuers of private placements. The even larger effect for firms whose most recent bond issue was public rather than private is also consistent with this prediction, since recent public bond market access is likely to be a better indicator of the firm's current ability to access this market. Similarly, during recessions, spreads are 20 basis points higher than during expansions, which is consistent with Hypothesis 2, subject to the caveat mentioned in the previous section.[13]

Models 5 through 7 examine Hypotheses 1, 2, and 3 simultaneously. Model 5 measures market access by whether a firm has ever issued any bond, public or private. We see that firms that have issued any bond pay less than bank-dependent firms, and recessions raise spreads, with magnitudes that are

[13] For evidence on the impact of the state of the economy on bond credit spreads, see Santos (2006), Bernanke (1993), and Fama and French (1989).

significant and close to the results from Models 1 and 4. Nevertheless, the interaction between the recession and market-access dummies has a small, negative, and statistically insignificant coefficient, which does not strongly support Hypothesis 3.

Since we have argued that public bond market access is the better measure of not being bank-dependent, we now turn to our two variables that measure such access. Model 6 measures market access by whether a firm has ever issued a public bond. These results are much stronger. Firms that have issued public bonds pay less than other firms, with a magnitude comparable to the results of Model 2, and recessions raise rates by 31 basis points, 11 points more than in Model 4. Most strikingly, the interaction term is strongly statistically and economically significant, at minus 25 basis points. Effectively, firms that have issued public bonds experience only a 6-basis point rise in spreads during a recession, unlike other borrowers, who face an increase of 31 basis points. This is consistent with Hypothesis 3.

Model 7 measures market access by whether the firm's most recently issued bond was public. These results are even stronger. Again, the direct effect of having one's most recent bond issue be public is comparable to that from Model 3, and at 28 basis points the direct effect of a recession is somewhat greater than in Model 4. The interaction term now has an even stronger negative effect than in Model 6, at minus 32 basis points. Although the coefficient on the interaction term more than offsets the coefficient on the recession dummy, by 4 basis points, this difference is not statistically different from zero.

In sum, consistent with our predictions, access to public bond markets produces first-cut results that are consistent with our hypotheses on the impact of bank information monopolies on loan pricing. By contrast, access to private placements has mixed effects, reducing average loan spreads relative to those of firms without any bond market access but losing much of this advantage during recessions. In what follows, we focus on access to public bond markets as our measure of relative lack of bank-dependence. Nevertheless, a caveat is in order: None of these results control for firm and loan risk. Since these are likely to vary across borrower types (those with access to public bonds versus those without) and across time (recessions versus expansions), the results in this section are suggestive at best. We now turn to multivariate results that include controls for firm and loan characteristics.

B. A Multivariate Analysis of Loan Spreads

Our initial multivariate results are shown in Table V. In Models 1 through 4, we measure public bond market access by whether a firm's most recent bond issue was public; in Models 5 and 6, we measure access by whether the firm has ever issued a public bond. As we will see, the results continue to support Hypotheses 1 through 3.

Model 1 begins the new analysis by including the firm-specific controls derived from Compustat. Although having access to the public bond market

Table V

Loan Spreads for Bank-Dependent and Nondependent Borrowers: Multivariate Analysis

The dependent variable is LOANSPREAD. LINTCOVERAGE = log(1 + INTCOVERAGE), with INTCOVERAGE truncated at 0. LTIMRPBOND: Log of 1 plus the number of months since the firm issued its most recent public bond. LAGE = Log(AGE). LSALES = LOG(SALES). LLENDERS = LOG(LENDERS). LMATURITY = Log(MATURITY). See Table I for variable definitions. Included in the regressions but not shown in the table are also dummy variables for the issuer's sector of activity as defined by one-digit SIC code. Models are estimated with robust standard errors and clustered by firm to correct for correlation across observations of a given firm. Values in parentheses are *p*-values.

Variables	1	2	3	4	5	6
CONSTANT	342.673	339.422	628.031	629.020	629.694	630.476
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
MRPBOND	−17.410	−36.940	−38.073	−48.649		
	(0.000)	(0.000)	(0.000)	(0.000)		
MRPBONDblg		37.206		34.670		
		(0.000)		(0.000)		
MRPBONDnrt		44.295		35.484		
		(0.008)		(0.007)		
PBOND					−18.324	−33.729
					(0.029)	(0.000)
PBONDblg						30.480
						(0.000)
PBONDnrt						31.940
						(0.004)
REC	32.801	32.278	22.492	21.790	22.067	21.555
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
REC · MRPBOND	−21.598	−15.797	−20.212	−16.483		
	(0.002)	(0.037)	(0.001)	(0.010)		
REC · MRPBONDblg		6.050		8.322		
		(0.663)		(0.448)		
REC · MRPBONDnrt		−31.824		−11.709		
		(0.293)		(0.666)		
REC · PBOND					−11.301	−15.119
					(0.068)	(0.023)
REC · PBONDblg						29.628
						(0.009)
REC · PBONDnrt						−23.645
						(0.322)
FIRM CONTROLS						
LAGE	−5.401	−4.922	−5.832	−5.401	−6.442	−5.777
	(0.008)	(0.016)	(0.002)	(0.003)	(0.001)	(0.002)
LSALES	−44.186	−41.951	−18.100	−16.152	−19.756	−16.946
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
TANGIBLES	−30.331	−29.449	−24.483	−24.111	−24.565	−23.463
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ADVERTISING	0.328	1.484	−4.591	−3.487	−5.467	−3.947
	(0.988)	(0.946)	(0.829)	(0.870)	(0.794)	(0.852)
R&D	−0.459	−0.488	−0.135	−0.165	−0.104	−0.148
	(0.570)	(0.549)	(0.864)	(0.835)	(0.894)	(0.850)

(*continued*)

Table V—*Continued*

Variables	1	2	3	4	5	6
MKTBOOK	−5.965	−5.713	−4.566	−4.388	−4.692	−4.425
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
PROFMARGIN	−0.238	−0.242	−0.130	−0.135	−0.130	−0.135
	(0.107)	(0.105)	(0.372)	(0.354)	(0.368)	(0.350)
LINTCOVERAGE	−32.260	−32.180	−27.777	−27.602	−27.593	−27.533
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
NWCDEBT	0.002	0.001	0.003	0.002	0.003	0.003
	(0.327)	(0.355)	(0.030)	(0.032)	(0.024)	(0.028)
LEVERAGE	26.999	23.559	35.245	33.076	34.144	30.389
	(0.002)	(0.006)	(0.000)	(0.000)	(0.000)	(0.001)
ZSCORE	−0.014	−0.014	−0.013	−0.013	−0.013	−0.013
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
LOAN CONTROLS						
LTIMRPBOND			5.027	3.692	3.552	2.758
			(0.000)	(0.007)	(0.008)	(0.034)
CORPURPOSES			−21.801	−22.886	−21.570	−22.756
			(0.000)	(0.000)	(0.000)	(0.000)
REFINANCE			−16.914	−18.050	−16.338	−17.995
			(0.000)	(0.000)	(0.000)	(0.000)
TAKEOVER			3.161	2.154	3.479	2.064
			(0.511)	(0.655)	(0.470)	(0.668)
WORKCAPITAL			−21.380	−22.836	−20.522	−22.230
			(0.000)	(0.000)	(0.000)	(0.000)
CREDITLINE			−21.261	−20.712	−21.255	−20.101
			(0.000)	(0.000)	(0.000)	(0.001)
TERMLOAN			15.787	15.948	16.164	16.613
			(0.011)	(0.009)	(0.010)	(0.008)
BRIDGELOAN			98.385	96.776	99.051	97.905
			(0.000)	(0.000)	(0.000)	(0.000)
LLOANAMT			−21.543	−21.732	−21.712	−21.874
			(0.000)	(0.000)	(0.000)	(0.000)
DIVRESTRICT			17.674	16.865	18.333	16.898
			(0.000)	(0.000)	(0.000)	(0.000)
SENIOR			−16.629	−14.841	−16.423	−15.137
			(0.010)	(0.022)	(0.011)	(0.020)
SECURED			60.307	59.983	60.975	59.352
			(0.000)	(0.000)	(0.000)	(0.000)
RENEWAL			−6.241	−7.066	−5.965	−6.714
			(0.442)	(0.383)	(0.461)	(0.403)
GUARANTOR			15.377	15.402	16.672	15.988
			(0.120)	(0.119)	(0.091)	(0.107)
SPONSOR			52.686	49.984	52.783	50.132
			(0.000)	(0.000)	(0.000)	(0.000)
LLENDERS			−2.872	−3.058	−3.220	−3.109
			(0.232)	(0.203)	(0.187)	(0.201)
SINGLENDER			6.258	6.069	5.767	6.011
			(0.135)	(0.146)	(0.169)	(0.150)
LMATURITY			−18.808	−20.096	−18.275	−19.924
			(0.000)	(0.000)	(0.000)	(0.000)
Adjusted-R^2	0.3625	0.3658	0.4626	0.4652	0.4609	0.4649
# observations	13,846	13,846	13,846	13,846	13,846	13,846

continues to reduce spreads significantly, the magnitude of the decrease is much smaller now: The coefficient on the recent public bond market access dummy is now only minus 17 basis points, versus minus 95 in Model 1. This suggests that much of the coefficient in Model 1 is due to differences in risk between firms with public bond market access and those without. By contrast, the recession dummy and the interaction between recessions and public bond market access have coefficients of roughly the same magnitude as before. Recessions raise spreads by 33 basis points, versus 28 basis points in Model 7 of Table IV. In a recession, loan spreads for firms with public bond market access rise by 22 basis points less than those of firms without such access, versus 32 basis points in Model 7 of Table IV. These results are consistent with Hypotheses 1, 2, and 3.

The coefficients for the control variables are generally consistent with the discussion given in the Data and Methodology section. Older firms, larger firms, and firms with more tangible assets pay significantly lower spreads. The variables proxying for intangible assets have insignificant effects, though advertising's sign is in the expected direction. The market-to-book ratio comes in strongly negative; as discussed earlier, this probably reflects the role of greater market value in cushioning the risk of debt. Most of the proxies for default risk—profit margin, interest coverage, leverage, and Z-score—have their expected signs, and all but profit margin are strongly significant.

In Model 2, we decompose the public bond market access variable by adding two additional dummies. Again, *MRPBONDblg* takes on the value one if the firm has most recently issued a public bond that was below investment grade, and *MRPBONDnrt* takes on the value one if the firm has most recently issued a public bond that was not rated (or the rating was missing in the SDC database). With the addition of these variables, the coefficient on the initial bond market access dummy (*MRPBOND*) represents the impact of recent public issues of *investment-grade* bonds; to get the full impact of having publicly issued below-grade or unrated bonds, one must add the coefficient of the appropriate new dummy to that of the initial dummy, and similarly for the appropriate interaction terms.

The results are largely what one would expect. As before, recessions have a significantly positive impact on spreads. Firms whose most recent bond issue was public and investment-grade have spreads 37 basis points below those of bank-dependent firms, firms with public below-grade bonds have equivalent spreads to bank-dependent firms (a total impact of zero basis points), and firms with public unrated bonds have somewhat higher spreads (total impact of plus 7 basis points). The interaction of the recession dummy with the investment-grade bond access dummy is still significantly negative, but only minus 16 basis points now and with a p-value of 0.04. The additional impact in a recession of having recently issued below-grade or unrated debt is not statistically different from zero, and, even taking these coefficients at face value, the net impact (coefficient on the investment-grade dummy interaction with recession plus that on the appropriate credit rating interaction with recession) remains negative.

In Model 3, we augment the firm-level controls of Model 1 with the loan-specific controls. The basic thrust of the results is unchanged. The direct impact of public bond market access more than doubles to minus 38 basis points, though the impact of a recession falls to 22 basis points and the interaction term is largely unchanged at minus 20 basis points. The larger coefficient on bond market access suggests that there are significant differences in the loan terms between loans to firms with market access and those to firms that are bank-dependent. The weaker effect of a recession may reflect the fact that, in recessions, lenders tighten nonprice terms, muting the impact on spread. On a related note, one new control is the log of one plus the time since the firm's last public bond issue (*LTIMRPBOND*). This has a significantly positive effect, which is consistent with the notion that, the less recently the firm has issued public bonds, the lower the likelihood that it still has such access.

Looking at the other new controls, among the loan-purpose variables takeovers have an insignificant impact on spreads, whereas other purposes have negative impacts of varying sizes. Among loan types, credit lines have lower spreads than term loans, which in turn are not nearly as risky as bridge loans. Larger loans have lower spreads. This could reflect economies of scale in loan size, but it may also reflect the fact that larger (and hence safer) firms have larger loans, which is consistent with the decrease in the coefficient on the log of firm sales.

With the exception of seniority, loan features that aim to increase loan safety (dividend restrictions, secured interests, guarantors, and sponsors) generally have positive effects on spreads. This is consistent with the well-established result that banks tend to require these features for riskier credits. Conversely, longer-term loans have lower spreads, reflecting lenders' preference for lending on a long-term basis only to safer credits. As noted above, many of these loan features are clearly endogenous, that is, lenders set them in tandem with the spread. The number of banks in the loan syndicate does not appear to have an effect on the loan spread once we control for our set of loan features. Renewals have an insignificant negative impact on spreads, which probably reflects the large number of missing observations noted earlier.

Model 4 adds the breakdowns of bond access by credit rating to the augmented list of controls. The results are broadly similar to those of Model 2. The impact of public bond market access is somewhat larger for investment grade bonds, and the net impact is now negative for both below-grade and unrated borrowers (minus 14 and minus 13 basis points, respectively). As in Model 2, the impact of a recession is slightly weaker (plus 22 basis points), but the impact of the interaction terms is roughly the same.

Thus far we have tested our hypotheses assuming that firms have access to the bond market if their most recent bond issue prior to the loan was a public bond. Models 5 and 6 of Table V investigate how our results are affected if instead we classify firms as having access to the public bond market if they have ever issued public bonds prior to the date of the loan. The results are generally consistent with our hypotheses, but weaker than the other results in Table V. This is consistent with our view that recent bond issuance is a better measure of current access to the bond market. In this respect, it is interesting to note that the coefficient on the interaction term for below-grade is significantly positive, causing a net positive impact. Since we do not find this when we look at the most recent bond issue as a measure of bond market access, this suggests that below-grade bond issuers are more likely to be shut out of the public bond market in a recession if they have not issued there recently.

Overall, our results suggest that, during recessions, banks increase lending rates for bank-dependent firms more than the rates for firms with public bond market access—even if the access is to below-grade or unrated bonds. This is consistent with our model, which links this change to an increase in banks' informational power when firms have more risk of failure. In general, rates to bank-dependent firms are higher than those for firms with public bond market access, but this difference is largely driven by investment-grade firms. Banks also increase their lending rates in recessions by more than is explained by our controls. In the next section, we will subject these results to a number of robustness tests.

IV. Robustness Issues

Although our results in the previous section support our hypotheses, a number of potential objections can be raised. One possible explanation for our results is that bank-dependent firms that get loans are riskier in recessions than in expansions, whereas firms with bond market access that get loans are safer in recessions than in expansions. As discussed in the introduction, we address this possibility in two ways: first, through difference-in-differences tests, and second, by examining the impact of other risk proxies when interacted with bond market access. Another concern is that we have been treating bond market access as an exogenous variable, but in reality it is endogenous. We address this through a two-step procedure. We also explore whether including additional risk factors alters our results. Finally, we perform several other robustness tests. For example, we investigate if our results continue to hold when we account for firms' lending relationships, when we change our sample definition, and when we include firm fixed effects. As we will see, our results are robust to all of these concerns.

A. Selection Bias

As noted earlier, our results may be subject to a subtle form of selection bias. Suppose that those bank-dependent firms that seek loans during recessions are riskier on average than those that seek loans in expansions, whereas the opposite is true for firms with bond market access. If this form of selection holds true, then it would explain why bank-dependent firms pay relatively more in recessions than firms with bond market access.

We address this possibility in two ways. First, we directly examine whether key proxies for firm risk and quality vary across business conditions in different ways depending on whether the firm is bank-dependent or not. To do so, we first limit our sample to firms that do borrow in recessions, so we can see whether their risk changes in a significant way over the business cycle. We then construct difference-in-differences tests for each of the following seven variables: fraction of assets that are tangible, interest coverage, leverage, Z-score, stock return volatility, sales growth, and market-to-book ratio. For each of these variables, we compute mean values for bank-dependent and non-bank-dependent firms in expansions and in recessions. We then compute the difference between each firm type's mean in expansions and in recessions, and investigate whether the difference between these two differences is significant. The results (available on request) show that none of these difference-in-differences are significant. However, there are signs that a few outliers may be affecting some of these results. When we repeat the analysis with the data Winsorized at the 1% level, the difference-in-differences for stock volatility becomes weakly significant at the 10% level.[14] Since only one of seven tests is significant, and only at the 10% level, this suggests that business cycle selection effects in observable risk proxies are not a key driver of our results. As we will see later in this section, even when we include stock volatility (the variable for which the difference-in-differences test is significant) as an explanatory variable, our results do not change.[15]

Of course, there may still be selection along some unobservable dimension of risk. Our second method for dealing with this is to note that the prediction that hold-up effects should be greater for firms with higher risk can be applied to other measures of risk besides recessions.[16] For example, if we interact a measure of risk based on leverage with our bond market access dummy, this term should have a negative coefficient in our regression: Firms with high leverage that are bank-dependent pay higher rates than firms with high leverage that have market access. Moreover, this type of regression sidesteps the issue of differential selection during recessions.

Accordingly, for each of five risk proxies (tangible assets, leverage, Z-score, interest coverage, and stock volatility), we construct dummy variables that equal one if the measure in question is riskier than the median value. We then regress loan spreads on our usual set of firm- and loan-specific controls, along with four variables of interest: the bond market access dummy, the risk proxy dummy just described, the interaction of bond market access and the risk proxy dummy, and the recession dummy. If our hold-up model is correct, we should observe negative coefficients on bond market access and the interaction term, and positive coefficients on the risk proxy and recession dummies. The results are given in Table VI.

[14] To make sure the outliers are not driving our earlier results, we rerun our multivariate tests on the Winsorized data. Hypotheses 1 through 3 continue to hold. We also rerun our multivariate tests on the subsample of firms that borrow in recessions; again, Hypotheses 1 through 3 still hold.

[15] Our results are also unchanged when we add the firm's stock return over the previous year as another stock market-related explanatory variable.

[16] As a further test, we subdivide our sample into riskier and safer firms based on each of five risk proxies: tangible assets, leverage, Z-score, interest coverage, and stock volatility. We then rerun the analogue of Model 3 in Table V for each subsample—that is, we regress loan spread on bond market access, the recession dummy, and their interaction, along with our standard list of firm- and loan-specific controls. Generally speaking, Hypotheses 1 through 3 continue to hold, and the relevant coefficients are larger for the riskier subsamples. (Results available on request.)

Table VI

Loan Spreads for Bank-Dependent and Nondependent Borrowers: Interaction of Bond Market Access and Firm Risk Measures

The dependent variable is LOANSPREAD. FIRM CONTROLS and LOAN CONTROLS coincide with the set of firm controls and loan controls, respectively, used in Table IV. See Table I for variable definitions. Included in the regressions but not shown in the table are also dummy variables for the issuer's sector of activity as defined by one-digit SIC code. Models are estimated with robust standard errors and clustered by firm to correct for correlation across observations of a given firm. Values in parentheses are p-values.

Variables	1	2	3	4	5
CONSTANT	604.140	631.419	622.366	547.534	492.724
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
MRPBOND	−42.114	−34.970	−34.331	−23.829	−36.999
	(0.000)	(0.000)	(0.000)	(0.005)	(0.000)
TANGIBLES-B50	11.000				
	(0.001)				
MRPBOND · TANGIBLES-B50	−0.560				
	(0.927)				
LEVERAGE-A50		13.704			
		(0.000)			
MRPBOND · LEVERAGE-A50		−9.987			
		(0.000)			
ZSCORE-B50			6.905		
			(0.039)		
MRPBOND · ZSCORE-B50			−14.156		
			(0.018)		
INTCOVERAGE-B50				66.269	
				(0.000)	
MRPBOND · INTCOVERAGE-B50				−24.691	
				(0.000)	
STOCKVOL-A50					68.304
					(0.000)
MRPBOND · STOCKVOL-A50					13.846
					(0.120)
REC	18.087	17.801	17.820	17.548	10.195
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
FIRM CONTROLS	IN	IN	IN	IN	IN
LOAN CONTROLS	IN	IN	IN	IN	IN
Adjusted-R^2	0.4616	0.4610	0.4624	0.4582	0.5006
# observations	13,846	13,846	13,846	13,846	12141

Again, the results are generally consistent with our predictions. The coefficient on bond market access is always negative and significant, whereas the coefficient for the risk proxy dummy is always positive and significant. The coefficient on the interaction term is negative and significant in three cases (leverage, Z-score, and interest coverage), negative and insignificant in one case (tangible assets), and positive and insignificant in one case (stock volatility).[17] The recession dummy continues to have a positive and significant impact in all cases. In the one case (Z-score) where the sum of the interaction coefficient and the risk dummy is negative, the sum of the coefficients is not significantly different from zero.

B. Endogenous Access to Bond Markets

It is possible that our results are driven by unobservable risk factors that affect both loan spreads and access to public bond markets. To deal with this possibility, we use a two-stage procedure. In the first stage, we estimate a probit model of the determinants of recent access to public bond markets (*MRPBOND*). In the second stage, we use the predicted value of recent bond market access in our loan spread regression. We can view the first stage as constructing an alternative proxy for information availability. Alternatively, our two-stage procedure would be equivalent to an instrumental variables approach if our additional first-stage variables are uncorrelated with loan spreads once bond market access is accounted for.[18] As we will see, if anything, our results in the two-stage procedure are more striking than those from the previous section.

Clearly, a number of the firm-level factors used to explain spreads are also likely to be important in the first stage. Older, and hence better-established, firms and larger firms are more likely to be well known, improving access to public bonds markets, so we include our variables for firm age and sales. Firms with higher profit margins, more tangible assets, and greater interest coverage may be more attractive to bondholders, whereas firms with more intangible assets or greater risk of default may be less attractive, so we include the relevant variables from our list of firm-level controls. Again, some of these determinants (such as the market-to-book ratio) may have an ambiguous sign, for reasons similar to those discussed in the Data and Methodology section.

We also include variables that are not included in the second-stage regression. Two of the variables we use relate to the firm's visibility through inclusion in well-known groups: the S&P 500 Index (*S&P500*) and the New York Stock Exchange (*NYSE*). Inclusion in either should boost the firm's visibility to the financial community, making it less costly to issue bonds to the investing public. For similar reasons, following Faulkender and Petersen (2006), we include a dummy variable that equals one if the firm's bond issues exceed the minimum amount required to be in Lehman Brothers Corporate Bond Index (*LEHMAN*). Although all of these variables tend to correlate with size, note that size itself is included as a separate regressor. Our goal is to try to pick up effects that are

[17] The lack of significance for the stock volatility interaction dummy may reflect the nature of the risk involved. Although some stock volatility does translate to greater risk of default, much of the volatility may involve future growth opportunities. Since debt levels are typically tied to current cash flows and tangible assets, loss of growth opportunities would not necessarily cause default. We are grateful to Mitchell Petersen for raising this point.

[18] We are grateful to the referee for pointing out this clarification.

incremental to firm size, that is, for a given size, does inclusion on the NYSE favor bond market access?

In addition to depending on the visibility of the individual firm, access to bond markets may depend on industry visibility. If a firm is in an industry where many competitors have already issued bonds to the public, investors may find it easier to evaluate any bonds that this firm issues simply because they already have experience in assessing this industry. Accordingly, we include the percentage of firms in the given firm's two-digit SIC code industry that either have issued their most recent bonds publicly (*INDACCESSmrpbond*) or have ever issued public bonds (*INDACCESSpbond*). Finally, we include a dummy (*OLDERFIRMS*) that equals one if the firm is at least 21 years old, which is the median age of firms that have publicly issued bonds. Here, the goal is to pick up nonlinear effects of firm age.

Table VII displays the first-stage results. The controls for firm age and sales all have the expected positive signs and are strongly significant, as is the market-to-book ratio. Interest coverage and leverage are strongly significant but have opposite effects from what one might expect: Firms with higher interest coverage are less likely, and firms with higher leverage more likely, to have public bond market access. These findings are consistent with Faulkender and Petersen (2006), who find that, all else equal, firms with public bond market access have higher leverage than those without such access. As they suggest, this may reflect a relaxation of credit constraints for firms that are not bank-dependent.

All of the new variables have the expected positive signs, and most—inclusion in the S&P, NYSE, or Lehman indexes, and percentage of industry firms whose most recent bond issue was public—are strongly significant. The exceptions are the dummy for firm age over 21 years and the percentage of industry firms that have ever issued a public bond, which are statistically insignificant. Also, a chi-squared test soundly rejects the null hypothesis that the coefficients of the new variables are jointly equal to zero.

The second-stage regression results are shown in Table VIII. The six models correspond to the six models from Table VII. In all cases, we also include the firm-level controls but not the loan-specific controls. The results generally support the three hypotheses of our paper. The coefficient on the predicted value of recent bond market access is negative and both economically and statistically significant in all versions. The coefficient on the recession dummy is positive and at the high end of the range of estimates from Table V, at roughly 32 basis points. Finally, the coefficient on the interaction between the recession dummy and the predicted value of recent bond market access is always significantly negative, with estimates ranging from 22 to 23 basis points.

Although it is possible that some of our additional variables correlate directly with unobserved risk factors that affect loan spreads, this is hard to argue in most of the cases. Inclusion in the S&P, NYSE, or Lehman Brothers Index probably correlates with size and age, but these are included in the second-stage regression. One might postulate that these variables reflect some impact of extremely large size on risk, but the fact that all three have positive and

Table VII

Determinants of Firms' Access to the Bond Market: First Stage of a Two-Stage Procedure for Market Access

The dependent variable is MRPBOND. See Table 1 for variable definitions. Probit models are estimated with robust standard errors and clustered by firm to correct for correlation across observations of a given firm. Included in the regressions but not shown in the table are also dummy variables for the issuer's sector of activity as defined by one-digit SIC code. Values in parentheses are p-values.

Variables	1	2	3	4	5	6
CONSTANT	−2.390	−2.340	−2.371	−2.365	−2.417	−2.413
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
NYSE	0.507	0.506	0.493	0.499	0.494	0.501
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
LEHMAN	0.982	0.981	0.982	0.979	0.982	0.979
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
S&P500	0.357	0.354	0.367	0.366	0.370	0.369
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
OLDERFIRMS		0.067	0.061	0.063		
		(0.552)	(0.589)	(0.580)		
INDACCESSmrpbond			0.976		0.983	
			(0.031)		(0.030)	
INDACCESSpbond				0.457		0.464
				(0.215)		(0.206)
FIRM CONTROLS						
LAGE	0.194	0.165	0.160	0.163	0.186	0.190
	(0.000)	(0.004)	(0.005)	(0.005)	(0.000)	(0.000)
LSALES	0.232	0.231	0.220	0.223	0.221	0.224
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
TANGIBLES	0.141	0.140	0.054	0.090	0.054	0.090
	(0.264)	(0.266)	(0.669)	(0.476)	(0.668)	(0.476)
ADVERTISING	0.770	0.763	0.703	0.743	0.710	0.750
	(0.206)	(0.208)	(0.252)	(0.222)	(0.249)	(0.219)
R&D	−1.535	−1.502	−1.410	−1.428	−1.439	−1.458
	(0.078)	(0.082)	(0.092)	(0.092)	(0.088)	(0.088)
MKTBOOK	0.034	0.034	0.034	0.034	0.035	0.034
	(0.009)	(0.009)	(0.007)	(0.007)	(0.007)	(0.007)
PROFMARGIN	0.008	0.008	0.008	0.008	0.007	0.008
	(0.489)	(0.496)	(0.463)	(0.482)	(0.452)	(0.472)
LINTCOVERAGE	−0.100	−0.098	−0.096	−0.096	−0.097	−0.097
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
NWCDEBT	−0.000	−0.000	−0.000	−0.000	−0.000	−0.000
	(0.837)	(0.871)	(0.818)	(0.840)	(0.786)	(0.808)
LEVERAGE	0.382	0.387	0.377	0.383	0.372	0.377
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
ZSCORE	0.000	0.000	0.000	0.000	0.000	0.000
	(0.373)	(0.376)	(0.396)	(0.393)	(0.394)	(0.391)
Pseudo-R^2	0.3854	0.3855	0.3867	0.3859	0.3866	0.3858
P-value for H1: ($\beta = 0$)	0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
# Observations	13,846	13,846	13,846	13,846	13,846	13,846

Table VIII

Loan Spreads for Bank-Dependent and Nondependent Borrowers: Second Stage of a Two-Stage Procedure for Market Access

The dependent variable is LOANSPREAD. The models in this table are the second stage of a two-stage procedure for bond market access. For the first stage models, including the additional variables used in each model to determine access to the bond market, see Table VII. $\widehat{MRPBOND}$: Fitted values for MRPBOND are computed based on models in Table VII. See Table I for variable definitions. Included in the regressions but not shown in the table are also dummy variables for the issuer's sector of activity as defined by one-digit SIC code. Models are estimated with robust standard errors and clustered by firm to correct for correlation across observations of a given firm. Values in parentheses are p-values.

Variables	1	2	3	4	5	6
CONSTANT	341.540	341.449	341.240	341.385	341.322	341.469
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
$\widehat{MRPBOND}$	−33.059	−34.207	−35.996	−34.437	−34.953	−33.364
	(0.010)	(0.007)	(0.005)	(0.008)	(0.007)	(0.010)
REC	32.230	32.213	32.411	32.363	32.427	32.380
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
REC · $\widehat{MRPBOND}$	−21.715	−21.843	−23.095	−22.589	−22.985	−22.479
	(0.052)	(0.050)	(0.040)	(0.044)	(0.041)	(0.046)
FIRM CONTROLS						
LAGE	−4.624	−4.566	−4.463	−4.546	−4.516	−4.601
	(0.028)	(0.030)	(0.034)	(0.031)	(0.032)	(0.029)
LSALES	−41.829	−41.653	−41.346	−41.596	−41.506	−41.760
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
TANGIBLES	−30.054	−30.034	−30.005	−30.032	−30.023	−30.050
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ADVERTISING	1.213	1.279	1.387	1.296	1.327	1.235
	(0.956)	(0.954)	(0.950)	(0.953)	(0.952)	(0.956)
R&D	−0.487	−0.490	−0.493	−0.490	−0.491	−0.488
	(0.552)	(0.550)	(0.548)	(0.550)	(0.549)	(0.551)
MKTBOOK	−5.828	−5.818	−5.799	−5.814	−5.809	−5.824
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
PROFMARGIN	−0.237	−0.237	−0.237	−0.237	−0.237	−0.237
	(0.111)	(0.111)	(0.112)	(0.111)	(0.111)	(0.111)
LINTCOVERAGE	−32.499	−32.516	−32.542	−32.519	−32.526	−32.503
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
NWCDEBT	0.002	0.001	0.001	0.001	0.001	0.001
	(0.356)	(0.358)	(0.364)	(0.360)	(0.362)	(0.358)
LEVERAGE	28.575	28.693	28.895	28.729	28.788	28.619
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
ZSCORE	−0.014	−0.014	−0.014	−0.014	−0.014	−0.014
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Adjusted R^2	0.361	0.361	0.360	0.361	0.361	0.361
# Observations	13,846	13,846	13,846	13,846	13,846	13,846

significant impacts makes this unlikely. Furthermore, although inclusion in these indexes increases visibility, it is difficult to see why it would have any direct effect on risk other than by making access to financial markets easier, which is the point we want to focus on. The only variable that might proxy for unobserved risk is the fraction of firms in the industry that have issued public bonds. Conceivably, industries with more such firms may be less risky, which might affect spreads directly in ways we have not controlled for. This should be offset at least in part by our use of industry dummies, albeit at the one-digit SIC code level.

C. Accounting for Firm Risk

As further tests of robustness, we determine whether our results are driven by risk and performance factors that were not included in our initial specifications. As we will see, although these additional factors are often significant determinants of loan spreads, they do not alter our core results.

These tests are presented in Tables IX and X. All tests are modifications of Models 3 and 4 of Table V; these include both firm- and loan-specific controls, but for brevity, we do not report the coefficients on these controls. Recall that the difference between Models 3 and 4 is that the latter accounts for the credit rating of the firm's last public bond prior to the loan.

Our first test adds the firm's sales growth over the previous year as a measure of performance. Arguably, firms with higher sales growth may be stronger firms. The results of including this variable are given in Models 1 and 2 of Table IX. As can be seen, sales growth is not significant, and our main hypotheses continue to be supported.

Our second set of tests adds stock market variables as measures of risk and performance. Models 3 and 4 of Table IX add the firm's percentage stock price volatility over the previous year. It has a significant positive impact on spread, as one would expect. In Models 5 and 6, we also add the firm's excess stock return over the previous year as a measure of performance. It is significant at the 10% level; higher stock returns lead to lower spreads, which is consistent with higher stock returns reducing firm risk, all else equal. More importantly, the coefficients on bond market access, the recession dummy, and their interaction are statistically significant and consistent with Hypotheses 1 through 3.

Our third test attempts to control for expected changes in the short-term interest rates and for the price of risk in the bond market. Toward this end we add to our core models the slope of the Treasury yield curve as defined by the difference between the 30- and 5-year Treasury bonds, and the spread of new BBB industrial bonds over new AAA industrial bonds. Both of these spreads are measured at the time of the loan. The results of this test are reported in Models 7 and 8 of Table IX. The coefficients on the new controls are positive and statistically significant. Interestingly, the coefficient on the recession dummy falls to 12 or 13 basis points, suggesting that the BBB spread accounts for some of the increase in loan spreads during recessions. Nevertheless, the results on our three terms of interest are consistent with Hypotheses 1 through 3. Again, that the coefficient on the recession dummy is smaller than that on the interaction term may be due to the problems in cleanly identifying Hypothesis 2: Recessions correlate with the other risk factors included in the regression. Also, the sum of the coefficients is not statistically different from zero.

Table IX

Loan Spreads for Bank-Dependent and Nondependent Borrowers: Robustness Checks: Additional Risk Controls

The dependent variable is LOANSPREAD. Models 1 and 2 control additionally for SALESGROWTH. Models 3 and 4 control additionally for STOCKVOL. Models 5 and 6 further add to Models 3 and 4 STOCKRET. Models 7 and 8 control additionally for TSPREAD, and for BBBSPREAD. Models 9 and 10 control additionally for LIBOR. Models 11 and 12 use the NBER identification of peaks and troughs to identify the recessions in the American economy as opposed to the Stock–Watson Experimental Coincident Recession Index, which is used in all of the other models. Based on the new criteria, REC equals one for the loans taken out between August 1990 and March 1991, and between April 2001 and November 2001. FIRM CONTROLS and LOAN CONTROLS coincide with the set of firm controls and loan controls, respectively, used in Table V. See Table I for variable definitions. Included in the regressions but not shown in the table are also dummy variables for the issuer's sector of activity as defined by one-digit SIC code. Models are estimated with robust standard errors and clustered by firm to correct for correlation across observations of a given firm. Values in parentheses are p-values.

Variables	1	2	3	4	5	6	7	8	9	10	11	12
CONSTANT	635.821	636.958	513.911	512.543	509.745	508.020	584.911	585.192	632.787	634.598	631.866	633.216
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
MRPBOND	−37.385	−47.389	−31.726	−39.120	−31.873	−39.631	−34.069	−45.143	−38.339	−48.971	−41.195	−51.169
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
MRPBONDblg		33.319		28.213		29.499		36.216		34.871		34.885
		(0.000)		(0.001)		(0.001)		(0.000)		(0.000)		(0.000)
MRPBONDnrt		35.838		24.049		24.281		38.045		35.651		32.590
		(0.008)		(0.033)		(0.031)		(0.004)		(0.008)		(0.009)
REC	22.558	21.859	17.724	17.066	18.218	17.565	12.572	11.660	22.494	21.785	14.613	13.808
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.004)	(0.000)	(0.000)	(0.003)	(0.006)
REC · MRPBOND	−20.882	−18.651	−20.840	−18.493	−20.589	−17.826	−20.004	−15.235	−21.092	−17.847	−21.317	−18.809
	(0.000)	(0.003)	(0.000)	(0.003)	(0.000)	(0.005)	(0.001)	(0.019)	(0.000)	(0.005)	(0.002)	(0.008)
REC · MRPBONDblg		12.658		10.796		9.566		5.531		9.845		15.772
		(0.245)		(0.370)		(0.428)		(0.614)		(0.374)		(0.300)

(*continued*)

Table IX—*Continued*

Variables	1	2	3	4	5	6	7	8	9	10	11	12
REC · MRPBONDnrt		−9.948		−11.482		−12.595		−15.968		10.398		12.805
		(0.713)		(0.619)		(0.585)		(0.561)		(0.701)		(0.734)
SALESGROWTH	0.001	0.002										
	(0.837)	(0.666)										
STOCKVOL			9.371	9.389	9.694	9.731						
			(0.000)	(0.000)	(0.000)	(0.000)						
STOCKRET					−0.057	−0.060						
					(0.090)	(0.072)						
TSPREAD							11.420	11.432				
							(0.000)	(0.000)				
BBBSPREAD							41.981	42.794				
							(0.000)	(0.000)				
LIBOR									−1.235	−1.429		
									(0.131)	(0.078)		
FIRM CONTROLS	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN
LOAN CONTROLS	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN
Adjusted-R^2	0.4645	0.4671	0.4950	0.4969	0.4960	0.4980	0.4682	0.4709	0.4628	0.4655	0.4603	0.4630
# Observations	13,846	13,846	10,201	10,201	10,201	10,201	13,846	13,846	13,846	13,846	13,846	13,846

Our fourth test attempts to control for changes in the level of the interest rate used to compute the loan spreads. Toward this end we add to the core models the level of this interest rate, *LIBOR*. The results of this test are reported in Models 9 and 10 of Table IX. Note that there is a negative relationship between loan spreads and *LIBOR*. However, this variable is only significant in Model 6, and even there its p-value is only 0.08. More importantly, as with the previous tests, the results on our three variables of interest are consistent with Hypotheses 1 through 3.

Our fifth test investigates the impact of the way we control for the state of the economy. All the results presented so far are based on the recession dummy created from the Stock–Watson Coincidental Recession Index. Recall that we use this because it represents information that banks have at the time they are making loan pricing decisions. Nevertheless, as a test of robustness, in Models 11 and 12 of Table IX we use a recession dummy based on NBER's (backward-looking) definitions of business cycle peaks and troughs.[19] Once again, the results are consistent with our three hypotheses. The coefficient on the recession dummy (14–15 basis points) is again smaller in magnitude than that on the interaction term (minus 19–21 basis points), but here the backward-looking recession dummy is probably even more positively correlated with the other risk factors included in the regression. In any case, the sum of the coefficients is not statistically different from zero.[20]

Our sixth test investigates the impact of potential changes in firms' risk over the business cycle. Toward this end we added to our core models the interaction of our recession dummy with the firm's leverage and with its Z-score; arguably, firms with higher leverage or Z-score face relatively greater risk during recessions than during other times. In the interest of space and because the second interaction is not statistically significant, Models 1 and 2 of Table X report our results with only the first interaction. This new variable is positive and statistically significant, suggesting that the impact of leverage on spreads is heightened during downturns. As a result, the coefficient on the recession dummy is smaller and (in Model 2) just insignificant at the 10% level. Nevertheless, the sum of the coefficients on the recession dummy and the interaction term is not statistically different from zero. The coefficients on bond market access and on the interaction of access and the recession dummy continue to support Hypotheses 1 and 3.

Our final test for the impact of risk concerns the role of firm size. It is possible that all of our results are driven by the smallest firms, which are overwhelmingly bank-dependent; that is, the differential impact of bank-dependence is driven by (nonlinear) risk differences between these very small firms and the typically much larger firms with bond market access. To control for this, we

[19] Based on this definition there are two recessions during our sample period. The first recession starts in August of 1990 and ends in March of 1991. The second recession starts in April of 2001 and ends in November of 2001.

[20] In an earlier version of this paper we also ran regressions where we used the Stock–Watson Index itself rather than the recession dummy created from it. Again, the results supported our three hypotheses.

reestimate our results after dropping the smallest quartile of firms ranked by sales (dropping the smallest quartile of firms by assets produces similar results). The results are shown in Models 3 and 4 of Table X. Once again, the results continue to support Hypotheses 1 through 3.

D. Controlling for Lending Relationships

As a further test, Models 5 through 8 of Table X investigate the robustness of our findings when we account for firms' bank lending relationships. We assume that a firm has a relationship with a bank if it borrowed from it at least once over the last 3 years prior to the loan. We measure a firm's lending relationships with banks in two alternative ways. Under our first definition, *RLENDINGnarrow*, we consider the firm's relationship with only the banks that act as lead underwriters for the loan syndicates that lent to the firm. Under the second definition, *RLENDINGbroad*, we consider the firm's relationship with all the banks that participated in the syndicates in a lending capacity to the firm. The results of controlling for the first measure of a firm's bank lending relationships are reported in Models 5 and 6 of Table X; the results of controlling for the second measure are reported in Models 7 and 8. Our findings show that firms that borrow repeatedly from the same bank pay roughly 6 basis points less on their loans, irrespective of the way we measure bank lending relationships. Nevertheless, the results on bond market access, the recession dummy, and their interaction continue to support Hypotheses 1 through 3.

Based on Rajan's (1992) model, we would expect that relationship borrowing is less attractive during recessions, when the information hold-up problem should be stronger.[21] To test for this, Models 9 through 12 of Table X add interaction terms between the relationship dummy (first narrow, then broad) and the recession dummy. As expected, the interaction terms have positive and significant coefficients that are even larger than the coefficients on the matching relationship terms. Although these results are consistent with the existence of hold-up problems, caution is needed since the choice of bank is endogenous.

E. Issues Involving Sample Definition

Our next set of tests has to do with the definition of our sample. As we now show, our results are robust to these concerns.

The first issue involves the existence of loan deals that give rise to multiple facilities. In some cases there are differences between these facilities; some have longer maturity than others, are associated with larger loans, or are provided by different sets of banks. In other cases, however, the multiple facilities of the same deal are similar. Thus far, we have treated each facility in the database as a different loan, but to the extent these loans are priced similarly, it might be more accurate to count each deal only once. To address this concern, we select (randomly) one facility from those deals with multiple facilities and retain the

[21] We are grateful to the referee for suggesting this test.

Table X

Loan Spreads for Bank-Dependent and Nondependent Borrowers: Robustness Checks: Risk in Recessions, Small Firms and Lending Relationships

The dependent variable is LOANSPREAD. Models 1 and 2 control for the interaction of REC with LEVERAGE. Models 3 and 4 are estimated on the sample of loans taken out by larger firms (excludes from the original sample all loans made taken out by the smaller firms defined by the first quartile of sales). Models 5 through 8 control for our two alternative measures of firms' lending relationships: RLENDINGnarrow and RLENDINGbroad, respectively. Models 9 through 12 investigate the effects of lending relationships in recessions by adding to Models 5 through 8 the interaction of REC with our two alternative measures of firms' lending relationships. FIRM CONTROLS and LOAN CONTROLS coincide with the set of firm controls and loan controls, respectively, used in Table V. See Table I for variable definitions. Included in the regressions but not shown in the table are also dummy variables for the issuer's sector of activity as defined by one-digit SIC code. Models are estimated with robust standard errors and clustered by firm to correct for correlation across observations of a given firm. Values in parentheses are p-values.

Variables	1	2	3	4	5	6	7	8	9	10	11	12
CONSTANT	629.954	630.880	564.388	562.371	629.780	630.791	632.345	633.338	629.665	630.810	632.489	633.733
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
MRPBOND	−37.922	−48.945	−39.687	−49.417	−38.158	−48.772	−38.216	−48.886	−37.509	−47.926	−37.110	−47.374
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
MRPBONDblg		35.875		32.827		34.729		34.882		34.424		34.216
		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)		(0.000)
MRPBONDnrt		35.771		28.680		35.678		35.609		35.399		11.647
		(0.007)		(0.028)		(0.007)		(0.007)		(0.008)		(0.009)
REC	11.023	9.859	28.049	27.296	22.509	21.807	22.537	21.833	16.863	16.115	12.474	11.647
	(0.064)	(0.102)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.004)	(0.007)
REC · MRPBOND	−21.171	−16.281	−24.507	−22.575	−20.402	−16.650	−20.638	−16.760	−22.605	−19.454	−24.897	−22.362
	(0.000)	(0.011)	(0.000)	(0.001)	(0.001)	(0.010)	(0.000)	(0.000)	(0.000)	(0.003)	(0.000)	(0.001)
REC · MRPBONDblg		3.433		12.261		8.417		7.922		9.964		11.941
		(0.763)		(0.282)		(0.443)		(0.470)		(0.363)		(0.277)

(*continued*)

Table X—*Continued*

Variables	1	2	3	4	5	6	7	8	9	10	11	12
REC · MRPBONDnrt		−12.506		−1.968		−12.584		−12.746		−7.601		−4.702
		(0.636)		(0.945)		(0.644)		(0.641)		(0.779)		(0.863)
RLENDINGnarrow					−5.164	−5.260			−8.877	−8.996		
					(0.035)	(0.031)			(0.001)	(0.001)		
RLENDINGbroad							−9.772	−9.843			−14.972	−15.094
							(0.000)	(0.000)			(0.000)	(0.000)
REC · RLENDINGnarrow									18.195	18.340		
									(0.003)	(0.003)		
REC · RLENDINGbroad											25.479	25.792
											(0.000)	(0.000)
LEVERAGE	30.124	27.708	24.628	22.279	35.952	33.792	36.848	34.878	36.004	33.857	36.931	34.779
	(0.002)	(0.004)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
REC · LEVERAGE	35.606	37.004										
	(0.043)	(0.038)										
FIRM CONTROLS	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN
LOAN CONTROLS	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN
Adjusted-R^2	0.4631	0.4657	0.4621	0.4654	0.4629	0.4655	0.4636	0.4662	0.4635	0.4661	0.4649	0.4675
# Observations	13,846	13,846	10,386	10,386	13,846	13,846	13,846	13,846	13,846	13,846	13,846	13,846

deals with a single facility. This reduces our sample from 13,846 loans to 9,857 loans. We then rerun our core loan pricing models (analogous to Models 3 and 4 of Table V) on the reduced sample. The results are reported as Models 1 and 2 in Table XI. Once again, the results continue to support Hypotheses 1 through 3.

Another concern about our sample is that, because Dealscan's coverage of the loan market improved markedly into the early 1990s, the loans from the 1980s may not be very representative. To address this concern, we drop all loans from 1987 to 1989, reducing our sample to 12,505 loans, and rerun our core loan pricing models on the loans from 1990 to 2002. As reported in Models 3 and 4 of Table XI, the results continue to support Hypotheses 1 through 3.

A final concern with our sample is that many of the loans in the Dealscan database are renegotiations of earlier loans. If these indicate troubled credits and thus greater risk, and if bank-dependent firms are more likely to renegotiate loans than firms with bond market access, this could bias our results in favor of finding higher spreads for bank-dependent firms. To address this concern, we drop all loans that are listed as amending or restating previous loans. This reduces our sample to 11,887 loans. We rerun our core loan pricing models on the reduced sample, as reported in Models 5 and 6 of Table XI. The results continue to support Hypotheses 1 through 3.

F. Firm Fixed Effects

One possible caveat to our results is that there may be unobservable firm-specific factors that influence loan spreads. One way to control for this is to use firm fixed effects. One difficulty with this approach is that bond market access is itself a firm-specific factor. Thus, with firm fixed effects, the coefficient on the market access dummy will only pick up the impact of changes in bond market access for any given firm over time, rather than cross-sectional differences. This is especially problematic for our sample, because its variation is largely cross-sectional: Most firms have only a few different observations, if more than one at all. Nevertheless, in Models 7 and 8 of Table XI we rerun our core regressions with firm fixed effects. It is immediately clear that the results continue to support our three hypotheses. In Model 7, the coefficient on bond market access is minus 43 basis points, that on the recession dummy is 26 basis points, and that on the interaction of the two is minus 18 points. When we add the credit rating breakdowns in Model 8, the results are basically the same; the base coefficients are almost identical, while the terms for below-investment grade or nonrated borrowers are insignificant and do not offset the base coefficients' signs.

G. Additional Robustness Checks

In the process of investigating the robustness of our results we consider additional robustness checks. For example, one possible concern with our results relates to possible errors in our measurement of recessions. Since the recession dummy is the same for all loans in a given quarter, such measurement errors

Table XI

Loan Spreads for Bank-Dependent and Non-Bank-Dependent Borrowers: Robustness Checks: Sample Definition, Firm Fixed Effects, and Simultaneous Clustering by Firm and Time

The dependent variable is LOANSPREAD. Models 1 through 6 are clustered by firm to correct for correlation across observations of a given firm. Models 7 and 8 are estimated with firm fixed effects. Models 1 and 2 are estimated on the sample made of one facility per loan deal. In the case of deals with multiple facilities, we select the facility for this sample randomly. Models 3 and 4 are estimated on the sample of loans taken out during the 1990–2002 time period (excludes from the original sample loans taken out in the 1987–1989 time period). Models 5 and 6 are estimated on the sample of nonrenegotiated loans (excludes from the original sample loan renegotiations). Models 7 and 8 are estimated with our original sample with robust standard errors and firm fixed effects. Models 9 and 10 are also estimated with our original sample, but are clustered both by firm and time (to correct for correlation across observations within each quarter). FIRM CONTROLS and LOAN CONTROLS coincide with the set of firm controls and loan controls, respectively, used in Table V. See Table 1 for variable definitions. Models estimated with robust standard errors. Included in the regressions but not shown in the table are also dummy variables for the issuer's sector of activity as defined by one-digit SIC code. Values in parentheses are p-values.

Variables	1	2	3	4	5	6	7	8	9	10
CONSTANT	649.596	651.060	635.660	635.739	623.770	624.740	400.542	400.555	628.031	629.020
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
MRPBOND	−38.111	−46.261	−30.657	−40.398	−31.959	−41.684	−42.737	−42.823	−38.073	−48.649
	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.002)	(0.000)	(0.000)
MRPBONDblg		33.297		31.674		32.704		−2.256		34.670
		(0.000)		(0.000)		(0.000)		(0.861)		(0.000)
MRPBONDnrt		39.209		23.621		40.433		14.821		35.484
		(0.001)		(0.026)		(0.008)		(0.501)		(0.025)
REC	20.350	19.704	24.328	23.619	22.395	21.773	26.180	26.209	22.492	21.790
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
REC · MRPBOND	−18.634	−15.600	−19.659	−15.959	−20.686	−18.902	−18.434	−14.064	−20.222	−16.483
	(0.001)	(0.006)	(0.001)	(0.013)	(0.001)	(0.005)	(0.008)	(0.056)	(0.004)	(0.036)
REC · MRPBONDblg		7.372		5.5678		17.832		−21.419		8.322
		(0.522)		(0.621)		(0.122)		(0.113)		(0.587)
REC · MRPBONDnrt		−8.554		−2.561		−14.521		3.579		−11.709
		(0.783)		(0.927)		(0.570)		(0.924)		(0.705)
FIRM CONTROLS	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN
LOAN CONTROLS	IN	IN	IN	IN	IN	IN	IN	IN	IN	IN
Adjusted-R^2	0.4688	0.4713	0.4769	0.4790	0.4700	0.4726	0.2788	0.2759	0.4626	0.4652
# Observations	9,857	9,857	12,505	12,505	11,887	11,887	13,846	13,846	13,846	13,846

would lead the error terms in our regression to be clustered by time as well as by firm. To address this issue, we follow Petersen (2006) and rerun our core regressions with clustering by time as well as by firm. The results of this test are reported in Models 9 and 10 of Table XI. Once again, our results continue to support Hypotheses 1 through 3.

Another potential concern is that the different types of loans (credit lines, term loans, etc.) may have very different pricing characteristics that are not captured by the additive specification we use. To control for this, we rerun our core models on the subsample of lines of credit in our data, which are the most numerous loan type (66%) in our sample. We find similar results to our earlier tests when we consider this subsample.

Yet another caveat derives from the quality of our data on advertising and R&D expenses. As we mentioned earlier, firms are required to report these variables only when they exceed a certain amount. Because of this criterion, this variable is often missing in Compustat. As we note earlier, in these cases we set both variables equal to zero. We rerun our core models dropping both variables from our list of controls and again find similar results on the variables of interest to test Hypotheses 1 through 3. This was somewhat expected given that both variables are usually not statistically significant.

Finally, as we noted earlier, one of the important loan controls in the LPC data set, the indication of whether the loan is secured, is missing for about 30% of the loans in our sample. In creating our secured dummy variable we set these missing observations to zero. In other words, we classify the loans for which this variable is missing as unsecured. Following a suggestion of the referee, we rerun our core models (Models 3 and 4 of Table V) adding an additional dummy variable that takes the value one for these missing observations. We continue to find similar results on the variables of interest to test Hypotheses 1 through 3 after we add this additional control variable. This new variable comes out positive and statistically significant, but with a coefficient much smaller than the coefficient on our original secured dummy, suggesting that most, but not all, of the missing observations are indeed of secured loans.

V. Final Remarks

In this paper, we compare bank loan spreads for borrowers that have access to public bond markets with those of bank-dependent borrowers. We find that these spreads i) are higher for bank-dependent firms than for firms with access to public bond markets, ii) rise in recessions, and iii) rise by a greater amount in recessions for bank-dependent firms. These contrasts are stronger when public bond market access is more recent. Our results are both economically and statistically significant, and they continue to hold when we control for firm- and loan-specific factors and for the endogeneity of public bond market access. Our findings are consistent with a model in which banks earn informational rents as per Rajan (1992), since such rents should be greater for bank-dependent firms and should increase when firms face greater risk.

Our work opens up several avenues for additional research. As we have noted, our sample focuses on relatively large, often syndicated, loans. Since information problems are typically thought to be greater for smaller firms, investigating the behavior of loan spreads for such firms across the business cycle might afford greater insight into the size of any informational rents that banks earn.

A further question involves the possible benefits of bank-firm relationships. Petersen and Rajan (1995) find that, although banks charge higher average spreads when they have more monopoly power, they also extend loans to riskier young firms because their future rents on the survivors make up for the additional failures. Some have argued that similar concerns ought to lead banks with greater power over their borrowers to subsidize them in bad times, raising spreads in recessions by less than banks that have less power over their borrowers. Although we do not find evidence of this in our comparison of bank-dependent firms with firms that have public bond market access, it would be interesting to investigate how loan spread behavior for bank-dependent borrowers varies with their banks' market power.

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