Internet Appendix for Corporate Governance and Loan Syndicate Structure.

## A A Simple Model for the Development of Empirical Hypotheses

The purpose of this section is to derive precise empirical predictions to guide the empirical tests in the paper. It is well known from the literature (e.g., Holmström and Tirole (1997)) that firms with limited public information require due diligence and monitoring by an “informed” lender before “uninformed” lenders invest in the firm. We conjecture that this due diligence is even more critical for firms that have greater shareholder rights. The reason is that shareholder-friendly firms are more likely (relative to less shareholder-friendly firms) to indulge in risk-shifting actions that benefit equity at the expense of debtholders (Jensen and Meckling (1976)). Since an informed lender’s monitoring and due diligence effort is unobservable, such a lender must retain a larger financial stake in the borrowing firm to form a viable lending syndicate. We formalize this intuition to derive our empirical predictions.

### A. Model setup

We assume that all agents are risk neutral, and the riskless interest rate is 0. The economy has a single firm managed on behalf of equity investors, and the firm needs to borrow to finance an investment opportunity that requires *I* dollars. The loan may be contracted from a lending syndicate with the lead lender retaining a share  while the participants provide the balance  share of the loan. If  the loan is provided by a sole lender and thus is not a syndicated loan.

There are three dates in the model, . The firm contacts debt investors at the initial date 0 to finance its project. The debt matures at date 2, which is also when the project payoffs are realized. The assets purchased at date 0 can be liquidated at date 1 for value *L* where *L*  *I*. There is no liquidation value at date 2. If the firm is not liquidated, the project payoffs are realized at date 2 and lenders are paid either the promised face amount or the entire payoff in the event that the realized payoffs are not sufficient to repay the face value of the debt.

The firm’s project is either good or bad. A good project is always successful and returns *G* with complete certainty. A bad project is successful with probability  [0, 1] and returns *B* if successful and 0 otherwise. We make the following assumption:

Assumption 1: 

Assumption 1 states that the good project has a positive NPV, while the bad project has a negative NPV. Further, if the type of the project is known, liquidation is preferable to the continuation of the risky project.

As of date 0, the type of project undertaken by the firm is not yet determined. The project type is realized and becomes known to the firm at date 1. The project is bad with probability  and good otherwise, for a type *p* firm. The manufacturing policy or management style (also termed the *p*-policy) followed by a shareholder-friendly firm is more likely to result in a bad project (but one that is preferable to equity holders, who realize a higher payoff if it is successful and walk away because of their limited liability if it is unsuccessful) realized at date 1. We thus interpret *p* as the degree of shareholder rights in the model.

The firm has no money of its own and has to borrow from financiers to invest in the project. There are two types of lenders (informed and uninformed) in the date 0 market for credit. Credit markets are assumed to be competitive.

• Banks/Informed/Relationship lenders enter the market at date 0 and date 1 to acquire information and make loans. A bank that makes a loan to a firm at date 0 can obtain information about that firm by monitoring its activities (more on this below). Much of the information obtained is not verifiable or cannot be credibly communicated to a third party (e.g., a judicial court).

• Uninformed/Loan syndicate participant investors lend at date 0 and return at date 2 for their promised payments. We assume that these investors do not monitor the firm, by invoking the standard argument that the dispersion of security holders generates either free-rider problems or a wasteful multiplication of monitoring costs as in Diamond (1984).

The firm makes an investment *I* at date 0 and its type *p* is private information on this date. The bank does not know the borrower’s type and in turn chooses to monitor at cost *c*. Then, at date 1, it will learn the true project type realized by the firm (Good or Bad) and can choose to liquidate the bad project and realize *L*. We assume that it is not possible to write state-contingent contracts on the type of project investment or the state realized. Further, we allow only debt contracts, an assumption justified by appealing to the costly state verification technology in Gale and Hellwig (1985). Since any debt contract can be expressed as a linear combination of pure discount debt contracts, we consider only the latter, in this model, in order to keep matters simple. The discount debt contract involves a borrowing *I* at date 0 and a single repayment *D* at date 2. Thus, the face value of debt is *D*.

While the NPV of the bad project is negative, a borrower is inclined to shift risk into and invest in such a project to transfer wealth from the lenders. We ensure risk-shifting by assuming the following:

Assumption 2: 

Assumption 2 states that that the borrower prefers the bad project to the good one, if financed by outside investors.

In the case of a single informed lender (the bank) it is clear that the bank will monitor the firm at a cost *c* to learn the project type at date 1. Since  .(*D* has to be less than *B*, the lender payoff when the risky project succeeds and  by assumption), it pays off for the lender to invest in monitoring and due diligence. Doing so will enable the lender to liquidate the risky project if undertaken, at .

### B. Theoretical Results and Empirical Implications

We now turn to syndicated lending. For simplicity, we consider one lead lender with share of loan *I* and one syndicate member with share (1-)*I*. We evaluate two cases: No monitoring by the lead (lead lender moral hazard since it owns only a fraction of the loan but has to pay the full cost of monitoring), and monitoring by the lead bank.

No monitoring by the lead bank : The break-even conditions for lending for the lead and the syndicate member imply,

 

Monitoring by the lead bank : The break-even conditions for lending for the lead and the syndicate member imply,

 

 

The lead lender will commit to monitor (incurring cost *c*) and liquidate the firm at t=1 if the payoff from this action is greater than shirking monitoring. Of course, ex ante the contract is set as if the lead is committed to monitoring. This implies

 

  (IA.1)

This result implies that the syndicate member needs a minimum commitment of  from the lead to ensure that the latter will monitor and conduct due diligence on the firm. Thus, syndicate structure is a function of the primitives of the model, including shareholder rights and the cost of monitoring. We derive the following testable implications from the model:

*Empirical Implication 1:*

 

This result implies that as firms increase their shareholders’ rights, the lead lender share in the syndicate increases. The intuition is that firms with greater shareholder rights tend to pursue riskier projects that benefit shareholders at the expense of creditors. Thus, the lead lender is required to exert greater due diligence in monitoring in order to prevent syndicate moral hazard. The result says that in order to provide the lead lender with sufficient monitoring incentives, syndicates must be more concentrated as shareholder rights increase.

We can rewrite equation (1), the viable condition for syndicated lending, by rearranging it as

  (IA.2)

This leads to our next empirical implication.

*Empirical Implication 2:*

For firms below a critical level of shareholder rights *p*, syndicated lending is feasible. Firms above *p* have to resort to sole lender loans. The intuition for this implication is similar to that of implication 1. If firms are too prone to risk-shifting (too shareholder friendly above a certain threshold), the lead needs to hold the entire share of the loan as a commitment device to monitor the firm.

*Empirical Implication 3:*

 

This result is not unique to this setup and was first highlighted by Holmström and Tirole (1997). Opaque firms need greater monitoring efforts by the lead lender, and in the presence of syndicate moral hazard such efforts can be accomplished by the lead having adequate skin in the game. We test these empirical implications in the data using a combination of natural experiments that exogenously shift shareholder rights (as described in the paper) and panel regressions using empirical proxies for shareholder rights (described below).

## B Additional Robustness Tests

### A. Elastic supply of capital

The discussion on page 13 of the paper suggests that our interpretation of changes in the monitoring incentives due to reduced takeover threat (in the natural experiment results in Table 3) might be confounded by changes in the elasticity of credit supply by other banks in the syndicate for the same reason. This is an important issue that has to be addressed to ensure a clean interpretation of our results. Griffith and Reisel (2017, U. Chicago Law Review) note that “dead hand proxy puts” are a contractual innovation to the change-of-control provision that has been standard in corporate debt agreements since the 1980s. Dead hand proxy puts are a contractual innovation in corporate debt agreements that change the nature of proxy fights. This provision triggers default and immediate repayment of corporate indebtedness in the event that a dissident slate wins a majority of the seats on the target company’s board. The provision originated in the days of leveraged buyouts–specifically the 1988 takeover of RJR Nabisco by Kohlberg Kravis Roberts & Co (KKR), when, rather than refinancing the target’s outstanding debt, KKR added layers of additional leverage, thereby reducing the value of existing bonds by 14.5 percent[[1]](#footnote-1) [[2]](#footnote-2) [[3]](#footnote-3). These change-of-control covenants were originally designed to protect creditors from a sudden increase in credit risk associated with leveraged buyouts and hostile takeovers. This provision has been standard in loan agreements since 1988. By explicitly controlling for year fixed effects (and thus controlling for the elastic supply of capital argument) in our regressions in Table 3 of the paper, we find significant results on the AFTER\_ATS variable, suggesting that monitoring incentives are also important in the formation of syndicate structure.

We also design two other stringent versions of this test, as follows. Before 1988, in the absence of these change-of-control covenants, the antitakeover law changes would affect both the monitoring incentives of the lead arranger and the elasticity of supply of credit to the lead arranger. However, post-1988, the presence of these specific change-of-control covenants in loan agreements would shut down the elasticity of credit supply channel, since the specific concern of the syndicate members is now explicitly addressed by these covenants included in the loan agreements. Thus, if we find a significant coefficient on the antitakeover law changes in the post-1988 period in the regressions, it can be interpreted cleanly. Our results can be attributed to changes in monitoring incentives of the lead arranger, after the law change.

To implement this, we split our AFTER\_ATS variable into two variables: AFTER\_ATS  PRE\_1988 and AFTER\_ATS  POST\_1988. We expect the coefficient on the AFTER\_ATS  POST\_1988 to be significant in the regressions, consistent with the clean interpretation that the law change affected the monitoring incentives of the lead arranger. We estimate the model in Table 3 with and without year fixed effects. We exclude the year fixed effects for two reasons. First, our key variable AFTER\_ATS, is a time-series variable. Second, since the change-of-control provision (POST\_1988) in our model is also a time variable, it can be argued that adding year fixed effects to the model would difference out any meaningful variation of the economic effect we seek to capture in our tests. Hence year fixed effects should be excluded in the estimations to assess the true economic effect. Nevertheless, we also estimate a far more restrictive model in which we include year fixed effects as additional controls, to assess the robustness of our results. We present the summary of the key results of Table 3 estimated both ways: in Table IA.1, Panel A (year fixed effects excluded) and Table IA.1, Panel B (year fixed effects included) below:

**TABLE IA.1 Panel A**

**Effect of Change in Antitakeover State Laws on Loan Syndicate Structure**

All models include purpose of loan, and firm () fixed effects. Heteroscedastic robust *t*-statistics controlling for state of incorporation cluster effects are reported in parentheses (\*\*\* Significant at the one percent level, \*\* Significant at the five percent level, \* Significant at the ten percent level).

|  |  |  |
| --- | --- | --- |
|  | Calendar Time Sample | Event Time Sample |
|  | (1) | (2) | (3) | (4) | (5) | (6) |
|  | % HELD\_BY\_LEAD | HERFINFDAHL | ln(1+#LENDERS) | % HELD\_BY\_LEAD | HERFINFDAHL | ln(1+#LENDERS) |
| AFTER\_ATS  PRE\_1988 | -0.0521 | -0.0586 | 0.1285 | -0.0627 | -0.0572 | 0.2640 |
|  | [-1.57] | [-1.89] | [1.27] | [-3.69] | [-4.48] | [5.87] |
| AFTER\_ATS POST\_1988 | -0.0457 | -0.0303 | 0.1681 | -0.0483 | -0.0321 | 0.1587 |
|  | [-3.57] | [-3.30] | [5.26] | [-2.75] | [-2.23] | [3.94] |
| Loan Purpose Indicators | Yes | Yes | Yes | Yes | Yes | Yes |
| Other Control Variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | No | No | No | No | No | No |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1,748 | 1,748 | 1,748 | 557 | 557 | 557 |
| Adjusted  | 0.742 | 0.765 | 0.769 | 0.664 | 0.699 | 0.725 |

As seen from Table IA.1, Panel A, consistent with our clean interpretation that the law change affected the monitoring incentives of the lender, we find that the coefficient of AFTER\_ATS  POST\_1988 is the correct sign and is statistically significant for all three syndicate structure measures. This result applies to both the calendar time sample (specifications 1-3) and the event time sample (specifications 4-6).

**TABLE IA.1 Panel B**

**Effect of Change in Antitakeover State Laws on Loan Syndicate Structure (Including Year Fixed Effects)**

All models include purpose of loan, year (), and firm () fixed effects. Heteroscedastic robust *t*-statistics controlling for state of incorporation cluster effects are reported in parentheses (\*\*\* Significant at the one percent level, \*\* Significant at the five percent level, \* Significant at the ten percent level).

|  |  |  |
| --- | --- | --- |
|  | Calendar Time Sample | Event Time Sample |
|  | (1) | (2) | (3) | (4) | (5) | (6) |
|  | % HELD\_BY\_LEAD | HERFINFDAHL | ln(1+#LENDERS) | % HELD\_BY\_LEAD | HERFINFDAHL | ln(1+#LENDERS) |
| AFTER\_ATS  PRE\_1988 | -0.0527 | -0.0578 | 0.1396 | -0.0423 | -0.0443 | 0.2649 |
|  | [-1.45] | [-1.79] | [1.27] | [-1.26] | [-1.91] | [2.83] |
| AFTER\_ATS  POST\_1988 | -0.0852 | -0.0783 | 0.2766 | -0.0991 | -0.1101 | 0.3556 |
|  | [-2.00] | [-1.60] | [1.71] | [-3.70] | [-3.89] | [3.34] |
| Loan Purpose Indicators | Yes | Yes | Yes | Yes | Yes | Yes |
| Other Control Variables | Yes | Yes | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm Fixed Effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Observations | 1,748 | 1,748 | 1,748 | 557 | 557 | 557 |
| Adjusted  | 0.742 | 0.765 | 0.769 | 0.664 | 0.699 | 0.725 |

As seen from Table IA.1, Panel B (including year fixed effects in all regression specifications), these results are largely similar to Table IA.1, Panel A. However, as we argued above, the results are statistically weaker, albeit significant at the conventional levels in five of the six specifications for the AFTER\_ATS  POST\_1988 interaction variable.

Overall, we conclude from these results that lead arranger monitoring incentives are a significant determinant of the syndicate structure, even after controlling for the elastic supply of capital to the lead arranger by syndicate participants.

### B. Joint Estimation of Loan Spread and Syndicate Structure

To address the issue that the price of the loan and the composition of its syndicate are jointly determined, we follow the methodology in Denis, Nandy, and Sharpe (2000) and Bharath et al. (2011): we estimate a joint system of equations, one of which is the syndicate structure and the other the ln(loan spread), using instrumental variables (IV) methods. To uniquely identify the system we need an instrument for the loan spread. We use the lagged average loan spread for all loans made to firms in the same industry (using the 12 Fama–French industry classifications) over the previous 6 months. The argument is that past market pricing (a proxy for market conditions) on other similar loans is an important consideration for bankers when determining the current market pricing of loans. Further, past loan rates are unlikely to affect the current loan’s syndicate structure except possibly through the current loan rates, thus potentially satisfying the exclusion restriction. We note that this instrument has also been used by Bharath et al. (2011). We present the second-stage regression of syndicate structure (the focus of our interest) in the table below:

**TABLE IA.2**

**IV estimation: Joint Estimation of Syndicate Structure & Loan Spread**

We use the ln(Average AISD of all deals in the last 6 months in the same 12 Fama–French industries) as an instrument for the ln(AISD) (following Bharath et al.(2011)). The table reports the second-stage estimates along with the first-stage F-statistic for the instrument quality.

|  |  |  |  |
| --- | --- | --- | --- |
|  | (1) | (2) | (3) |
|  | % HELD\_BY\_LEAD | HERFINDAHL | Ln(1+#LENDERS) |
| G-Index | -0.002 | -0.002 | 0.013 |
|  | [-1.92] | [-2.13] | [2.38] |
|  | 0.007 | -0.006 | 0.474 |
| (instrumented) | [0.18] | [-0.17] | [2.25] |
| First stage | 0.008 | 0.008 | 0.008 |
| Partial R2 |  |  |  |
| First stage | 14.68 | 14.68 | 14.68 |
| F- Stat |  |  |  |
| All other | Yes | Yes | Yes |
| controls included? |  |  |  |
| Observations | 3090 | 3090 | 3090 |
| Adjusted  | 0.425 | 0.412 | 0.574 |

First, we note that the instrument is relevant and overcomes the weak instrument problem since the first-stage F-statistic of 14.68 is greater than the cutoff of 10 suggested by Stock and Yogo (2005). We find that the G-Index continues to be strongly statistically significant as before, even after accounting for the joint determination of loan spread and syndicate structure. Interestingly, consistent with the reviewer’s point, we find that loan spread does matter in determining the number of lenders in the syndicate. The instrumented loan spread is positive and statistically significant in specification 3. Overall we conclude that joint determination of loan spread and syndicate structure is important, and our results continue to hold even after controlling for this issue.

### C. Flex Pricing

After the Russian debt crisis in 1998, the period that covers our G-Index sample, arrangers adopted a strategy known as market-flex language, which allows them to change the pricing of the loan based on investor demand – in some cases within a predetermined range – as well as shift amounts between various tranches of a loan, as a standard feature of loan commitment letters, thus tailoring the loan to make it more attractive to potential subscribers. Because of the flex provision, a loan syndication functions as a “book-building” exercise. A loan is originally launched to market at a target spread or with a range of spreads. Potential subscribers then will make commitments that in many cases are tiered by the spread. At the end of the process, the arranger will total up the commitments and then make a call on where to price, or “print,” the paper. Thus what what we potentially measure is the relation between the G-Index and the “quality” of the flex provisions affecting the syndication success.

Ideally, if we can include the flex-pricing measure and estimate the regression, the omitted variable bias (OVB) created by not including the flex-pricing feature can be eliminated. Unfortunately, there is a paucity of data on the existence and the extent of flex pricing in the DealScan database. A possible way to address the OVB is to include a proxy that is related to the flex pricing, our omitted variable. As we describe below, flex pricing appears to be a dominant feature of the “leveraged” loan syndication process rather than of the entire universe of syndicated loans. A recent working paper by Bruche, Malherbe, and Meisenzahl (2018) examines the flex-pricing feature using the data from S&P Capital IQ’s Leveraged Commentary and Data (LCD). This database is different from the DealScan database we use in our paper, and it appears to be the only source for identifying flex pricing. In our conversation with the data vendors we learned that flex pricing is largely a feature of “leveraged loans” and that within the leveraged loans it tends to be rare for revolving lines of credit. Specifically the vendors informed us that flex pricing is mostly used for loan facilities designed for institutional investors such as insurance companies, hedge funds, and CLOs. This institutional tranche is a bullet repayment term loan (typically this is referred to as term loan B tranche or simply TLB). This discussion suggests that (i) non-leveraged loan facilities are unlikely to have flex pricing and (ii) even within the leveraged loan facility universe, the revolving lines of credit are not likely to have the flex-pricing feature. We first examine what fraction of our sample consists of leveraged loans, and especially of leveraged loans that are not revolving lines of credit, as this subset is most likely to have flex pricing. To identify leveraged loans we follow the S&P LCD guideline. We classify all loans with a spread of LIBOR +125 basis points or higher and secured as leveraged loans. Within the subsample of leveraged loans we exclude revolving lines of credit since these are unlikely to have flex pricing. This classification yields 203 out of 3,223 facilities in our sample (approximately 6.3% of the overall sample). This subset of loans in our sample is most likely to be subject to flex pricing, and we classify these loans with the indicator variable “Flex Pricing” that equals 1, and 0 otherwise. We run a regression in which we include the “Flex Pricing” indicator as a proxy for the flex-pricing feature. We find that our results on the shareholder rights reported in the paper remain essentially unchanged (see Table IA.3 below). We also find that flex pricing is an important factor for determining the syndicate structure as measured by the number of lenders. The presence of flex pricing is associated with more lenders in the syndicate.

**TABLE IA.3**

**Controlling for the Flex-Pricing Feature**

We include the indicator variable “Flex Pricing” that equals one if the loan facility has a spread of LIBOR + 125 and is not a revolving line of credit and is secured.

|  |  |  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|  | % HELD\_BY\_ | HERFINDAHL | Ln(1+# | % HELD\_BY\_ | HERFINDAHL | Ln(1+# | % HELD\_BY\_ | HERFINDAHL | Ln(1+# |
|  | LEAD |  | LENDERS) | LEAD |  | LENDERS) | LEAD |  | LENDERS) |
| G-INDEX | -0.0031 | -0.0021 | 0.0080 |  |  |  |  |  |  |
|  | [-3.06] | [-2.30] | [1.83] |  |  |  |  |  |  |
| CLASSIFIED\_BOARD & |  |  |  | -0.0126 | -0.0118 | 0.0502 |  |  |  |
| PROHIBITIONS |  |  |  | [-2.40] | [-2.45] | [2.16] |  |  |  |
| ON\_VOTING |  |  |  |  |  |  |  |  |  |
| CLASSIFIED\_BOARD & |  |  |  |  |  |  | -0.0127 | -0.0092 | 0.0235 |
| POISON\_PILL |  |  |  |  |  |  | [-2.33] | [-1.87] | [1.00] |
| & BLANK\_CHECK |  |  |  |  |  |  |  |  |  |
| FLEX\_PRICING | 0.0225 | 0.0202 | 0.2135 | 0.0230 | 0.0204 | 0.2126 | 0.0234 | 0.0208 | 0.2113 |
|  | [1.28] | [1.20] | [4.03] | [1.31] | [1.22] | [4.02] | [1.33] | [1.24] | [3.99] |
| All other | Y | Y | Y | Y | Y | Y | Y | Y | Y |
| controls included? |  |  |  |  |  |  |  |  |  |
| Observations | 3,223 | 3,223 | 3,223 | 3,223 | 3,223 | 3,223 | 3,223 | 3,223 | 3,223 |
| Adjusted  | 0.414 | 0.415 | 0.578 | 0.413 | 0.415 | 0.579 | 0.413 | 0.415 | 0.577 |

1. Janet Key, $25 Billion Nabisco Sale Largest Takeover (Chi Tribune, Dec 1, 1988, visited Feb 1, 2019, Perma archive unavailable) http://articles.chicagotribune.com/1988-12-01/news/88022101251rjr–nabisco–camel–cigarettes–kohlberg–kravis–roberts [↑](#footnote-ref-1)
2. Kenneth N. Gilpin, Bid for RJR Nabisco Jolts Bonds (NY Times, Oct 21, 1988) online, visited Feb 1, 2019, Perma archive unavailable) http://www.nytimes.com/1988/10/21/business/credit-markets-bid-for-rjr-nabisco-jolts-bonds.html [↑](#footnote-ref-2)
3. George Anders, ‘Recapitalizations’ Area Bonanza for Some, but Bondholders Can Take a Terrific Beating, Wall St J 53 (June 1, 1987) (describing the growing use of debt-buying strategies in the late 1980s and their effect on bondholders) [↑](#footnote-ref-3)