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Why do loans contain covenants? Evidence from lending relationships[☆]

Robert Prilmeier

A. B. Freeman School of Business, Tulane University, New Orleans, LA 70118, USA

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ABSTRACT

Despite the importance of banks' role as delegated monitors, little is known about how non-price terms of loan contracts are structured to optimize information production in a lending relationship. Using a large sample of corporate loans, this paper examines the effect of relationship lending on covenant choice. Consistent with information asymmetry theories, covenant tightness is relaxed over the duration of a relationship, especially for opaque borrowers. In contrast, the effect of lending relationship intensity on the number of covenants included in a loan follows an inverted U shape. I discuss potential explanations for this finding.

1. Introduction

Finance theory has long held that banks perform a special role as delegated monitors (e.g., Diamond, 1984; 1991). Through repeated interaction with the borrower, a relationship bank produces information that can reduce overall contracting costs for the firm (Fama, 1985). Although the structure of a loan contract should be set to optimize this information production, surprisingly little is known about the effect of lending relationships on loan terms beyond the price and availability of credit.[1]

This paper contributes to closing that gap by studying how relationship lending affects the use of loan covenants in a sample of large loans to publicly listed borrowers. The contracting literature suggests that financial covenants play a key role in creditors' monitoring activities. In models by Aghion and Bolton (1992) and Dewatripont and Tirole (1994), assigning state-contingent control rights to creditors can enhance firm value. Financial covenants provide for such a shift of control rights outside of bankruptcy

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E-mail address: rprilmei@tulane.edu

[1] One exception is Bharath et al. (2011) who consider the effect of lending relationships on maturity and collateral requirements.

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when borrower performance falls below a predefined accounting threshold. Recent studies show that lenders actively use these control rights to protect their interests and that the presence of covenants is associated with lower interest rates.[2] How should control rights be distributed when relationship lenders acquire information about the borrower?

Forming and maintaining a lending relationship affects information asymmetries at two levels. First, as the lender becomes informed about the borrower, information asymmetries between the two parties are reduced. Gârleanu and Zwiebel (2009) develop a model in which information asymmetries make it optimal to set excessively restrictive covenants at the beginning of a lending relationship. Covenants are subsequently relaxed as the lender learns about the borrower's type and the need for covenant protection declines. Second, the relationship lender's information acquisition potentially increases the information distance between herself and nonrelationship lenders. It is not clear how this difference in knowledge about the borrower should affect covenant choice. Rajan (1992) argues that lock-in effects allow relationship banks to impose less favorable terms on the borrower. However, Schenone (2010) finds that lock-in effects are not a concern for publicly listed borrowers such as those studied in this paper. Rajan and Winton (1995) and Park (2000) develop models in which a lender has the option to become informed and covenants incentivize the lender to acquire information despite free-riding by uninformed creditors. However, these models consider differences between a bank lender and dispersed outside creditors, rather than differences between bank lenders that have similar monitoring technologies. Exactly how relationship lending affects covenant use is thus an empirical question.

I explore this question with a sample of 7,924 loans taken from the DealScan database. I measure relationship status in two different ways. The first measure, relationship intensity, is the proportion of the firm's loans over the previous five years that have been arranged by the current lender. A low level of relationship intensity implies that the current lender has not previously been the borrower's main lender. A medium level implies that the current lender is likely to be the main lender but the firm also borrows from other lenders, while a high level implies an exclusive lending relationship. Thus, this measure is meant to proxy for how well the lender knows the borrower relative to other lenders. Consequently, it allows distinguishing between relationship effects in exclusive and nonexclusive relationships. The second measure is the duration of the borrower's relationship with the current lender, which can be viewed as a proxy for how well the lender knows the borrower in absolute terms.

I first test the effect of relationship status on covenant tightness, which is defined as the average ex ante violation probability of a loan's financial covenants. Results suggest that covenant tightness monotonically decreases in a lending relationship, and more so for small, unrated borrowers for whom a reduction in information asymmetries is likely to be important. Moreover, this effect primarily applies to information asymmetries between the borrower and the lender, as measured by relationship duration, rather than the intensity of the relationship relative to other lenders. These results strongly support the theory proposed by Gârleanu and Zwiebel (2009).

The analysis next turns to covenant intensity, defined as the number of financial covenants attached to a loan. In contrast to the results for covenant tightness, the effect of lending relationships on covenant intensity appears to be driven by relationship intensity rather than the duration of the relationship. In addition, the relationship effect is nonlinear. Covenant intensity is highest for medium levels of relationship intensity. Loans have fewer covenants both when the current lender has little prior relationship with the borrower and when the current lending relationship is exclusive. I discuss various potential explanations for this inverted U effect. One potential explanation is that the empirical result is created by a confluence of separate factors. The increasing portion of the inverted U could be due to borrowers suffering hold-up effects (Rajan, 1992). The decreasing portion could potentially be related to information asymmetry effects. Note, however, that Schenone (2010) finds that these two factors have a U effect, rather than an inverted U effect, on the yield spreads paid by unlisted borrowers. Another potential explanation is that borrowers that use multiple lenders find it optimal to give monitoring incentives in the form of covenants to their main relationship lender, whereas using just one lender reduces the free-rider problem and thus limits the benefits from incentivizing that lender with additional covenants.

One way to assess reasons for the inverted U effect is to examine differences in the relationship effect across borrowers with varying degrees of bargaining power. A high degree of bargaining power should help borrowers negotiate a contract that is preferable from their point of view. The results show that the decrease in covenant intensity in exclusive relationships is concentrated in large borrowers with access to the public debt market. In addition, in a syndicated loan, all loan participants are entitled to the same covenants. If the inverted U effect is related to monitoring incentives, it should be stronger for sole lender loans or loans with only one lead arranger since for such loans the sole lender or lead arranger captures a larger fraction of the benefits from monitoring. Indeed, covenant use increases more strongly in nonexclusive relationships for such loans. I find some evidence that the decrease in covenant intensity in exclusive relationships is concentrated in loans with one lead arranger. There is no evidence that relationship effects on covenant intensity are stronger for opaque borrowers that are more likely to be subject to hold-up and information asymmetry concerns.

The choice to borrow from a relationship lender is likely endogenous. In addition, borrowers that maintain multiple relationships could differ from borrowers that rely on an exclusive lender. To rule out that the results are driven by selection effects or omitted variable bias, I employ several different strategies. First, I use instrumental variables (IV) estimation, exploiting differences in borrowers' proximity to banks that actively syndicate loans. The

[2] See Bradley and Roberts (2015), Matvos (2013), Chava and Roberts (2008), Roberts and Sufi (2009), Nini et al. (2009); 2012), Reisel (2014).

results are consistent with a causal effect of relationship status on covenant choice. In addition, results are robust to using propensity score matching methods. I also investigate the relationship effect on loan contract terms that are not likely to create monitoring incentives. There is no evidence of an inverted U effect of relationship intensity on these contract terms. Finally, several papers find a trade-off between debt covenants and debt pricing, suggesting that these contract terms are determined simultaneously (Bradley and Roberts, 2015; Matvos, 2013; Reisel, 2014). Results show that the inverted U effect holds even after accounting for the price-covenant trade-off.

This paper contributes to the growing literature on why debt contains covenants. Previous work shows that covenants are used to mitigate agency costs and to flexibly monitor borrower performance.[3] I examine the setting modeled by Gârleanu and Zwiebel (2009) and show that covenant tightness behaves as predicted by the information asymmetry theory. This paper also contributes to the large literature on lending relationships. While this literature has traditionally focused on small, unlisted firms, in a recent paper Bharath et al. (2011) find that loans with higher relationship intensity carry lower interest rates, reduced collateral requirements, and are associated with larger availability of credit. However, this paper is the first to investigate the effect of relationship intensity on covenant use. In addition, the differences in the results for covenant tightness and covenant intensity are consistent with these terms performing different roles in a loan contract. Thus, the paper adds to the finding of Demiroglu and James (2010) that covenant tightness contains private information about the firm's prospects, but covenant intensity does not.

The remainder of this paper is structured as follows. Section 2 provides an institutional background on financial covenants and describes theories on why loans should contain covenants. Section 3 details the data collection process. Section 4 presents the results for covenant tightness, while Section 5 discusses the results for covenant intensity. Section 6 performs additional robustness checks, and Section 7 concludes.

2. Institutional background and covenant theories

This section describes how financial covenants work and provides two examples of loans whose covenants were violated. It then discusses theories of covenant choice in bank loans.

2.1. Financial covenants as a monitoring device

Financial covenants require the borrower to maintain certain accounting measures of financial health, such as a minimum net worth or a maximum ratio of debt to earnings before interest, taxes, depreciation and amortization (EBITDA). If the borrower fails to comply with the covenant, creditors obtain the right to accelerate the debt. In practice, creditors rarely exercise this right (Nini et al., 2012). Instead, covenant violations trigger a renegotiation process in which the right to accelerate assigns a high amount of bargaining power to the creditors. Recent studies show that creditors use this bargaining power to increase interest rates, reduce the size of the credit line, or require additional collateral, but they also push for performance improvements and replacement of poorly performing chief executive officers (CEOs) (Chava and Roberts, 2008; Roberts and Sufi, 2009; Nini et al., 2009; 2012).

The following two cases provide good examples of how covenants work and how a lack of covenants can erode value. In September 1998, Key Energy Services Inc (KES) entered a revolving credit agreement with two term loans for $550 million with a loan syndicate led by PNC Bank. The deal contained four covenants with a maximum debt/EBITDA covenant that was set at slightly less than one quarterly standard deviation above KES's debt/EBITDA ratio. Between September and December 1998, the company's stock lost roughly 50% in value, and the company reported a net loss and a violation of its covenants. The loan syndicate responded by reducing the credit available, increasing the interest rate, loosening some covenants, and adding new covenants and restrictions. Ultimately, the firm recovered. In this case, creditors used their control rights to reassess the company's risk, reduce their exposure, and add restrictions to curb moral hazard. While the firm might have recovered even without creditor intervention in this particular example, the above-mentioned findings of Nini et al. (2012) suggest that creditors do play a positive role in a firm's recovery after a covenant violation.

Creditors were less fortunate in the case of HomeBase Inc. This company entered a revolving credit agreement for $250 million with a loan syndicate led by BankBoston. The loan contained only one tangible net worth covenant that was loosely set. In fiscal year 2000, HomeBase reported a net loss of $70 million, which did not trigger the covenant. On October 27, 2001, $117.8 million were outstanding under the facility and the company reported a net loss of $188 million year-to-date, resulting in a covenant violation. On November 7, 2001, the company filed for Chapter 11. Eventually it went into liquidation, and creditors lost 70 cents on the dollar. This example illustrates that when the covenant package is too weak, a violation can arrive too late for the creditors to be able to protect themselves.

2.2. Theories of covenant use

The contracting literature offers several theories of covenant use. These theories revolve around moral hazard and information production and they are not mutually exclusive. This section discusses how these theories relate to the setting examined in this paper.

2.2.1. Protection against information asymmetries

Gârleanu and Zwiebel (2009) develop a model that seeks to explain why covenants assign control to creditors remarkably often. In their model, there is an information asymmetry between a lender and an entrepreneur about the potential for future wealth transfers. They show

[3] See, for example, Billett et al. (2007), Chava et al. (2010), Miller and Reisel (2012), Qi and Wald (2008), and Qi et al. (2011), in addition to the papers mentioned earlier.

that in this situation firm value can be enhanced by giving the lender strong decision rights that provide protection against the information asymmetry. As the lender collects information about the borrower, ex post renegotiation will be biased towards the lender relinquishing these excessive rights. The setting of this model directly mirrors a lending relationship between a firm and a bank. Thus, the model has two implications in the empirical setting studied here. First, to the extent that information asymmetries decline over the course of a lending relationship, covenants should become less restrictive. Second, this effect should be stronger for more opaque borrowers for whom ex ante information asymmetries are likely to be larger.

2.2.2. Covenants as a monitoring incentive

When a firm obtains capital from multiple sources, individual claimholders have limited incentives to monitor because they would rather free-ride on other claimholders' monitoring efforts. This lack of monitoring incentives could subject the borrower to additional contracting costs. For example, new creditors could require a higher interest rate than they would if another creditor was already monitoring the firm, or they could refuse to extend credit. Rajan and Winton (1995) and Park (2000) develop models that show how covenants help solve this problem. In Rajan and Winton (1995), a borrower obtains capital from a bank and a set of small outside creditors who do not monitor. The firm and the bank choose between a short-term loan without covenants and a long-term loan with covenants. The bank monitors if the gains from monitoring outweigh monitoring costs. Rajan and Winton (1995) argue that the loan with covenants increases the bank's benefits from monitoring relative to not monitoring since it makes the loan's effective maturity contingent on monitoring. Park (2000) studies different classes of debt. In his model, the optimal debt structure involves giving a senior claim with strong covenants to the lender with the lowest monitoring cost, while all other lenders are junior and do not monitor.

If covenants are used as monitoring incentives, it seems conceivable that the setting of monitoring incentives would be related to the borrower's relationship with the lender. However, the existing models provide limited guidance for the setting studied in this paper. First, this paper examines contract differences across multiple senior bank lenders rather than a dichotomy between a senior bank lender and small, uninformed creditors, who, in Park (2000), are junior to the bank. Second, the above theories study a choice between a contract with covenants and one without covenants, whereas the loans studied in this paper all have covenants, but the degree of covenant strictness varies. It is thus not immediately obvious what predictions could be derived from the above models in the context of lending relationships. Due to the limited theoretical guidance on how various motivations could affect covenant choice in lending relationships, this paper treats the impact of relationship lending on covenant choice as an empirical question. To this end, I use various measures of relationship status and test for any nonlinearities in the relationship effect. In particular, the tests allow for differences between exclusive and nonexclusive relationships.

3. Data and measurement

I obtain data on syndicated and large sole lender loans from Loan Pricing Corporation's DealScan database. DealScan reports yield spreads, covenants, maturities, and other loan characteristics and accounts for a large proportion of the U.S. loan market.[4] The sample consists of 7,924 loans incurred by 3,171 U.S. borrowers between the years 1995 and 2008. Firms from the financial, utility, or public administration sectors are excluded. Loans are reported in DealScan as deals that contain one or several facilities. Since the same covenants apply to all facilities in a deal, I aggregate all data to the deal level. I merge these data with the borrowers' accounting data from Compustat using a link file provided by Michael Roberts and Sudheer Chava and described in Chava and Roberts (2008).

Covenant choice involves a decision of how many covenants to include in a loan (*covenant intensity*) and how tightly to set these covenants (*covenant tightness*). Covenant tightness is measured as follows. As discussed in Murfin (2012), for a covenant that stipulates a minimum value $\underline{r}$ for the financial ratio r that is normally distributed with standard deviation σ, tightness can be measured as the probability of a covenant violation:

$$p = 1 - \Phi\left(\frac{r_t - \underline{r}}{\sigma}\right), \tag{1}$$

where Φ denotes the cumulative standard normal distribution function. If the covenant limits r to a maximum ratio, the numerator in the parentheses (the covenant slack) is multiplied by minus one. Using this equation, I estimate the tightness of each financial covenant that is attached to a loan. Slack is measured using the difference between the financial ratio in the quarter prior to the loan start date and the covenant trigger. The quarterly standard deviation is estimated using the past 12 quarters. Finally, I calculate a loan's covenant tightness as the average tightness across its financial covenants.[5]

I measure financial covenant intensity by counting the number of financial covenants that are attached to a loan. Table 1 details the various types of covenants in the sample. Financial covenants are grouped into six categories following Nini et al. (2009): debt to balance sheet, coverage, debt to cash flow, liquidity, net worth, and EBITDA covenants. These covenants restrict the maximum indebtedness the borrower is allowed to incur or require the

[4] According to Carey and Hrycay (1999), DealScan covers between 50% and 75% of all commercial loans (by value) in the U.S. in the early 1990s and coverage further increases thereafter.

[5] See also Freudenberg et al. (2015) for a similar measure. Note that data on intangible assets are frequently unavailable in Compustat Quarterly, which poses a problem in estimating the tightness of covenants related to tangible net worth. I remedy this problem by measuring covenant slack as of the end of the previous fiscal year and using the median standard deviation of the financial ratio for all firms in the borrower's two-digit Standard Industrial Classification (SIC) industry whose size is within a range of plus or minus 25% of the borrower's assets. In addition, as mentioned by other researchers (e.g., Dichev and Skinner, 2002; Chava and Roberts, 2008; Murfin, 2012), some covenants appear to be violated at origination, which could be due to heterogeneity in covenant definitions and unmeasurable adjustments. I exclude such covenants from the analysis, and measure tightness using the data on the loan's remaining covenants.

Table 1
Frequency of financial and nonfinancial covenant types.
This table shows the frequency of inclusion of the different covenant types reported in DealScan for the sample of loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995 to 2008 for which covenant information is available.

	Percent
Financial covenants	
Debt to equity covenant	0.76
Debt to tangible net worth covenant	10.71
Leverage ratio covenant	17.60
Loan to value covenant	0.11
Senior leverage covenant	0.15
Any debt to balance sheet covenant	28.87
Cash interest coverage covenant	1.27
Debt service coverage covenant	8.06
Fixed charge coverage covenant	40.41
Interest coverage covenant	41.07
Any coverage covenant	79.47
Debt to EBITDA covenant	57.37
Senior debt to EBITDA covenant	10.93
Any debt to cash flow covenant	59.81
Current ratio covenant	11.24
Quick ratio covenant	2.33
Any liquidity covenant	13.48
Net worth covenant	22.78
Tangible net worth covenant	19.99
Any net worth covenant	42.77
EBITDA covenant	9.24
Nonfinancial covenants	
Asset sales sweep	35.26
Equity issuance sweep	23.59
Debt issuance sweep	25.74
Any sweep provision	38.10
Capital expenditure restriction	22.30
Dividend covenant	77.80
Observations	7924

maintenance of certain minimum coverage or liquidity ratios or of a minimum net worth or EBITDA. Among financial covenants, coverage covenants are the most frequent, with 79% of all loans containing at least one such covenant. Debt to cash flow covenants and net worth covenants are included in 60% and 43% of the loans, respectively.

Nonfinancial covenants include sweep provisions, dividend restrictions, and capital expenditure restrictions. Sweep provisions require the borrowing firm to repay part or all of the loan prematurely if it takes certain actions such as, in the case of an asset sale sweep, selling assets in excess of certain allowances. Close to 80% of all loans have a dividend restriction, while slightly more than one-fifth of the loans have a capital expenditure restriction, and 38% of the loans carry a sweep provision.

Online Appendix Table A-1 shows that both covenant intensity and tightness determine the frequency with which control shifts to creditors. Both variables strongly predict actual covenant violations over the course of the loan using the covenant violation data from Nini et al. (2012). Moreover, the coefficient for covenant intensity in the predictive regression is almost entirely unaffected by the presence of the tightness variable and vice versa, suggesting that tightness and intensity are not mere substitutes.

To determine relationship status for a given loan, I focus on the loan's lead arranger since the lead arranger acts as an intermediary between the borrower and the participant lenders and is better informed (Ivashina, 2009; Guerin, 2007). I designate as lead arrangers any lender for which the field "lead arranger credit" is marked "Yes" in DealScan as well as the lenders of all sole lender loans. In addition, I search the field "lender roles" and define the following roles as lead arrangers: agent, administrative agent, arranger, lead bank. I then identify all instances in DealScan in which the borrower obtained funding in the five years prior to the current loan and measure relationship intensity as the percentage of previously borrowed amounts arranged by the same bank:[6]

$$\textit{Relation (Maximum)} = \max_k \frac{\sum_j \text{Loan amount}_j * I(k)}{\sum_j \text{Loan amount}_j}, \quad (2)$$

where $I(k)$ indicates lead arranger k's participation in loan j. This measure is designed to capture the information distance between the current lender and other lenders. For loans with multiple lead arrangers I take the maximum of this ratio across all lead arrangers since the monitoring effort is likely to be led by the best informed bank. The measure is undefined if there are no loans in the previous five years. Setting relationship intensity to zero for a firm's first loan does not change the results, however. I track bank mergers using the Federal Reserve's National Information Center and attribute an acquired bank's relationships to the surviving entity. Relationships are tracked at the individual bank level, but results are not sensitive to aggregating to the bank holding company level.

As an alternative to the above measure, the following relationship intensity measure gives equal credit to each lead arranger involved in the loan and calculates the sum of relationship intensities across lead arrangers:

$$\textit{Relation (Weighted)} = \sum_k \frac{\sum_j \text{Loan amount}_j / N_j * I(k)}{\sum_j \text{Loan amount}_j}, \quad (3)$$

where N_j indicates the number of lead arrangers for loan j. Unlike the *Relation (Maximum)* measure, this measure equals one only if there is no lender outside of the current syndicate who has ever arranged a loan for the firm. Nonetheless, the two measures are highly correlated and lead to very similar results. Finally, *Relation (Duration)* is the time elapsed between the borrower's first interaction with the lead arranger in the DealScan database and the current loan. This variable captures the lender's knowledge of the borrower in absolute terms, rather than comparing the current lender's knowledge with that of other lenders.

To measure relationship intensity and duration as accurately as possible, the above measures are based on all of a borrower's loans, including those that are not in the final sample due to missing information. Table 2 shows the number of loans available per firm for the final sample and for the sample used to determine relationship in-

[6] This measure is also used by Bharath et al. (2011) and Schenone (2010). The former paper also provides a detailed description of the aforementioned lender roles.

Table 2
Comparison of the number of loans per firm used in the final sample and the number of loans used to determine relationship intensity.
This table compares the number of loans per firm that enter the final sample with the number of loans per firm that are available in the sample used to determine relationship intensity. Figures shown are for the firms that have at least one loan that enters the final sample. There is no firm with only one loan in the relationship sample since the definition of relationship intensity requires at least two loans (relationship intensity cannot be determined for the first loan agreement that a firm enters). The final sample includes 7,924 loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995 to 2008.

	Final sample		Relationship sample	
Loans per firm	Number	Percent	Number	Percent
1	1293	40.78	0	0.00
2	737	23.24	753	23.75
3	406	12.80	603	19.02
4	287	9.05	444	14.00
5	192	6.05	332	10.47
6	116	3.66	275	8.67
7	60	1.89	209	6.59
8	44	1.39	159	5.01
9	17	0.54	107	3.37
10	8	0.25	78	2.46
11	7	0.22	55	1.73
12	1	0.03	42	1.32
13	1	0.03	30	0.95
14	2	0.06	22	0.69
15 or more	0	0.00	62	1.96
Total	3171	100.00	3171	100.00

tensity. The median firm has two loans in the final sample and four loans in the sample used to determine relationship status. The online Appendix discusses the reasons why certain loans drop out of the final sample and performs robustness checks to ensure that this does not affect results.

Table 3 presents univariate tests of differences in firm and loan characteristics conditional on relationship status. In Panel A, loan observations are sorted on relationship intensity. Relationship intensity is categorized as “low” if *Relation (Maximum)* is less than 30%, “high” if it is more than 70%, and “medium” if it is in between. The results show that covenant tightness is about three percentage points lower for loans with high relationship intensity compared to those with low intensity. Financial covenant use increases by about 4% from the low to the medium category and decreases by about 7% from the medium to the high category. These differences are highly statistically significant. Yield spread, nonfinancial covenant use, and collateral requirements decrease in relationship intensity whereas maturity is largely unchanged. In Panel B of Table 3, loan observations are sorted on relationship duration. The first column contains loans with no previous relationship, whose relationship duration is zero. Relationship duration is categorized as “short” if it is less than the median duration among relationship loans, and “long” otherwise. In these sorts, covenant tightness is about five percentage points lower for loans with long relationship duration than for nonrelationship loans. Covenant intensity is about 3% higher in short duration relationships than in loans with no previous relationship, but declines by about 12% from short duration to long duration relationships. However, Table 3 also shows that firm characteristics are correlated with both relationship intensity and duration. Firms with strong relationship status are larger, more likely to be a member of the Standard and Poor’s (S&P) 500, more likely to be rated, have better ratings, and have a lower current ratio. Therefore, I now turn to multiple regressions.

4. Multiple regressions for covenant tightness

This section explores the effects of relationship lending on covenant tightness. Table 4 shows results for ordinary least squares (OLS) regressions of covenant tightness on relationship intensity and relationship duration. Controls include loan and firm characteristics as well as industry fixed effects at the one-digit SIC industry level, year fixed effects, and loan purpose and loan type fixed effects. The top and bottom 1% of all financial ratios are winsorized to reduce the impact of outliers. Column 1 shows that tightness is negatively related to relationship intensity as measured by *Relation (Maximum)*. In addition, covenants are tighter for borrowers that are small, have high leverage, few tangible assets, low current ratios and coverage ratios, and poor ratings. Column 2 replaces the relationship intensity measure with two indicator variables to allow capturing any nonlinear effect. *Low relation* and *High relation* equal one if relationship intensity is below 30% and at least 70%, respectively. Consequently, loans with medium relationship intensities act as the base group. Column 2 shows that loans with low relationship intensity have significantly tighter covenants than loans with medium relationship intensity. Column 3 suggests that the decline in tightness appears to be driven by the duration of a lending relationship rather than the information distance between the current lender and other banks. When relationship duration and the information advantage measure *Relation (Maximum)* both enter the same regression, the coefficient on relationship duration is negative and significant at the 1% level, while *Relation (Maximum)* is not significant. This result is consistent with the setup of the theoretical model in Gârleanu and Zwiebel (2009), who study the evolution of covenant strictness over the course of a relationship rather than differences across different relationships. Columns 4 through 6 show additional evidence consistent with the information asymmetry theory. These regressions include the interaction of relationship duration with dummy variables indicating the presence of a rating, access to the commercial paper market, as evidenced by a short-term rating of A-2 or better (Murfin, 2012), and total assets below the sample median. The results show that the decline in covenant tightness over the course of a lending relationship is concentrated in small, unrated borrowers and nonexistent for borrowers with access to the commercial paper market.

There is reason to expect OLS estimates of the relationship effect on covenant tightness to be biased. As discussed earlier, Demiroglu and James (2010) show that covenants are set more tightly when borrowers and lenders have private information indicating future improvements in the firm’s financial performance, whereas covenant intensity does not exhibit this pattern. If positive private informa-

Table 3

Univariate tests of differences in firm characteristics and debt issue characteristics conditional on relationship status.

This table presents averages and univariate *t*-tests of differences in covenant use, loan, and firm characteristics across loans with different relationship status for the sample of 7,924 loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995–2008. In Panel A, loans are sorted on relationship intensity as measured by *Relation (Maximum)*. I classify relationship intensity as "low" if relationship intensity is less than 30%, "high" if it is at least 70%, and "medium" if relationship intensity is between 30% and 70%. In Panel B, loans are sorted on relationship duration. Column "Zero" shows loans with no prior relationship. The median relationship duration among relationship loans is 3.95 years. Relationship loans are split into "short" and "long" duration groups based on whether their relationship duration is below or above this median. *Relation (Maximum)* and *Relation (Weighted)* denote the proportion of the total amount borrowed in the previous five years where the current lead arranger acted as a lead arranger. For previous loans with multiple lead arrangers, *Relation (Maximum)* denotes the maximum of the lead arrangers' relationship intensities. *Relation (Weighted)* gives each of *N* lead arrangers on a previous loan credit for 1/*N* times the amount of the loan and takes the sum of relationship intensities across lead arrangers. *Relation (Duration)* indicates the number of years elapsed since the borrower first obtained a loan arranged by the same bank. *Tightness* denotes the average ex ante probability of violation for all financial covenants in a deal. This probability is estimated by evaluating the cumulative normal distribution function using the slack of the covenant at origination divided by the standard deviation of the corresponding financial ratio over the previous 12 quarters. *FinCov* and *NonFinCov* count the number of financial and nonfinancial covenants included in the loan, respectively. *Maturity* and *AllInDrawn* denote the weighted average maturity and yield spread over the London Interbank Offered Rate (LIBOR) for each dollar drawn down on the loan, respectively. *Collateral* is a dummy variable that equals one if at least one of the facilities that form a loan is secured and zero otherwise. *Loan amount* is the total amount of the deal, and *Assets* are the borrowing firm's total assets. All dollar amounts are converted to 2008 US dollars using the Consumer Price Index for all urban consumers. *Leverage* is defined as the book value of debt divided by total assets. *Tangibility* is the ratio of net property, plant, and equipment to total assets. *Rating* is a categorical variable that equals zero if the firm has no S&P long-term issuer credit rating, 1, 2, 3, 4, if the rating is AAA, AA+, AA, AA-, respectively, and so on. *Not rated* is a dummy variable that equals one if the firm has no S&P rating. *MTB* is the market-to-book ratio, calculated as the market value of the firm's shares outstanding plus the book value of debt and preferred stock divided by the book value of assets. *Current ratio* is the ratio of current assets to current liabilities and *Coverage ratio* is calculated as EBITDA divided by interest expense. Membership in the S&P 500 index is indicated by the dummy variable *S&P 500*. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Relationship intensity sort

	Low	Medium	High	M – L	H – M	H – L
Relation (Maximum)	0.0198	0.5193	0.9646	0.4995***	0.4453***	0.9448***
Relation (Weighted)	0.0169	0.4739	0.8890	0.4570***	0.4151***	0.8721***
Relation (Duration)	0.8436	3.9575	5.6373	3.1139***	1.6798***	4.7937***
FinCov	2.5781	2.6733	2.4838	0.0952*	−0.1895***	−0.0943***
Tightness	0.1292	0.1128	0.0984	−0.0164**	−0.0145**	−0.0308***
NonFinCov	1.9577	1.9906	1.7377	0.0329	−0.2529***	−0.2200***
AllInDrawn	212.8762	185.1955	158.8888	−27.6808***	−26.3067***	−53.9875***
Maturity	45.0775	46.8121	45.5454	1.7346*	−1.2667	0.4679
Collateral	0.7141	0.6262	0.5470	−0.0880***	−0.0792***	−0.1672***
Loan amount	337.4239	568.9049	630.9992	231.4810***	62.0943	293.5753***
Assets	1559.6816	3631.1574	4256.5766	2071.4758***	625.4191	2696.8950***
Leverage	0.3125	0.3486	0.3017	0.0361***	−0.0469***	−0.0108*
Tangibility	0.3310	0.3500	0.3325	0.0190*	−0.0175*	0.0015
Rating	11.9314	11.4875	10.5904	−0.4439**	−0.8971***	−1.3410***
Not rated	0.6401	0.4545	0.4806	−0.1857***	0.0262	−0.1595***
MTB	1.3595	1.3993	1.4618	0.0398	0.0625	0.1023***
Current ratio	2.0222	1.9315	1.8770	−0.0907	−0.0545	−0.1452***
Coverage ratio	18.6227	13.7248	19.8880	−4.8979***	6.1631***	1.2652
S&P 500	0.0807	0.1644	0.1909	0.0837***	0.0266*	0.1102***
Observations	2837	955	4132			

Panel B: Relationship duration sort

	Zero	Short	Long	S – Z	L – S	L – Z
Relation (Maximum)	0.0000	0.7940	0.8442	0.7940***	0.0502***	0.8442***
Relation (Weighted)	0.0000	0.7331	0.7737	0.7331***	0.0406***	0.7737***
Relation (Duration)	0.0000	2.0179	8.6197	2.0179***	6.6019***	8.6197***
FinCov	2.6028	2.6729	2.3542	0.0702*	−0.3188***	−0.2486***
Tightness	0.1314	0.1190	0.0858	−0.0124**	−0.0332***	−0.0456***
NonFinCov	1.9727	1.9628	1.6227	−0.0099	−0.3401***	−0.3500***
AllInDrawn	218.6560	183.8708	147.0592	−34.7853***	−36.8116***	−71.5968***
Maturity	44.8668	45.7355	45.8969	0.8687	0.1614	1.0301
Collateral	0.7294	0.6567	0.4787	−0.0728***	−0.1780***	−0.2507***
Loan amount	283.8898	491.3441	747.3098	207.4542***	255.9657***	463.4199***
Assets	1246.2577	2759.0818	5367.1521	1512.8240***	2608.0704***	4120.8944***
Leverage	0.3071	0.3193	0.3066	0.0122*	−0.0127*	−0.0005
Tangibility	0.3269	0.3397	0.3346	0.0128	−0.0051	0.0077
Rating	12.0065	11.5042	10.3913	−0.5023***	−1.1129***	−1.6152***
Not rated	0.6774	0.5690	0.3773	−0.1085***	−0.1917***	−0.3002***
MTB	1.3734	1.4954	1.3778	0.1220***	−0.1176***	0.0044
Current ratio	2.0379	1.9575	1.8256	−0.0804*	−0.1319***	−0.2124***
Coverage ratio	19.3010	19.4925	17.3680	0.1915	−2.1245	−1.9330
S&P 500	0.0675	0.1155	0.2505	0.0480***	0.1350***	0.1830***
Observations	2384	2770	2770			

Table 4
The effect of relationship intensity and duration on covenant tightness.
This table shows OLS regressions of covenant tightness on relationship intensity, relationship duration, and control variables for the sample of loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995–2008. Covenant tightness is estimated as follows. For each covenant type, the probability of a covenant violation is estimated by evaluating the cumulative normal distribution function using the slack of the covenant in the quarter immediately prior to the start date of the deal divided by the standard deviation of the corresponding financial ratio over the previous 12 quarters. Each loan's tightness is calculated as the average tightness of the loan's financial covenants. Since information on intangible assets is frequently missing in Compustat Quarterly, I substitute the information for tangible net worth covenants and debt to tangible net worth covenants with the annual slack divided by the median standard deviation of the financial ratio for comparable firms with quarterly data. Comparable firms are defined as firms in the same two-digit SIC industry with total assets that differ by no more than plus or minus 25% from the total assets of the borrower. Relationship intensity is measured as *Relation (Maximum)* defined in Eq. (2). *Low relation* and *High relation* refer to a relationship intensity of less than 30% and more than 70%, respectively. *Ln(Relation (Duration))* is the natural logarithm of one plus the number of years elapsed since the borrower first obtained a loan arranged by the same bank. *CP access* indicates that the borrower has access to the commercial paper market (proxied by a short-term credit rating of A-2 or better). *Small borrower* is a dummy variable indicating that the borrower's size is below the median size of borrowers in the same year. The independent variables are defined in Table 3. All regressions control for industry fixed effects at the one-digit SIC level, year fixed effects at the respective loan's origination date, as well as loan purpose and loan type fixed effects. Numbers in parentheses are *t*-statistics adjusted for heteroskedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Relation	−0.0105***		0.0024			
	(−2.84)		(0.46)			
Low relation		0.0101**				
		(2.05)				
High relation		0.0003				
		(0.08)				
Ln(Relation (Duration))			−0.0095***	−0.0129***	−0.0100***	−0.0033
			(−3.58)	(−5.10)	(−5.13)	(−1.40)
Ln(Relation (Duration)) × Rated				0.0085**		
				(2.42)		
Ln(Relation (Duration)) × CP access					0.0121**	
					(2.44)	
Ln(Relation (Duration)) × Small borrower						−0.0112***
						(−3.25)
CP access					−0.0210*	
					(−1.96)	
Small borrower						0.0270***
						(4.01)
Ln(Loan amount)	−0.0030	−0.0030	−0.0035	−0.0031	−0.0032	−0.0034
	(−1.05)	(−1.04)	(−1.21)	(−1.08)	(−1.13)	(−1.17)
Ln(Maturity)	−0.0202***	−0.0203***	−0.0202***	−0.0204***	−0.0205***	−0.0201***
	(−5.19)	(−5.21)	(−5.19)	(−5.24)	(−5.26)	(−5.19)
Ln(Lenders)	−0.0015	−0.0015	−0.0012	−0.0012	−0.0010	−0.0007
	(−0.62)	(−0.61)	(−0.47)	(−0.46)	(−0.42)	(−0.30)
Ln(Assets)	−0.0062**	−0.0062**	−0.0052**	−0.0052**	−0.0053**	−0.0015
	(−2.40)	(−2.40)	(−1.98)	(−2.01)	(−2.04)	(−0.51)
Leverage	0.0659***	0.0661***	0.0664***	0.0668***	0.0667***	0.0657***
	(4.84)	(4.84)	(4.89)	(4.92)	(4.89)	(4.82)
Tangibility	−0.0457***	−0.0457***	−0.0456***	−0.0450***	−0.0451***	−0.0447***
	(−5.25)	(−5.25)	(−5.27)	(−5.19)	(−5.20)	(−5.17)
Current ratio	−0.0075***	−0.0075***	−0.0074***	−0.0073***	−0.0074***	−0.0073***
	(−5.02)	(−4.99)	(−4.96)	(−4.89)	(−4.96)	(−4.87)
Ln(1+Coverage ratio)	−0.0213***	−0.0213***	−0.0213***	−0.0210***	−0.0212***	−0.0210***
	(-8.73)	(-8.75)	(-8.78)	(-8.63)	(-8.70)	(-8.63)
Rating	0.0070***	0.0071***	0.0069***	0.0072***	0.0068***	0.0070***
	(6.22)	(6.25)	(6.06)	(6.35)	(5.44)	(6.17)
Not rated	0.0946***	0.0949***	0.0920***	0.1060***	0.0915***	0.0920***
	(7.14)	(7.16)	(6.94)	(7.38)	(6.20)	(6.92)
MTB	−0.0032	−0.0032	−0.0035*	−0.0035*	−0.0036*	−0.0033*
	(-1.59)	(-1.57)	(-1.76)	(-1.76)	(-1.77)	(-1.65)
S&P 500	−0.0001	0.0001	0.0003	−0.0007	0.0001	−0.0033
	(-0.01)	(0.02)	(0.05)	(-0.11)	(0.02)	(-0.52)
Constant	0.1331***	0.1230***	0.1309***	0.1222***	0.1333***	0.0947***
	(3.76)	(3.47)	(3.63)	(3.41)	(3.63)	(2.59)
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan type effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6947	6947	6947	6947	6947	6947
Adj. R-squared	0.173	0.173	0.175	0.176	0.176	0.177

tion increases the probability of continuing a lending relationship, OLS estimates of the relationship effect would be biased towards zero. Alternatively, if the analysis omitted some variable that is correlated with both longer relationship duration and looser covenants, OLS estimates could be overstated. I address this problem using an instrumental variables (IV) approach.

The key to IV estimation is to find an instrument that is correlated with relationship status, but has no effect on covenant choice other than through relationship status. A number of papers argue that geographical proximity between borrowers and lenders fosters the gathering and processing of firm-specific information and hence relationship formation (e.g., Bharath et al., 2011; Berger et al., 2005; Dass and Massa, 2011). These arguments imply that relationship duration is partly a function of historical location. I exploit this proximity effect by using the distance between the borrower and the nearest syndication active bank as an instrument. I define a bank as syndication active in year *t* if it acts as lead arranger for at least 30 loans in DealScan during that year. Importantly, this instrument does not condition on whether the borrower actually obtains a loan from a nearby bank since that decision could be subject to the same endogeneity issues as relationship choice itself. As a result, the identification strategy rests on the assumption that borrowers do not have control over whether or not nearby banks actively syndicate loans. One caveat to this strategy is that borrowers do have a choice of where to locate their headquarters. It seems unlikely that firms would incur the expense of relocating headquarters just to be closer to a relationship bank. However, to the extent that firms' location choice is correlated with unobserved firm characteristics that are also correlated with covenant choice, the IV approach cannot fully rule out an omitted variables alternative.

Historical addresses of borrowers' headquarters are obtained from the headers of the firms' 10-K filings using DirectEDGAR.[7] Historical lender addresses are from Call Reports and the National Information Center (NIC) of the Federal Reserve System. Bank mergers pose a challenge to using the addresses of banks' headquarters. Since I assume that relationships are inherited by the successor institution, a bank merger could increase the distance between the borrower and the closest potential lender even as relationship duration continues to increase, spuriously weakening the instrumental variable. To deal with this concern, I include the addresses of the currently syndication active banks' predecessor institutions that were syndication active at or after the time of the borrower's first loan in DealScan. I then translate these data into geographical longitudes and latitudes using the WebGIS application provided by Texas A&M University at https://geoservices.tamu.edu/. Finally, I compute the log of one plus the spherical distance in miles between the borrower and each syndication active bank using the formula given in Dass and Massa (2011) and find the bank with the lowest distance.

Table 5 shows the results from the IV estimation. Column 1 reports the first-stage coefficient for the geographic instrument. Borrowers that are located closer to a syndication active bank have significantly longer relationship duration. Column 2 shows that the relationship coefficient is much larger than in the OLS regression and is negatively significant at the 5% level. Two instrument weakness tests are reported: the Cragg-Donald *F*-statistic, which assumes homoskedastic errors, and the Kleibergen-Paap rk *F*-statistic, which is robust to firm-level clustering of standard errors. The Stock and Yogo (2005) critical value for instrument weakness is reported as well. If the Cragg-Donald *F*-statistic is larger than this critical value, one rejects the hypothesis that the actual maximal size of a 5% Wald test of joint significance of the endogenous regressors exceeds 10%. Critical values for the Kleibergen-Paap rk *F*-statistic are not available in the literature to the best of my knowledge. The Cragg-Donald statistic has a value of 27.90, strongly rejecting instrument weakness. The Kleibergen-Paap statistic is smaller with a value of 15.46. Columns 3 through 8 show results of IV estimation for the interaction of relationship intensity with measures of information asymmetries. As discussed in Wooldridge (2002), it is easy to construct an additional instrument for these interaction terms: one simply uses a linear regression to predict relationship duration, then interacts the predicted values with the information asymmetry measure and uses this fitted interaction term as an instrument in the first stage of the two-stage least squares (2SLS) estimation.

Columns 3 through 5 show that the reduction in covenant tightness is again concentrated in small, unrated borrowers that do not have access to the commercial paper market. These results are statistically significant. Covenant definitions can vary across loans, which likely induces noise in the estimation. For this reason, columns 6 through 8 present estimates based on a tightness measure that only uses covenants for which the definitions are relatively standard across loans: net worth, tangible net worth, EBITDA, debt to EBITDA, and liquidity covenants.[8] These are likely to be measured with less noise. As shown in columns 6 through 8, coefficients are larger when focusing on these covenant types. All interaction terms remain statistically significant.

5. Multiple regressions for covenant intensity

This section reports results for covenant intensity. It then discusses potential explanations for these results and performs additional tests to assess these explanations as well as control for endogeneity.

[7] I thank Burch Kealey for help with these data.

[8] To determine which covenants to include, I read the covenant definitions in the loan contracts collected by Nini et al. (2009). For each covenant type I randomly sample 20 contracts containing the covenant from the intersection of my sample with their sample. I classify covenants as relatively standard if at least 15 of the 20 covenants use the same definition (disregarding unmeasurable non-generally accepted accounting principles (GAAP) adjustments). For all covenant types that do not meet this criterion, less than ten covenants use the same definition, hence this seems a natural cutoff point. Also note that recent studies focus on the same covenant types as they are easier to measure e.g., Chava and Roberts (2008) use net worth, tangible net worth, and current ratio covenants, and Demiroglu and James (2010) use current ratio and debt-to-EBITDA covenants.

Table 5
Instrumental variables regressions for the effect of relationship duration on covenant tightness.
This table shows 2SLS estimates of the effect of relationship duration on covenant tightness for the sample of loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995–2008. Columns 1 through 5 show results using the tightness measure as defined in Table 4. Columns 6 through 8 measure tightness using only those covenants for which definitions across loans are relatively standardized (i.e., out of a random sample of 20 covenants, at least 15 use the same definition (disregarding non-GAAP adjustments)): net worth, tangible net worth, EBITDA, debt to EBITDA, and liquidity covenants. For all other financial covenant types, less than ten contracts use the same definition. The instrumental variable is the log of one plus the distance between the borrower and the nearest syndication active bank. A bank is defined as syndication active in a given year if it acted as lead arranger for at least 30 deals in DealScan during that year. Column 1 shows the first-stage regression of relationship duration on the instrument. Column 2 shows the second stage of the 2SLS estimation. The interaction terms in columns 3 through 8 are instrumented by regressing the log of one plus relationship duration on the log of the distance between the borrower and the nearest syndication active bank as well as the control variables, and interacting the predicted value for the log of one plus relationship duration with the interaction variable. The resulting variable is used as an instrument in the first stage of the 2SLS estimation, as discussed in Wooldridge (2002). *CP access* indicates that the borrower has access to the commercial paper market (proxied by a short-term credit rating of A-2 or better). *Small borrower* is a dummy variable indicating that the borrower's size is below the median size of borrowers in the same year. Control variables are the same as in Table 4. All regressions control for industry fixed effects at the one-digit SIC level, year fixed effects at the respective loan's origination date, as well as loan purpose and loan type fixed effects. Numbers in parentheses are *t*-statistics adjusted for heteroskedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	1st Stage	Tightness				Tightness (Narrow def.)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln(Closest bank distance)	−0.0304***							
	(−3.93)							
Ln(Relation (Duration))		−0.0772**	−0.0987**	−0.0757**	−0.0487	−0.1178**	−0.0887*	−0.0486
		(−2.09)	(−2.46)	(−2.14)	(−1.48)	(−2.09)	(−1.76)	(−1.05)
Ln(Relation (Duration)) × Rated			0.0533***			0.0783***		
			(3.74)			(4.09)		
Ln(Relation (Duration)) × CP access				0.0307**			0.0582***	
				(2.01)			(2.99)	
Ln(Relation (Duration)) × Small borrower					−0.0610***			−0.0825***
					(−4.62)			(−4.54)
CP access				−0.0486*			−0.1031***	
				(−1.69)			(−3.12)	
Small borrower					0.0756***			0.0865***
					(5.08)			(4.47)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6502	6502	6502	6502	6502	5242	5242	5242
Weak ID tests								
Cragg-Donald *F*		27.90	14.31	14.53	13.78	8.81	8.90	8.44
Kleibergen-Paap rk *F*		15.46	8.30	8.21	8.08	5.29	5.34	5.19
Stock-Yogo (2005) crit.		16.38	7.03	7.03	7.03	7.03	7.03	7.03

5.1. Poisson regressions

Since the number of financial covenants is a count variable, I test the effect of relationship intensity on financial covenant intensity by estimating Poisson regressions. One way to allow for a potential nonlinear effect is to use a quadratic form:

$$\log(\text{FinCov}_i) = \alpha_1 + \beta_1 \,\text{Relation}_i + \gamma_1 \,\text{Relation}_i^2 + \delta_1 \,\text{Controls}_i + \epsilon_{1,i}\,, \tag{4}$$

where *FinCov* is the number of financial covenants included in a deal and *Relation* is one of the two relationship intensity measures. Since the linear and squared terms of relationship intensity are highly correlated, one may be concerned that regression estimates are an artifact of this correlation. In addition, any nonlinearities need not necessarily be quadratic. Therefore, I focus on presenting results using indicator variables for low and high relationship intensity, that is, a relationship intensity below 30% and at least 70%, respectively:

$$\log(\text{FinCov}_i) = \alpha_2 + \beta_2 \,\text{Low relation}_i + \gamma_2 \,\text{High relation}_i + \delta_2 \,\text{Controls}_i + \epsilon_{2,i}. \tag{5}$$

Table 6 shows the results. The effect of relationship intensity on financial covenant intensity appears to follow an inverted U shape. For the quadratic specifications in columns 1 and 5, the linear term is significantly positive and the quadratic term significantly negative. For the dummy variable specifications in columns 2 and 6, both the low and high relationship dummies indicate a significantly lower covenant intensity compared to loans with medium relationship intensity. Column 3 investigates the effect of relationship duration on financial covenant use. Again, the results suggest an inverted U effect. Columns 4 and 7 allow for a horse race between relationship duration and the two relationship intensity indicators. In these regressions, the coefficients for the intensity measures remain highly similar, while dura-

Table 6
The effect of relationship intensity on financial covenant use.
This table reports Poisson regressions of financial covenant intensity on relationship intensity and control variables for the sample of loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995–2008. Regressions in columns 1 through 4 use *Relation (Maximum)* as the measure of relationship intensity, and regressions in columns 5 through 7 use *Relation (Weighted)*. *Low relation* and *High relation* refer to a relationship intensity of less than 30% and more than 70%, respectively. *Ln(Relation (Duration))* is the natural logarithm of one plus the number of years elapsed since the borrower first obtained a loan arranged by the same bank. Control variables are the same as in Table 4. All regressions control for industry fixed effects at the one-digit SIC level, year fixed effects at the respective loan's origination date, as well as loan purpose and loan type fixed effects. Numbers in parentheses are *z*-statistics adjusted for heteroskedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Relation (Maximum)				Relation (Weighted)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Relation	0.2246***				0.1964***		
	(4.33)				(4.10)		
$(\text{Relation})^2$	−0.1829***				−0.1546***		
	(−3.51)				(−3.24)		
Low relation		−0.0730***		−0.0669***		−0.0647***	−0.0544***
		(−5.39)		(−3.60)		(−5.36)	(−3.29)
High relation		−0.0333**		−0.0349**		−0.0277**	−0.0292**
		(−2.41)		(−2.50)		(−2.30)	(−2.42)
Ln(Relation (Duration))			0.0493***	0.0058			0.0143
			(3.19)	(0.24)			(0.64)
$[\text{Ln(Relation (Duration))}]^2$			−0.0129*	0.0002			−0.0026
			(−1.95)	(0.02)			(−0.33)
Ln(Loan amount)	−0.0160*	−0.0162*	−0.0150*	−0.0160*	−0.0161*	−0.0160*	−0.0158*
	(−1.89)	(−1.91)	(−1.77)	(−1.89)	(−1.90)	(−1.89)	(−1.87)
Ln(Maturity)	0.0140	0.0140	0.0131	0.0140	0.0141	0.0138	0.0139
	(1.21)	(1.21)	(1.13)	(1.21)	(1.21)	(1.19)	(1.20)
Ln(Lenders)	0.0418***	0.0425***	0.0429***	0.0424***	0.0418***	0.0427***	0.0424***
	(5.73)	(5.84)	(5.89)	(5.82)	(5.73)	(5.86)	(5.82)
Ln(Assets)	−0.0255***	−0.0250***	−0.0263***	−0.0256***	−0.0255***	−0.0252***	−0.0260***
	(−3.43)	(−3.37)	(−3.53)	(−3.43)	(−3.44)	(−3.40)	(−3.48)
Leverage	0.0447	0.0453	0.0435	0.0447	0.0440	0.0453	0.0441
	(1.28)	(1.29)	(1.24)	(1.28)	(1.26)	(1.30)	(1.26)
Tangibility	−0.0688***	−0.0688***	−0.0688***	−0.0689***	−0.0685**	−0.0688***	−0.0688***
	(−2.58)	(−2.58)	(−2.58)	(−2.59)	(−2.57)	(−2.58)	(−2.58)
Current ratio	0.0090**	0.0091**	0.0090**	0.0090**	0.0092**	0.0092**	0.0091**
	(2.16)	(2.19)	(2.15)	(2.17)	(2.20)	(2.21)	(2.19)
Ln(1+Coverage ratio)	0.0219***	0.0218***	0.0213***	0.0218***	0.0216***	0.0217***	0.0217***
	(3.41)	(3.40)	(3.31)	(3.40)	(3.37)	(3.39)	(3.38)
Rating	0.0271***	0.0273***	0.0274***	0.0274***	0.0273***	0.0275***	0.0275***
	(7.36)	(7.41)	(7.43)	(7.44)	(7.41)	(7.49)	(7.49)
Not rated	0.3687***	0.3696***	0.3709***	0.3715***	0.3709***	0.3724***	0.3733***
	(7.78)	(7.81)	(7.82)	(7.85)	(7.82)	(7.88)	(7.89)
MTB	−0.0099*	−0.0100*	−0.0091	−0.0097*	−0.0097*	−0.0095*	−0.0092
	(−1.74)	(−1.75)	(−1.60)	(−1.70)	(−1.70)	(−1.68)	(−1.63)
S&P 500	−0.1699***	−0.1698***	−0.1680***	−0.1701***	−0.1694***	−0.1689***	−0.1691***
	(−7.65)	(−7.65)	(−7.57)	(−7.69)	(−7.61)	(−7.59)	(−7.62)
Constant	0.6397***	0.7125***	0.6478***	0.7080***	0.6402***	0.7013***	0.6935***
	(6.74)	(7.40)	(6.78)	(7.32)	(6.74)	(7.31)	(7.20)
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7924	7924	7924	7924	7924	7924	7924

tion loses its significance. Covenant intensity appears to be driven by the intensity of the relationship relative to that of other lenders rather than the absolute length of the relationship.

Table 6 also shows that financial covenant use decreases in both loan size and firm size and increases in the number of lenders participating in the loan. The coefficient of leverage is positive, but not significant. Both the current ratio and the coverage ratio enter positively in the regression. Many covenants are written on ratios related to these two. Such covenants are potentially more informative if these ratios are above a certain threshold.[9] Firms with a worse credit rating or no credit rating at all are subject to more covenants, while loans to members of the S&P 500 carry fewer covenants.

The relationship effect is economically significant. Fig. 1 plots the effect of relationship intensity on covenant in-

[9] Current ratio covenants typically stipulate a minimum ratio of 1.0 or higher, while interest coverage ratio covenants typically stipulate a minimum of 1.25 or higher. If one excludes loans from borrowers whose ratios are below these thresholds, the current ratio and coverage ratio are no longer significant.

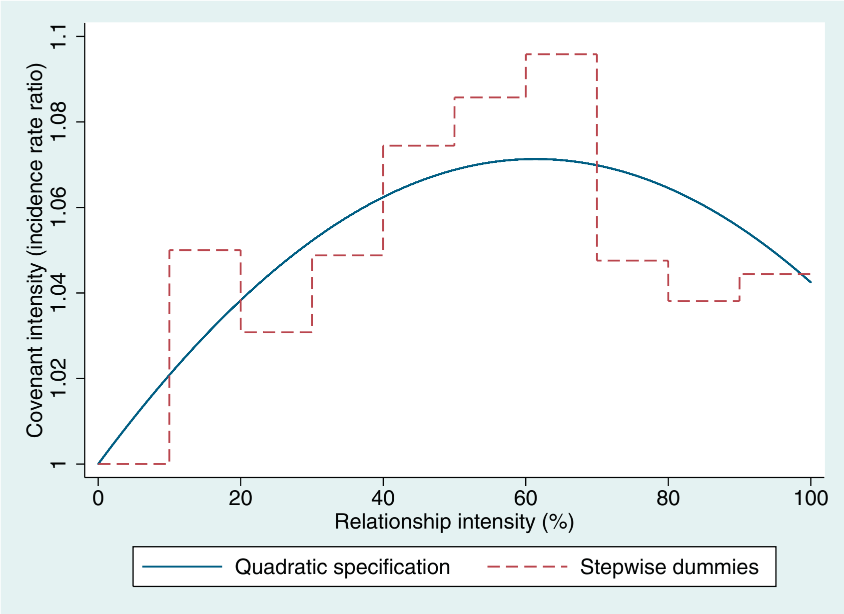

Fig. 1. Plot of the effect of relationship intensity on covenant intensity. This figure plots the incidence rate ratio of financial covenant intensity for different levels of relationship intensity. The sample includes 7,924 loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995 to 2008. The effect for the quadratic specification is plotted using the coefficients for the *Relation (Maximum)* measure from Table 6. The stepwise dummy specification shows the effect using ten dummy variables that equal one if relationship intensity is at least 0% (omitted from the regression), 10%, 20%, 30%, ..., 90%, and zero if it is below that dummy's threshold. The dummy specification (not reported in Table 6) controls for the same variables as the quadratic specification.

tensity using the quadratic specification from regression (2) in Table 6 and a stepwise dummy variable specification. Financial covenant intensity increases by about 8% from low to medium relationship intensity and decreases by about 4% from medium to high relationship intensity. These changes are equivalent to the effect of a change in the rating by two to three notches and by one to two notches, respectively. The relationship effect on covenants is also similar in size to the effect on other loan terms. For example, Bharath et al. (2011) find that a change in relationship intensity from 0% to 100% leads to a 5% decrease in the all-in spread drawn relative to their sample mean. The stepwise specification in Fig. 1 also shows that the relationship intensity thresholds of 30% and 70%, while somewhat arbitrary, capture the curve quite well. A variety of alternative cutoffs exists that yield similar or stronger results.

In sum, the results show that covenant intensity appears to be driven by relationship intensity, whereas covenant tightness is determined by the duration of the lending relationship. Considering the entire covenant package, the data suggest that medium relationship intensity lenders receive the largest number of covenants regardless of relationship duration, and as the relationship progresses, covenants become less tight.

5.2. Potential explanations

The regressions in Table 6 suggest that the effect of relationship intensity on covenant count follows an inverted U. One potential explanation for this result is that the inverted U is due to a confluence of two separate effects. It is conceivable that the downward sloping part of the inverted U is related to a reduction in information asymmetries. The increase in covenant intensity in medium intensity relationship loans could be related to lock-in effects that arise from a relationship bank's informational advantage over outside lenders (Rajan, 1992). Alternatively, the upward sloping part of the inverted U could be related to a relationship lender's incentive to preserve the value created by the relationship (Boot, 2000), which enhances the lender's willingness to renegotiate the loan and thus reduces the costs associated with covenant use compared to nonrelationship loans. All of these arguments suggest that the relationship effect should be stronger for informationally opaque borrowers that are subject to greater information asymmetries, greater potential for lock-in effects, and for whom lending relationships are likely to create more value.

Another potential explanation is related to the theory that covenants create monitoring incentives (Rajan and Winton, 1995; Park, 2000). As described in Section 2, these theories do not directly refer to a situation in which firms obtain multiple senior loans with varying degrees of covenant intensity from multiple sets of banks. However, if covenants are used as an incentive to monitor, giving strong covenants to all banks could result in a duplication of monitoring efforts and contract renegotiation. This effect could limit the contracting parties' ability to overcome the free-rider problem. To resolve this issue, it is conceivable that borrowers choose to give a stronger set of covenants to one bank lender and a less extensive set of covenants to the other bank lenders. If the additional covenants increase the likelihood that a deterioration in the borrower's financial condition will be detected before it is too late, they could enhance the chosen bank's incentive to monitor. If monitoring and/or renegotiation costs are lower in lending relationships, this argument would explain why a firm would give more covenants to the relationship lender.[10]

With the monitoring incentive interpretation, a potential explanation for the decline in covenant intensity in exclusive relationships is related to the costs and benefits of using covenants as a monitoring incentive. Covenants can create ex post costs for the borrower, for example, by imposing renegotiation costs or by allowing lenders to extract rents (Gârleanu and Zwiebel, 2009). Borrowers must trade off these costs with the benefits offered by covenants. This trade-off could explain the decline in covenant use if covenant costs are higher in exclusive relationships or covenant benefits are lower or both. The relation between covenant costs and relationship intensity is not obvious. Ex post hold-up would suggest that covenant costs increase in exclusive relationships. However, renegotiation costs are likely to be lower in lending relationships and are potentially lowest when there is one exclusive lender (Gertner and Scharfstein, 1991; Morris and Shin, 2004). Whether covenant benefits are lower in an exclusive lending relationship depends, in part, on the extent to which free-rider problems exist among senior bank lenders. If multiple bank lenders free-ride on each other, giving strong covenants to one bank could help give that bank an incentive to monitor. If a firm relies exclusively on one bank lender, the free-rider problem is diminished and the need for covenants declines. In sum, covenant use could decline in an exclusive relationship if either covenant costs increase or covenant benefits decline more strongly than covenant costs. In the following sections, I turn to tests aimed at distinguishing between the various potential explanations for the inverted U effect. However, one cannot rule out the possibility that an alternative explanation might exist that is consistent with the empirical results. Thus, the following tests also serve as reference points that such an alternative would need to be able to match.

To understand why the empirical results differ for covenant tightness and covenant intensity, it is worth examining the roles these features play in a contract. Different types of financial covenants cover different aspects of a borrower's financial performance. As a result, covenant intensity determines for what range of problems the lender can intervene, whereas covenant tightness determines how early the lender can intervene. Consider a loan contract with a maximum debt-to-EBITDA covenant. If rising market interest rates undermine the borrower's solvency, the debt-to-EBITDA covenant will not capture this problem regardless of how tight it is. An interest coverage covenant, however, would detect the problem. In contrast, choosing a tight covenant allows the borrower to commit to improvements along a certain dimension of financial performance. Previous studies suggest that borrowers use covenant tightness as a signal of expected future performance (Demiroglu and James, 2010; Li et al., 2016). The results in Section 4 add to these findings, suggesting that resolution of information asymmetries over the course of a relationship renders signaling with tight covenants less important.

With covenant tightness performing a signaling function, results for covenant intensity and tightness could differ if covenant intensity is related to monitoring incentives. With this explanation, adding additional covenants increases the number of borrower characteristics and activities that need to be monitored. As long as the lender expects the covenants to bind before the borrower is in distress, such monitoring is likely to take place regardless of exactly how tight the covenants are. By contrast, if ex post transfer of control rights is the sole function of covenants, then the contract should simply set the probability of a covenant violation to an optimal level. Increasing this probability can be achieved using either the number or the tightness of the covenants, such that the two would be interchangeable and the empirical findings should be the same for both contract features.

5.3. Bargaining power and syndicate structure

One way to assess potential reasons for the documented loan contract structure is to examine what structure borrowers with a high degree of bargaining power choose. Compared to other firms, such borrowers should have better ability to negotiate a contract that is preferable from their point of view. Access to public debt markets should strengthen a firm's bargaining power relative to its bank lender. I use the existence of an S&P long-term issuer rating as well as a firm's access to the commercial paper market as proxies for the firm's access to public debt markets.

Columns 1 and 5 of Table 7 show that the decrease in covenant intensity for firms in exclusive relationships is concentrated in rated firms. Among rated firms, covenant intensity is between 6% and 7% lower for borrowers in exclusive relationships as compared to borrowers in medium intensity relationships.[11] Columns 2 and 6 of Table 7 show that the interaction between high relationship intensity and access to the commercial paper market has similar coefficients, but is at best marginally significant. Consequently, it appears that having any rating is more important than having a particularly favorable rating.

The above result is difficult to reconcile with both hold-up and information asymmetry interpretations. If the increase in covenant intensity in a relationship is related to ex ante hold-up, it should be concentrated in firms with low bargaining power. In addition, both ex post hold-up as well as information asymmetry considerations should be

[10] In Rajan and Winton (1995), the contracting parties choose between long-term debt with covenants and short-term debt without covenants. One way to gauge the applicability of this model to the empirical setting is to test the model's prediction that variation in covenant use should be associated with variation in maturity. In an unreported regression, I confirm that a larger number of covenants is associated with a longer maturity.

[11] Ai and Norton (2003) point out that the interpretation of interaction terms can be difficult in nonlinear models. This problem does not apply here since the Poisson model is linear in the log of the covenant count, which makes analyzing percentage changes straightforward.

Table 7
The effect of relationship intensity on financial covenant use depending on the distribution of bargaining power and syndicate structure.
This table reports Poisson regressions for the sample of loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995–2008 to assess the interaction of the relationship effect with the borrower's bargaining power and the structure of the loan syndicate. The dependent variable is the number of financial covenants included in the loan. Columns 1 through 4 use *Relation (Maximum)* as the relationship variable, while columns 5 through 8 use *Relation (Weighted)*. *CP access* indicates that the borrower has access to the commercial paper market (proxied by a short-term credit rating of A-2 or better). *Sole lender* is a dummy variable that equals one if the loan is made by only one lender and zero if the loan is made by a syndicate of lenders. *Multiple lead* equals one if there are at least two lead arrangers involved in the loan, and zero if there is only one lead arranger (regardless of the number of participants). Control variables are the same as in Table 4. All regressions control for industry fixed effects at the one-digit SIC level, year fixed effects at the respective loan's origination date, as well as loan purpose and loan type fixed effects. Numbers in parentheses are z-statistics adjusted for heteroskedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	Relationship variable: Relation (Maximum)				Relationship variable: Relation (Weighted)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Low relation	−0.0548***	−0.0709***	−0.0566***	−0.0830***	−0.0568***	−0.0641***	−0.0496***	−0.0816***
	(−2.78)	(−5.07)	(−3.89)	(−5.26)	(−3.18)	(−5.11)	(−3.82)	(−5.62)
High relation	0.0009	−0.0282**	−0.0266*	−0.0402**	0.0011	−0.0231*	−0.0235*	−0.0446***
	(0.04)	(−1.97)	(−1.84)	(−2.48)	(0.06)	(−1.83)	(−1.89)	(−2.98)
Low relation	−0.0289				−0.0036			
× Rated	(−1.05)				(−0.15)			
High relation	−0.0692**				−0.0581**			
× Rated	(−2.55)				(−2.42)			
Low relation		−0.0269				0.0159		
× CP access		(−0.54)				(0.35)		
High relation		−0.0851*				−0.0495		
× CP access		(−1.77)				(−1.16)		
Low relation			−0.0718*				−0.0873**	
× Sole lender			(−1.78)				(−2.29)	
High relation			−0.0370				−0.0511	
× Sole lender			(−0.84)				(−1.22)	
Low relation				0.0428				0.0569**
× Multiple lead				(1.39)				(2.20)
High relation				0.0262				0.0536**
× Multiple lead				(0.92)				(2.22)
CP access		−0.1366***				−0.1756***		
		(−2.86)				(−4.28)		
Sole lender			−0.0388				−0.0260	
			(−0.94)				(−0.67)	
Multiple lead				−0.0362				−0.0515**
				(−1.36)				(−2.41)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan type effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7924	7924	7924	7924	7924	7924	7924	7924

less relevant for firms with access to outside capital since public information production should limit such problems for these firms. Yet covenant use does not decline in exclusive relationships for firms without public debt market access. Thus, the results are difficult to reconcile with a rise in covenant costs in exclusive relationships as the reason for the observed effects.

If the decrease in covenant intensity is related to a decline in covenant benefits, it also seems somewhat surprising that this decline is concentrated in rated firms. Firms with public debt market access potentially have a more diverse set of nonbank creditors. Hence, such firms should have more to gain from providing monitoring incentives to the bank lender even if the relationship is exclusive. Note, however, that public bonds typically lack covenants requiring the maintenance of financial ratios. Thus, even with a lower number of covenants the exclusive bank lender still is the only creditor whose contract includes the type of covenants modeled in Rajan and Winton (1995) and Park (2000). While this argument could help explain why rated borrowers reduce their covenant load at all in an exclusive relationship, it is interesting that unrated borrowers do not do so. Bharath et al. (2011) find that yield spreads paid by unrated borrowers decline in relationship intensity. It thus appears that these firms prefer negotiating a lower interest rate when relationship intensity is high rather than lowering their covenant load.

I next turn to the impact of syndicate structure on the relationship effect. In a borrowing syndicate with multiple loan participants or lead arrangers, every lender is treated equally in the event of a covenant breach. This limits the extent to which monitoring benefits accrue to the lead arranger. Consequently, if monitoring incentives motivate the inclusion of financial covenants in contracts with relationship lenders, the increase in covenant use from low to medium relationship intensities should be stronger for sole lender loans or loans with only one lead arranger. Similarly, the decrease in covenant use from medium to high relationship intensities could be less strong for loans with multiple participants and multiple lead arrangers. If the benefit from giving monitoring incentives to lenders of such loans is lower, the monitoring incentive explanation suggests an overall flatter inverted U. Columns 3 and 7 of Table 7 show that the difference in covenant intensity between a loan from a lender with little prior relationship and a loan from a medium intensity relationship lender is stronger for sole lender loans. The relationship effect is nearly twice as large for sole lender loans as it is on average. According to columns 4 and 8, the inverted U curve is flatter for loans with multiple lead arrangers, although the difference is significant only for the relationship measure that gives $1/N$ credit to each of the N lead arrangers.

5.4. Information asymmetries

This section tests more extensively the possibility that the lower covenant intensity for exclusive relationships is related to a reduction in information asymmetries. These tests use interactions of relationship intensity with dummy variables that indicate whether the borrower's total assets are below the sample median during the start year of the loan, whether the borrower's stock is a member of the S&P 500 index, whether the borrower is a high tech firm (Loughran and Ritter, 2004), whether the number of analysts following the borrower's stock is below the sample median for that year, whether the dispersion of analyst forecasts for the borrower's earnings per share is above the median, and whether the borrower is listed on Nasdaq as opposed to NYSE or Amex.

Table 8 presents results using the *Relation (Maximum)* measure. Results using the *Relation (Weighted)* measure are qualitatively and quantitatively similar and are omitted for brevity. Table 8 shows that the downward sloping part of the inverted U is stronger for large borrowers, borrowers with a large analyst following, and borrowers whose stock is part of the S&P 500. The first two of these interactions are statistically significant at the 5% level, while the interaction with S&P 500 membership is marginally significant at the 10% level. These results are consistent with the finding that the decrease in covenant count in exclusive relationships is driven by borrowers with better access to capital markets. There is no difference for high tech vs. other firms, Nasdaq vs. NYSE/Amex firms, or firms with high vs. low dispersion of analyst forecasts. Overall, the evidence is difficult to reconcile with covenant intensity primarily being driven by a reduction in information asymmetries between the borrower and the relationship lender. The evidence also does not seem consistent with the explanation that relationship banks' incentives to renegotiate drive variation in covenant use across different loans. Such an explanation would suggest that covenant use should increase more strongly when preserving the relationship is more valuable, which is likely the case for opaque borrowers.

5.5. Endogeneity of relationship choice

The choice of forming a lending relationship and of cultivating multiple relationships as opposed to just one is likely to be endogenous. Perhaps firms that do not form relationships differ from firms that have relationships with several banks and firms that have an exclusive relationship in ways that explain the inverted U effect shown thus far. I employ several different ways to test whether results are driven by endogeneity. This section discusses results from an instrumental variables approach, propensity score matching, and from analyzing loan terms that are unlikely to be used as monitoring incentives.

5.5.1. Instrumental variables

As with the regressions for covenant tightness, the distance between the borrower and the nearest syndication active bank or its syndication active predecessor serves as an instrument for relationship status. Since the relationship intensity variables also measure the exclusivity of the relationship, I construct a second instrument to address the likelihood of forming multiple relationships as opposed to one relationship. This instrument is the number of syndication active banks within a one-hundred-mile radius around the borrower's headquarters at the time the loan is made. If a borrower has multiple potential lead lenders nearby, the likelihood of forming multiple relationships should increase. Finally, as a third instrument I use the distance between the borrower and the nearest highly syndication active bank. Banks with high capacity for syndicating loans potentially are better able to form persistent relationships with their borrowers. A bank is defined as highly syndication active in year t if it is among the top three syndication active banks in that year.

Since the endogenous regressors represent a nonlinear function of the same variable, the potential of instrument weakness is a concern. To my knowledge, no procedure is available to test for instrument weakness in the nonlinear generalized method of moments (GMM) setting, whereas tests and implications in the linear model are better understood (Stock et al., 2002). For this reason, I implement a two-stage least squares (2SLS) estimation of Eq. (5), although results are similar when using nonlinear GMM instead. Because *Low relation* and *High relation* are indicator variables, I follow the recommendation in Wooldridge (2002) to first estimate a probability model for the relationship dummies with the instruments described above and then use the predicted probabilities as the actual instruments in the first stage of the 2SLS estimation.[12] To account for the dependence between the indicator variables, the estimation uses an ordered probit model.

Results from the IV estimation are displayed in Table 9. The first stage in Model (1) shows that all three instruments negatively predict relationship intensity, as expected. Model (2) omits the potentially endogenous variables maturity and syndicate size from the ordered probit, similar to Bharath et al. (2011). The instruments continue to negatively predict relationship intensity. The coefficients on the relationship indicators are negative and statistically significant in both models. It is apparent that the 2SLS coefficients are very large compared to the Poisson regressions. On the one hand, this is consistent with other studies in the relationship literature that have found much

[12] Note that this method is not the same as plugging the predicted probabilities into the second stage, which would amount to a case of "forbidden regression" (Wooldridge, 2002).

Table 8
The effect of relationship intensity on financial covenant use depending on the borrower's information opacity.
This table reports Poisson regressions for the sample of loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995–2008 to assess the interaction of the relationship effect with the borrower's information opacity. The dependent variable is the number of financial covenants included in the loan. *Relation (Maximum)* is used as the relationship variable, but results using *Relation (Weighted)* are qualitatively and quantitatively similar. *Small borrower* is a dummy variable indicating that the borrower's size is below the median size of borrowers in the same year. *Hightech* is a dummy variable that equals one if the borrower is a member of a hightech industry as defined in Loughran and Ritter (2004). *Low analyst* indicates that the number of analysts covering the borrower according to the Institutional Brokers' Estimate System (I/B/E/S) detail files is below the median number of analysts for borrowers in that year. *High forecast disp.* equals one if the dispersion of analyst forecasts for a firm's earnings per share (EPS) is above the sample median in that year, and zero otherwise. Forecast dispersion for a firm is measured as the standard deviation of EPS forecasts divided by the absolute value of the mean EPS forecast in the I/B/E/S summary file. The dummy variable *Nasdaq* indicates that the firm's stock traded on Nasdaq at the point of contracting the loan. Control variables are the same as in Table 4. All regressions control for industry fixed effects at the one-digit SIC level, year fixed effects at the respective loan's origination date, as well as loan purpose and loan type fixed effects. Numbers in parentheses are z-statistics adjusted for heteroskedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Low relation	−0.0795***	−0.0690***	−0.0687***	−0.0566***	−0.0747***	−0.0553***
	(−4.22)	(−4.76)	(−4.74)	(−3.03)	(−3.60)	(−3.09)
High relation	−0.0647***	−0.0266*	−0.0306**	−0.0625***	−0.0645***	−0.0473***
	(−3.61)	(−1.78)	(−2.07)	(−3.48)	(−3.24)	(−2.67)
Low relation	0.0226					
× Small borrower	(0.81)					
High relation	0.0668**					
× Small borrower	(2.47)					
Low relation		−0.0268				
× S&P 500		(−0.66)				
High relation		−0.0553				
× S&P 500		(−1.50)				
Low relation			−0.0323			
× Hightech			(−0.84)			
High relation			−0.0166			
× Hightech			(−0.41)			
Low relation				−0.0133		
× Low analyst				(−0.48)		
High relation				0.0585**		
× Low analyst				(2.23)		
Low relation					0.0026	
× High forecast disp.					(0.09)	
High relation					0.0293	
× High forecast disp.					(1.06)	
Low relation						−0.0283
× Nasdaq						(−1.05)
High relation						0.0353
× Nasdaq						(1.28)
Small borrower	−0.0362					
	(−1.31)					
Hightech			0.0452			
			(1.24)			
Low analyst				−0.0518**		
				(−2.08)		
High forecast disp.					−0.0566**	
					(−2.22)	
Nasdaq						0.0048
						(0.18)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes
Industry effects	Yes	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose effects	Yes	Yes	Yes	Yes	Yes	Yes
Loan type effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7924	7924	7924	7688	5971	7599

larger coefficients in IV regressions compared to regressions not accounting for endogeneity (e.g., Bharath et al., 2011; Berger et al., 2005). On the other hand, it could be a sign of instrument weakness. Using the Cragg-Donald F-statistic and the Kleibergen-Paap rk F-statistic, the table shows that Model (1) rejects instrument weakness, while Model (2) fails to reject.

Instrument weakness-robust inference can be achieved using the Anderson-Rubin (AR) (1949) statistic. The online Appendix discusses estimation of this statistic as well as the construction of weakness-robust confidence sets to assess the significance of each individual endogenous regressor. As shown in Table 9, the AR-statistic strongly rejects the hypothesis that the relationship variables jointly equal

Table 9

Instrumental variables estimation for the effect of relationship intensity on financial covenant use.

This table shows results for the relationship effects on financial covenant intensity from two-stage least squares (2SLS) estimation for the sample of loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995–2008. The column denoted *1st Stage* shows the results of an ordered probit model, where the dependent variable equals one for low, two for medium, and three for high relationship intensity. Relationship intensity is calculated using the *Relation (Maximum)* measure defined in Table 3. Predicted probabilities for low and high relationship intensity from this model are used as instruments in the first stage of the 2SLS estimation. The column denoted *IV* shows the 2SLS result for financial covenant intensity. The instruments include the log of one plus the distance between the borrower's headquarters and the headquarters of the nearest syndication active bank, the number of syndication active banks within a radius of 100 miles around the borrower's headquarters, as well as the log of one plus the distance between the borrower and the headquarters of the nearest highly syndication active bank. A bank is defined as syndication active in a given year if it acted as lead arranger for at least 30 deals in DealScan during that year. A bank is defined as highly syndication active if it is among the top three lenders in the syndicated loan market during the year of the loan. Model (1) includes the potentially endogenous variables maturity and syndicate size from the ordered probit, model (2) excludes these variables. Control variables are described in Table 4. *Cut 1* and *Cut 2* refer to the cut points for the latent variable in the ordered probit model. All regressions control for industry fixed effects at the one-digit SIC level, year fixed effects at the respective loan's origination date, as well as loan purpose and loan type fixed effects. Numbers in parentheses are *t*-statistics (*z*-statistics in the case of the ordered probit) adjusted for heteroskedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. The Cragg-Donald *F*-statistic and the Kleibergen-Paap rk *F*-statistic are tests for instrument weakness, where the former assumes homoskedastic i.i.d. errors, while the latter is robust to clustered standard errors. The critical value from Stock and Yogo (2005) is the value that the Cragg-Donald *F*-statistic needs to exceed to reject the hypothesis that a test for the joint significance of the endogenous regressors with nominal size 5% has a true size of more than 10%. The Anderson-Rubin *F*-statistic tests whether the endogenous regressors are jointly zero and is robust to instrument weakness.

	Model 1		Model 2	
	1st Stage	IV	1st Stage	IV
Low relation		−1.8244***		−2.9722***
		(−3.85)		(−3.17)
High relation		−1.6294***		−2.5355***
		(−3.31)		(−3.39)
Ln(Closest bank distance)	−0.0212*		−0.0199*	
	(−1.90)		(−1.79)	
Num. potential lenders	−0.0240***		−0.0239***	
	(−2.75)		(−2.78)	
Ln(Top 3 lender distance)	−0.0561***		−0.0548***	
	(−3.83)		(−3.72)	
Ln(Loan amount)	0.1171***	−0.0439**	0.2241***	−0.0647**
	(4.14)	(−2.24)	(9.22)	(−1.98)
Ln(Maturity)	−0.1799***	0.0464*		0.0754
	(−4.57)	(1.68)		(1.59)
Ln(Lenders)	0.2446***	0.0355		0.0078
	(9.74)	(1.18)		(0.15)
Ln(Assets)	−0.0185	−0.0279**	0.0009	−0.0285
	(−0.71)	(−2.20)	(0.03)	(−1.63)
Leverage	0.1035	0.0157	0.1461	−0.0206
	(0.89)	(0.24)	(1.25)	(−0.22)
Tangibility	0.0387	−0.0770*	0.0040	−0.0836
	(0.42)	(−1.74)	(0.04)	(−1.36)
Current ratio	−0.0082	0.0006	−0.0140	−0.0053
	(−0.56)	(0.08)	(−0.96)	(−0.46)
Ln(1+Coverage ratio)	0.0685***	0.0486***	0.0746***	0.0520***
	(3.04)	(3.43)	(3.28)	(2.71)
Rating	−0.0254**	0.0179**	−0.0303***	0.0134
	(−2.41)	(2.50)	(−2.86)	(1.34)
Not rated	−0.2924**	0.3112***	−0.3644***	0.2842**
	(−2.10)	(3.75)	(−2.61)	(2.49)
MTB	0.0141	−0.0223**	0.0148	−0.0283*
	(0.68)	(−2.01)	(0.71)	(−1.70)
S&P 500	−0.1105	−0.2147***	−0.1477**	−0.2329***
	(−1.62)	(−5.61)	(−2.17)	(−4.38)
Constant		2.1483***		3.1133***
		(4.58)		(3.76)
Cut 1	−0.3585		0.5655	
	(−0.81)		(1.33)	
Cut 2	−0.0161		0.9034**	
	(−0.04)		(2.12)	

(*continued on next page*)

Table 9 (*continued*)

	Model 1		Model 2	
	1st Stage	IV	1st Stage	IV
Industry effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes
Loan purpose effects	Yes	Yes	Yes	Yes
Loan type effects	Yes	Yes	Yes	Yes
Observations	7407	7407	7407	7407
Weak ID tests				
Cragg–Donald *F*		9.93		5.72
Kleibergen-Paap rk *F*		10.82		6.32
Stock-Yogo (2005) crit.		7.03		7.03
Weak ID robust inference				
Anderson-Rubin *F*		15.06		24.82
Anderson-Rubin *p*-value		0.000		0.000

zero. The AR confidence sets in the online Appendix show that each relationship indicator is individually significant. In summary, although reliable point estimates for relationship intensity are difficult to obtain using IV, the relationship effects are statistically significant even under weak-instrument robust inference.

5.5.2. Further tests

A different way of accounting for selection effects is to apply propensity score matching. Unlike the IV approach, this method produces unbiased estimates only if all factors affecting selection into treatment groups are observable. I estimate treatment effects by matching each loan with medium relationship intensity to a set of low relationship intensity loans based on each loan's propensity to be treated with medium relationship intensity. Similarly, high relationship intensity loans are matched to medium intensity loans based on their propensity to be in the high intensity group. Results, which are untabulated for brevity, are quantitatively and qualitatively similar to those from the Poisson model. As an additional robustness check, I distinguish between low relationship intensity borrowers whose previous loan also had a low relationship intensity and borrowers who broke up an existing relationship when entering the current loan. There is no evidence that the lower covenant intensity for low relationship intensity loans is driven by borrowers breaking existing relationships to lower their covenant load.

As a further test, I estimate relationship effects for loan contract terms that are not likely to be used as monitoring incentives. Yield spreads paid by the borrower do not assign state-contingent control to the lender. Finding an inverted U effect of relationship intensity on yield spreads could point to unobserved differences in risk across loans in different relationship categories. In unreported regressions, I find no such effect. Yield spreads monotonically decline in relationship intensity and this decline is stronger for small, unrated borrowers. This finding reduces the likelihood that the decrease of financial covenant intensity in exclusive relationships for large, rated borrowers is due to unobserved risk.

While financial covenants define financial health criteria that the borrower might fail to meet for a plethora of reasons, nonfinancial covenants restrict specific actions that involve some form of moral hazard, such as capital expenditures, dividend payouts, and asset sales. Financial covenants are thus written on quantities that are more volatile and less directly controllable by managers (Kahan and Tuckman, 1993). Consequently, financial covenants require a larger effort on the part of the monitor to evaluate the implications of a violation. As a result, any monitoring incentive considerations should be more pronounced for financial covenants, whereas nonfinancial covenants should be more strongly related to information asymmetries. Consistent with this prediction, I find that the use of nonfinancial covenants declines in relationship intensity, and there is no evidence of an inverted U effect.

6. Robustness checks

This section discusses results from structural estimation to account for the fact that a loan's interest rate and covenants are chosen simultaneously. It also discusses various additional robustness checks.

6.1. Simultaneity of covenant choice and loan pricing

The analysis in Section 5 assumes that covenant intensity is determined independently from other loan contract terms. In reality, contract terms are chosen simultaneously. Notably, Bradley and Roberts (2015) and Matvos (2013) find that loan covenants are priced and that borrowers are more likely to include a covenant when doing so allows them to obtain a larger reduction in the interest rate. This section investigates whether the effect of relationship intensity on covenant use holds after accounting for the price-covenant trade-off. The analysis relies on the structural estimation methodology used in Bradley and Roberts (2015), Goyal (2005), and Reisel (2014). Covenant choice is represented by the probit model:

$$I_i = \lambda\left(y_{lowcov,i} - y_{highcov,i}\right) + Z_i'\xi - \nu_i. \tag{6}$$

Following Bradley and Roberts (2015), I_i equals one if the loan includes more than two financial covenants (the sample median) and zero otherwise. The reduction in the log of the all-in drawn yield spread by including more than two covenants is given by $\left(y_{lowcov,i} - y_{highcov,i}\right)$ and the coefficient λ captures the extent to which borrowers are more likely to include the covenant when the resulting yield spread reduction is large. The vector Z_i contains other determinants of covenant choice.

There are two loan pricing equations, the first for loans with two or fewer covenants (low covenant load) and the second for loans with more than two covenants (high covenant load), both as a function of loan pricing determinants X_i:

$$y_{lowcov,i} = X_i'\pi_1 + u_{1i} \tag{7}$$

$$y_{highcov,i} = X_i'\pi_2 + u_{2i}. \tag{8}$$

As shown in Lee (1978), this system can be estimated in three steps. First, Eqs. (7) and (8) are substituted into Eq. (6) and the resulting reduced-form probit is estimated. The estimated linear predictor $\hat{\psi}$ is used to compute the Inverse Mills' ratio, defined as $\phi(\hat{\psi})/(1-\Phi(\hat{\psi}))$ for loans with low covenant load and $-\phi(\hat{\psi})/(\Phi(\hat{\psi}))$ for loans with high covenant load. Here, ϕ denotes the standard normal density function and Φ denotes the standard normal cumulative distribution function.

The second step is to estimate the loan pricing equations, correcting for selection bias by inserting the appropriate Inverse Mills' ratio into each equation. Using the estimated coefficients and excluding the effect of the Inverse Mills' ratio, one can now calculate the predicted yield spreads for the entire sample with a high covenant load and a low covenant load. In the third step, the structural probit equation is estimated plugging in the difference in the estimated yield spread for each loan $(\widehat{y_{lowcov,i}} - \widehat{y_{highcov,i}})$ to obtain consistent estimates of parameters λ and ξ.

A few comments regarding estimation are required. First, the structural probit suffers perfect multicollinearity if all variables X are contained in Z. It is acceptable, but not ideal, for all variables Z to be contained in X since the Inverse Mills' ratio is a nonlinear combination of these variables. To deal with these issues, I augment the set of variables in the loan pricing equations with the prevailing spread, defined as the average all-in drawn spread paid by all firms borrowing a loan during the previous 60 days. As discussed in Bharath et al. (2011), the marketwide level of yield spreads immediately prior to the time the loan is made should be related to the loan's spread. At the same time, it seems reasonable to argue that average past spreads should not affect covenant choice for this particular loan directly, other than through their effect on loan pricing. Similarly, the probit model includes the percentage of all loans during the previous 60 days that contain more than two covenants.

A small number of the predicted differences in yield spreads are very large (up to 950 basis points). To reduce the impact of such outliers, the predicted differences are winsorized at the 1% and 99% levels. These winsorizations enhance the precision of the coefficient estimate for the yield spread difference, but do not affect results for the relationship variables. Finally, since the difference in predicted spreads is subject to estimation error, the usual standard errors from the probit estimation are likely to be understated. To remedy this problem, I calculate bootstrapped standard errors using 500 replications.

Column 1 in Table 10 shows marginal effects for a simple probit regression of an indicator for more than two financial covenants on relationship intensity. Medium relationship intensity loans are 9.5% more likely to have more than two financial covenants than low intensity loans. High intensity loans are 5.7% less likely to have more than two financial covenants than medium intensity loans. Column 2 adds the log of the all-in drawn spread to the regression. The positive coefficient suggests that loans with a high covenant load are riskier on average. Columns 3 through 5 report results from the structural probit estimation using different sets of independent variables. Note that for each of these estimations all three steps need to be estimated. Intermediate results are omitted for brevity, although it shall be noted that the Inverse Mills' ratio is consistently significant in estimations of Eq. (8), but not Eq. (7). Columns 3 through 5 show that the predicted difference in yield spread is a positive predictor of high covenant load in all three specifications and statistically significant at the 10% level. For this larger sample, the numerical estimate is somewhat smaller than in Bradley and Roberts (2015), but statistical significance is very similar. However, adding this variable has little effect on the relationship terms. Column 5 drops the variables maturity and syndicate size, which are likely also simultaneously determined. Dropping these terms results in slightly larger coefficients for the relationship terms.

6.2. Other robustness checks

I perform various additional robustness tests. As stated in Section 3, the final sample of loans for which all necessary information is available is only half as large as the sample used to calculate relationship intensity. The online Appendix discusses reasons for why this is the case. Two reasons that one may be concerned about are that covenant information is missing in DealScan for 29% of the loans that were excluded, and for 13% of the excluded loans relationship intensity is unknown as there was no loan in the previous five years. The online Appendix shows results from Heckman sample selection models that assess whether there is something special about these loans. The results do not indicate the presence of a selection effect for loans that do not have covenant information available. Loans that are a borrower's first loan in five years do have fewer covenants, but adjusting for this effect does not alter the results with respect to relationship status.

Specific lenders could have certain preferences about covenant choice that could be spuriously correlated with relationship choice. To rule out this possibility, in unreported regressions I include lead arranger fixed effects to the set of control variables. Conclusions remain unchanged. Next, one may be worried that the results are driven by the credit boom that occurred before the onset of the recent financial crisis. To test this, I split the sample into two parts, separating the observations during the credit boom from those outside of it. I define the period from the year 2005 through mid-2007 as the credit boom period. There is no evidence that the credit boom drives the results.

Many loans have a relationship intensity of either zero or one. In fact, for borrowers that have only two loans in

Table 10
Adjusting for the price effect of covenants.
This table shows results for the relationship effects on financial covenant intensity after adjusting for the price effect of covenants using the Bradley and Roberts (2015) methodology. The sample includes loans incurred by nonfinancial, non-public administration, non-utility US borrowers from 1995–2008. Column 1 replicates the results from Table 6 for a simple probit whose left-hand-side variable is an indicator of whether or not a loan has more than two covenants. Column 2 adds the loan's yield spread to the regression. Columns 3 through 5 report results from estimating structural probit Eq. (6). In the first step, a reduced-form probit is estimated and its linear predictor is used to calculate the Inverse Mills' ratio. In the second step, structural loan pricing Eq. (7) and (8) are estimated by inserting the appropriate Inverse Mills' ratio to correct for the nonzero expectation of the error term. In the third step, the predicted difference in yields with a low vs. a high covenant load is calculated and the structural probit is estimated. The table reports the results from the third step. Intermediate steps are omitted for brevity. Relationship intensity is calculated using the *Relation (Maximum)* measure defined in Table 3. *Market % >2 covenants* denotes the fraction of loans issued during the 60 days before the current loan that have more than two covenants. This variable is excluded from the loan pricing equations. The loan pricing equations contain the average all-in drawn spread of loans issued over the previous 60 days as a variable that is excluded from the structural probit. Other variables are described in Table 4. All regressions control for industry fixed effects at the one-digit SIC level, year fixed effects at the respective loan's origination date, as well as loan purpose and loan type fixed effects. Numbers in parentheses are z-statistics adjusted for heteroskedasticity and firm-level clustering. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. All coefficients are stated in terms of marginal effects.

	(1)	(2)	(3)	(4)	(5)
Low relation	−0.0954***	−0.0940***		−0.0956***	−0.1126***
	(−4.31)	(−4.25)		(−4.03)	(−4.50)
High relation	−0.0573**	−0.0543**		−0.0624**	−0.0689***
	(−2.51)	(−2.38)		(−2.57)	(−2.73)
Ln(AllInDrawn)		0.0741***			
		(4.39)			
$(y_{lowcov,i} - y_{highcov,i})$			0.4264*	0.4346*	0.4606*
			(1.89)	(1.92)	(1.88)
Market % >2 covenants			0.0836	0.0824	0.0843
			(0.48)	(0.47)	(0.49)
Ln(Loan amount)	−0.0100	−0.0062	−0.0105	−0.0130	0.0150
	(−0.76)	(−0.47)	(−0.80)	(−0.99)	(1.33)
Ln(Maturity)	0.0214	0.0310*	0.0066	0.0114	
	(1.20)	(1.72)	(0.34)	(0.60)	
Ln(Lenders)	0.0434***	0.0450***	0.0544***	0.0514***	
	(3.79)	(3.93)	(4.73)	(4.40)	
Ln(Assets)	−0.0319***	−0.0235**	−0.0388***	−0.0399***	−0.0394***
	(−2.70)	(−1.97)	(−3.15)	(−3.21)	(−3.16)
Leverage	0.0056	0.0006	0.0706	0.0708	0.0869
	(0.11)	(0.01)	(1.19)	(1.18)	(1.40)
Tangibility	−0.0614	−0.0617	−0.0783*	−0.0815**	−0.0782*
	(−1.51)	(−1.51)	(−1.92)	(−1.99)	(−1.93)
Current ratio	0.0214***	0.0208***	0.0094	0.0091	0.0082
	(3.11)	(3.01)	(0.97)	(0.97)	(0.84)
Ln(1+Coverage ratio)	0.0178*	0.0274***	0.0315***	0.0320***	0.0382***
	(1.81)	(2.71)	(2.58)	(2.61)	(2.81)
Rating	0.0399***	0.0315***	0.0237**	0.0231**	0.0245***
	(7.61)	(5.70)	(2.32)	(2.24)	(2.59)
Not rated	0.5690***	0.4704***	0.3327**	0.3267**	0.3425**
	(8.34)	(6.57)	(2.31)	(2.25)	(2.57)
MTB	−0.0224**	−0.0177*	−0.0116	−0.0122	−0.0135
	(−2.31)	(−1.82)	(−1.07)	(−1.12)	(−1.25)
S&P 500	−0.1868***	−0.1855***	−0.0642	−0.0634	−0.0780
	(−5.20)	(−5.17)	(−0.87)	(−0.85)	(−1.07)
Industry effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
Loan purpose effects	Yes	Yes	Yes	Yes	Yes
Loan type effects	Yes	Yes	Yes	Yes	Yes
Observations	7650	7650	7650	7650	7650

DealScan, relationship intensity has to equal either zero or one by construction. To check whether this affects results, I re-estimate the regressions in Table 6 for only those loans whose relationship intensity is larger than zero and smaller than one. Alternatively, I restrict the sample to loans from borrowers that have at least four loans in DealScan. Either way, results are quantitatively and qualitatively similar.

In Park (2000), the monitoring lender's claim needs to be large enough for the lender to be impaired in bankruptcy, but small enough to preserve the incentive to liquidate in bad states of nature. Since relationship intensity is a function of past loan amounts, one may worry that the nonlinear results presented in this paper capture this prediction rather than the effect of relationship strength. In unreported regressions, I re-estimate results using only the number of past loans to calculate relationship strength. Coefficients are smaller, but conclusions remain the same. Since past loan amounts could proxy for either a stronger relationship or more exposure, I calculate the lender's exposure to the borrower as the amount of outstanding loans from this lender divided by the borrower's assets. I allow this variable to enter the regression as a linear and a squared term or as piecewise indicator variables for different levels of exposure. Results are unchanged.

A further worry could be that results are sensitive to the definition of covenant intensity. The results presented thus far use a simple count of the number of covenants attached to the loan. An alternative way of counting covenants is to consider the six groups presented in Table 1 and add one for each covenant group included in the loan: debt to balance sheet, coverage, debt to cash flow, liquidity, net worth, and EBITDA covenants. This avoids potential double counting of similar covenants. Results are robust to this change.

One may also wonder whether the inverted U effect is driven by a certain kind of financial covenant. In particular, the accounting literature distinguishes between performance or cash-flow-based covenants, such as covenants on coverage ratios or debt/EBITDA, and capital or balance sheet covenants, such as covenants on liquidity ratios or measures of net worth (Christensen and Nikolaev, 2012; Demerjian, 2011). Following these authors' definitions and examining performance and capital covenants independently, I find that the inverted U effect is qualitatively similar for both types of financial covenants.

Apart from the joint determination of covenants and price, non-price contract terms such as covenant intensity, tightness, loan maturity, amount, collateral, and syndicate size are likely to be determined jointly as well. Estimating a system of equations for all these terms does not seem practical due to the lack of credible instruments. To gauge the presence of any correlation effects, I estimate the relationship effects including all these variables as controls as well as excluding them. Results for covenant intensity and tightness are qualitatively and quantitatively highly similar regardless of whether these terms are included or excluded.

Finally, as Table 2 shows, the number of loans per firm in the final sample varies from one to 14. One may be concerned that firm-level dependence or an overweighting of certain firms relative to others drive the results. To address this concern, standard errors allow for clustering at the firm level throughout the paper. In additional robustness checks, I also allow standard errors to be clustered at the firm and time level (Petersen, 2009). Moreover, I randomly select one observation per firm to estimate the relationship coefficients and construct a confidence interval for the coefficients by repeating this analysis 1,000 times. The inverted U shape effect holds and is statistically significant across these methods.

7. Conclusion

In this paper, I study how loan covenants are structured in a lending relationship to optimize the lender's information production. Consistent with the information asymmetry theory of covenant use, covenant tightness monotonically decreases over the duration of a lending relationship, especially if borrowers are opaque. In contrast to this result, the number of covenants included in a loan appears to be related to relationship intensity rather than relationship duration. Specifically, the results suggest that relationship intensity has an inverted U effect on the number of covenants included in a loan, such that the firm's main relationship lender receives more covenants than other lenders, unless the relationship is exclusive. I discuss covenants' role as monitoring incentives as a potential explanation for the increase in covenant intensity in lending relationships. Consistent with covenants serving this purpose, the increase in covenant intensity in a lending relationship is stronger for sole lender loans and loans with only one lead arranger, for which covenants can be expected to be more effective in creating monitoring incentives. There is no evidence that the effect of relationship status on covenant count is linked to ex ante lock-in effects or information asymmetries.

The reduction in covenant intensity in exclusive relationships gives rise to interesting questions for future research. In this paper, I conjecture that the benefits from using covenants as a monitoring incentive could be lower when there is only one bank lender that can credibly provide monitoring. This conjecture relies on the assumption that multiple senior bank lenders are willing to free-ride on each other such that borrowing exclusively from one bank lender mitigates the free-rider problem that covenants are designed to address. Future research should examine the extent to which free-riding occurs among senior bank lenders and under what circumstances covenants, as opposed to exclusive lending, are part of an optimal solution to such free-riding. In this context, it is intriguing that unrated borrowers do not reduce their covenant load in exclusive relationships. Examining the reasons for this choice could lead to interesting insights into the contracting choices made by borrowers of different information opacity.

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