Tough Love: The Causal Effects of Debt Covenants on Firm Performance*

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Abstract

I investigate whether restrictive loan covenants disrupt or improve firm operating performance. Using an instrumental variables approach to address the endogenous relationship between covenant strictness and firms' efficiency, I find stricter loan covenants cause an increase in profitability and a reduction in operating cost. Stricter covenants improve performance only in firms with poor governance: those without large shareholder ownership, with weaker shareholder rights, facing softer competition in their product market, or inside directors dominate their board. The evidence is consistent with the view that the design of debt contracts can mitigate agency costs in firms that lack alternative governance mechanisms.

Do loans with restrictive covenants disrupt or improve borrowers' operating performance? The effect of loan covenants on firm efficiency is an important economic question because bank loans are one of the most important sources of external finance for firms. Estimating the causal effect of restrictive debt covenants on firm performance is a challenging task, because the design of a loan contract is endogenous to firm characteristics (Jensen and Meckling (1976)). Using an instrumental variables (IV) approach to address this identification issue, the main contribution of this paper is to show restrictive loan covenants have a causal impact on borrowers' profitability and cost efficiency.

Theory suggests restrictive debt covenants may have both a positive and a negative effect on firms' operating performance. On the one hand, covenants can have a negative effect on profitability by constraining managers' flexibility to make optimal decisions, and by altering the choice of projects and financial policies (Jensen and Meckling (1976), Tirole (2010)). On the other hand, restrictive loan covenants can also have a positive effect on firm performance by disciplining the manager (Myers (1977), Dewatripont and Tirole (1994)). The optimal set of covenants therefore should balance the conflicts of interest between creditors and shareholders in a way that maximizes the value of the firm (Smith and Warner (1979)). Yet, due to the separation of ownership and control, self-serving managers have an incentive to set covenants looser than what is optimal for the firm in order to protect their jobs, maintain their flexibility, and maximize their personal utility (see Donaldson (1963), Myers (1977)). Therefore, when intense agency conflicts occur between managers and shareholders, managers may select loan covenants that are not optimal for the firm. As a result, variation in covenant strictness could lead to significant efficiency gains for firms.

The main challenge in estimating the causal effect of stricter debt covenants on firm per-

formance is finding variation in covenant strictness that is exogenous to firm characteristics. Poorly performing firms are more likely to get stricter debt covenants (see Aghion and Bolton (1992), Nini et al. (2009), and Roberts and Sufi (2009a)), or firms may choose tighter covenants to signal positive private information (see Demiroglu and James (2010) and Beyhaghi et al. (2016)). I address this endogeneity issue using an IV approach.

A valid instrument for covenant strictness needs to affect the propensity of banks to impose stricter covenants, but it must be unrelated to borrower characteristics. A variable that satisfies both conditions is the number of defaults in the portfolio of the lead lender in the six-month period prior to the origination of the loan. Consistent with the findings of Murfin (2012), I find that when banks experience defaults in their loan portfolios they impose more restrictive debt covenants on new loans. Importantly, the change in covenant strictness is driven by shocks that affect only the lead arranger (the supply side) and is plausibly exogenous to unobserved borrower characteristics. To make the exclusion restriction more plausible, I mitigate the possibility that unobservable industry- or location-specific factors affect both borrowers' future performance and defaults in the lead arranger's portfolio (see Hertzel and Officer (2012)). To this end, I exclude from the default-count measure the defaulting firms that come from the same industry or state as the firm that receives the new loan.[1] The identifying assumption is that the defaults of firms located in different states and different industries affect the performance of a firm only through the strictness of the debt contract with the lead arranger.

[1]Consider, for example, the loan Bank of America made to Gap, Inc. on June 25, 2003. To construct the IV, I count how many active loans in the portfolio of Bank of America defaulted in the 180-day period leading up to Gap's loan, that is, 12/25/2014-06/25/2015. In this default-count variable, I do not include defaults of firms from the same state or industry as Gap. For example, two of the companies that defaulted six months prior to the loan Bank of America made to Gap are United Airlines (headquartered in Illinois) and Weirton Steel (headquartered in West Virginia). Therefore, the IV will not satisfy the exclusion requirement if we believe the defaults of a steel company from West Virginia (Weirton Steel) and an airline company from Illinois (United Airlines) correlate with unobserved factors that determine the operating performance of an apparel company in California (Gap).

I find that stricter debt covenants have a positive and economically large effect on profitability (ROA). In particular, a one standard deviation exogenous increase in covenant strictness causes a 2.6% increase in profitability one year after the start of the loan. The effect is economically larger in the subsample of borrowers that have a past credit relationship with the lead arranger, because these firms are less likely to engage in loan shopping due to high information costs (see Sharpe (1990) and Rajan (1992)). I also find that stricter covenants lead to a reduction in firms' operating costs, which suggests firms operate at a lower unit cost and increase their profit margins. The results are robust to controlling for loan-contract terms (including the loan spread), using a variety of fixed effects, and focusing on a sample of matched firms with similar characteristics and past performance.

I investigate whether the positive effect of covenant strictness on firm performance is due to managerial myopia. For instance, in response to stricter covenants, managers might try to boost short-run profitability at the expense of long-run performance, they might try to manipulate earnings, or choose riskier projects. The evidence is not consistent with this conjecture. Stricter covenants do not lead to a reversal in long-run profitability, and do not increase earnings management or operational risk.

The positive effects of debt covenants on profitability raise the following question: if stricter covenants improve firm performance, why do firms not always adopt stricter terms? Agency conflicts. Due to the separation of ownership and control, managers could choose loan terms that maximize their personal utility instead of shareholders'. For instance, managers prefer looser covenants to reduce the possibility of a covenant violation that would allow creditors to take control of the management (see Roberts and Sufi (2009b), Nini et al. (2012)). Therefore, managers, who make decisions on behalf of shareholders, might exploit their position

to the detriment of firms' operating efficiency, especially when the firm does not have strong governance mechanisms in place to mitigate these agency conflicts.

I hypothesize that if financial covenants mitigate managerial agency costs and have a disciplinary effect that forces managers to improve operating efficiency (Harris and Raviv (1990)), stricter covenants should have a positive effect only on firms that lack alternative governance mechanisms to monitor and discipline the manager. To test this hypothesis, I investigate whether stricter covenants have a larger effect on profitability for firms with larger manager-shareholder conflicts. Specifically, agency conflicts are greater and managers are less likely to maximize operating efficiency if firms face softer competition in their product market, if they lack large monitoring shareholders (blockholders), if inside directors dominate the board, or if managers are entrenched and protected from takeovers.[2] I find that an exogenous increase in covenant strictness increases operating efficiency only in firms with higher agency conflicts between managers and shareholders. Therefore, the heterogeneous impact of debt covenants on performance suggests that the design of debt contract terms can substitute for alternative external or internal governance mechanisms.

This paper contributes to the literature on the design of optimal debt contracts, and offers evidence consistent with theory suggesting financial covenants have a positive effect on firms by controlling agency problems (Myers (1977), Smith and Warner (1979), Berlin and Mester (1992)). Several empirical studies on debt contracting investigate the endogenous choice of debt covenants. For example, firms select tighter loan covenants in return for lower spreads (Matvos (2013), Bradley and Roberts (2015)), or because they want to signal private information about

[2]Without being exhaustive, the following work supports the view that poor governance leads to poor firm performance: Shleifer and Vishny (1986, 1997), Bertrand and Mullainathan (2003), Gompers et al. (2003), Cremers and Nair (2005), Bebchuk and Cohen (2005), Chen et al. (2007), Edmans (2009), Giroud and Mueller (2010, 2011), and Cohen and Wang (2013).

their future performance (Demiroglu and James (2010)). Banks are also more likely to impose covenants that restrict investment if firms have performed poorly before the origination of the loan (Nini et al. (2009)). However, due to the lack of exogenous variation in the strictness of debt contracts, this literature does not offer conclusive evidence on whether the design of financial covenants at the time of origination has a causal impact on performance.[3]

This paper is also related to empirical studies that examine the effect of covenants violations and creditor intervention on firm outcomes, such as financial policy (Chava and Roberts (2008), Roberts et al. (2009)), performance (Nini et al. (2012)), unemployment (Falato and Liang (2016)), and establishment closures (Ersahin et al. (2016)). This study is different because it suggests that even without creditor intervention, the design of debt contracts affects firm performance through incentives. I find evidence consistent with this hypothesis. Stricter covenants cause a larger increase in profitability if loans do not contain incentive schemes such as performance pricing terms, which link the cost of debt to firms' performance, providing managers with "carrot and stick "incentives to exert higher effort (Manso et al. (2010)).

The paper proceeds as follows. Section I describes the data and details of the empirical methodology. Section II presents the baseline results, and section III discusses the governance role of debt. In section IV, I test the robustness of the results, and section V concludes.

[3]For instance, Nini et al. (2009) argue "However, we view these results as suggestive, and we believe that more conclusive evidence is needed. One obvious angle is to isolate exogenous variation in the imposition of the restrictions shown here, and we hope that future research is able to utilize our data to answer the efficiency question more definitively." Similarly, Demiroglu and James (2010) argue, "Since we have no way of identifying exogenous variations in covenant choice, we cannot infer any causal connection between covenant choice and future performance. Therefore, our empirical findings are mainly descriptive."

I. Data and Empirical Strategy

A. Data and Summary Statistics

My empirical analysis focuses on primary issues of syndicated loans. Loan Pricing Corporation's (LPC) Dealscan database contains borrower-lender information of bank loan agreements, including financial covenants and performance-related restrictions. According to LPC's product manager, Dealscan covers 98% or more of global syndicated loans but varies in years.[4] I merge loan, lender, financial covenant, and pricing data from Dealscan with quarterly accounting information from Compustat using the link by Chava and Roberts (2008).

The merged Dealscan-Compustat sample consists of relatively large firms, because large firms have access to the market for syndicated loans. I exclude financial firms because their profitability and investment profiles significantly differ from non-financial firms. I do not exclude utility firms from the analysis, but the results do not change when I drop these observations. I also exclude loans whose primary purpose is to finance leveraged buyouts, because after the buyout, these firms usually go through severe restructuring and are likely to experience cash flow increases even without changes in efficiency (Hillier et al. (2011)).

My final dataset consists of 3,701 loan packages. A loan package can contain multiple loans, or facilities, of different types (e.g., term loans, revolvers, bridge loans), and each facility may have different characteristics, such as different maturity, amount, and spread. However, covenants pertain to the whole loan package and are the same for all loan facilities in this package. To avoid duplicating covenant and accounting information, therefore, the observations in my sample are at the package level.

[4]This statement comes from the online presentation of Dealscan in WRDS from LPC's product manager: http://wrds-web.wharton.upenn.edu/wrds/E-Learning/_000Video/Overview_of_Dealscan/index.cfm.

For every loan in the data, I identify the name of the borrower, the lender, the primary type and purpose of the loan, as well as its maturity and amount. I follow Chava and Roberts (2008) and define loan maturity as the number of months between the earliest loan origination date and the latest maturity date in the loan package. To define loan amount, I use the sum of all the amounts in a loan package.

The Appendix provides definitions for all variables, and Table I provides summary statistics for my sample. I winsorize the data at the 1st and 99th percentile to mitigate the effects of outliers. An average firm has approximately $3.3 billion in assets, and annual profitability (EBITDA/Assets) of 15%. The maturity and amount of an average loan package is four years and $475 million, respectively. Approximately 47% of the loans are secured (backed by collateral), and 9.7 banks participate in the syndication. The number of participating banks is an important control for contract strictness, because a large number of participants can potentially reduce the incentive of lead arrangers to screen and monitor the syndicated loan (see Gopalan et al. (2011) and Sufi (2007)). The summary statistics seem consistent with the averages reported in previous studies using Dealscan as a primary source of loan information (see Ivashina (2009)), Roberts and Sufi (2009b), Murfin (2012)).

[Insert Table I here]

The main dependent variable and measure of profitability that I use in this paper is ROA measured as EBITDA/Assets, calculated on a rolling four-quarter basis. I use EBITDA instead of net income because EBITDA captures operating cash flows more directly than net income, and excludes the effects of capital structure or taxation. I decompose EBITDA/Assets into two parts, EBITDA/Sales and Sales/Assets, and study how covenant strictness affects these measures of efficiency. This identity, known as DuPont identity, is informative because it

explains whether operating efficiency (EBITDA/Sales) or asset use efficiency (Sales/Assets) drives the change in profitability. I also use two measures of cost efficiency: cost of goods sold divided by sales, and administrative expenses divided by sales.

I have excluded from the regressions all firms that violate a financial covenant at loan origination and within a year from origination.[5] Covenant violations are technical defaults that allow creditors to accelerate loan payments and pave the way for creditors to directly intervene with firm policies. I exclude these violations to mitigate the possibility that firm outcomes are directly driven by creditor intervention.[6] I also exclude from the analysis facilities after renegotiations or covenant violations that appear as new loans and whose origination date starts before the expiration of the previous loan.

With accounting and loan data in place, I construct the two main variables of interest: contract strictness and the number of defaults the lead arrangers have suffered six months prior to making the loan. I construct these variables along the lines of Murfin (2012), and give a brief description of the process and summary statistics in the next two subsections.

A.1. Loan Covenant Strictness

To measure the strictness of loan covenants, I use a new measure from Murfin (2012), based on the probability of a covenant violation. Unlike a covenant-intensity index that simply counts the number of covenants in a debt contract, this measure of covenant strictness combines the total number of covenants, how tightly they are set, and also adjusts for the variance-covariance of the accounting ratios on which these covenants are based. As a result, this measure of covenant strictness provides a superior measure compared to other measures researchers have

[5] Amir Sufi generously provides the data on covenant violations on his website.
[6] I am particularly thankful to Cem Demiroglu for this suggestion.

used in the literature.[7] For example, Hertzel and Officer (2012), Kjenstad et al. (2013), and Bradley and Roberts (2015) use a covenant-intensity index, which simply counts the number of covenants in the loan contract. Counting the number of covenants (covenant intensity) does not necessarily imply covenants are stricter, especially if these restrictions are boilerplate covenants the firm will most likely not violate. For example, consider two firms with a financial covenant that requires both firms to keep their Debt/EBITDA ratio below 3. If, at the time of contracting, firm A's EBITDA/Debt ratio is 1, but is 2.8 for firm B, then simply counting the number of covenants cannot capture the fact that firm B is more likely to violate the covenant. Demiroglu and James (2010) create a measure of covenant tightness that captures this effect, but this measure does not account for the total number of covenants in the debt contract.

The measure of covenant strictness that I use in this paper proxies for the probability that the firm will violate at least one covenant over the next quarter. The Appendix describes how to estimate covenant strictness following Murfin's method. In Table I, the median of loan covenant strictness, or the probability of violating at least one covenant, is 18.3%. Creditors intentionally impose tight covenants at the time of origination, and loosen them later through renegotiations as information asymmetry between lenders and borrowers declines (see Garleanu and Zwiebel (2009)). Roberts et al. (2009) show that over 90% of long-term debt covenants are renegotiated prior to their stated maturity, not necessarily due to financial distress, but because the terms of the initial covenant are particularly strict. This evidence is similar to Denis and Wang (2014), who find evidence that creditors exert strong control rights over their borrowers'

[7] Demerjian and Owens (2014) hand-collect 5,278 covenants from a sample of 2,100 loans of the original Tearsheets in Dealscan and find that the definitions of the financial ratios on which covenants are defined are not homogeneous. They find, for example, 356 different definitions for Fixed Charge Coverage covenants in Dealscan. In this case, constructing this ratio using Compustat data could potentially contain measurement error. To address this issue, I construct the accounting ratios based on the definitions of the most popular and homogenous covenants according to Demerjian and Owens. However, the authors conclude measurement error is not likely to significantly affect the results, and argue in favor of a comprehensive measure of contract strictness such as the one Murfin (2012) uses.

operating and financial policies in a state-contingent manner through covenant renegotiations.

A.1. Instrumental Variable: Lenders' Defaults

The instrument for covenant strictness that I use in this paper is the number of defaults lead arrangers have suffered to their loan portfolios in the six-month period before issuing a new loan. I count the number of defaults from firms whose S&P long-term debt rating switched to default or selective default during the period that the firm had an outstanding loan with the bank. The purpose of the default count measure is to extract variation in covenant strictness from factors that are idiosyndcratic to the creditor and affect the supply side of strictness, and at the same time are unrelated to firm characteristics and do not affect the demand side of covenant strictness.

Dealscan provides information on all lenders participating in a syndicated loan. I only count the number of defaulting firms in the portfolio of lead arrangers and not of participating banks. Gopalan et al. (2011) show that failing to screen or monitor borrowers that perform poorly has a detrimental effect on the reputation of lead arrangers, and makes attracting participants in future syndications harder. Lead lenders are therefore more likely to change their monitoring in response to a larger number of defaults in order to improve their reputation as loan underwriters.

Lead arrangers have an average of 1.3 (median is zero) defaults in their loan portfolio six months before making a new loan. In constructing the default-count measure, I exclude defaults of firms that have the same one-digit SIC code or are headquartered in the same state as the new borrower. This procedure mitigates the concern that industry- or location-specific risk drives the change in strictness, and, therefore, makes it less likely that industry contagion

affects covenants strictness (see Hertzel and Officer (2012)). On the other hand, if changes in covenant strictness come from lender-idiosyncratic shocks, such as the number of defaults in a lender's portfolio, then one can more easily argue that contract changes come from the supply side and are therefore exogenous to firm performance.

B. Empirical Strategy

The most straightforward method to estimate the impact of covenant strictness on firm efficiency is via an OLS regression. In subsection B.1, I explain why estimates of OLS regressions could be biased downward. In subsection B.2, I describe the IV/2SLS methodology that I use in this paper to get unbiased estimates for the impact of covenant strictness on firm efficiency.

B.2. Simultaneity

The purpose of this section is to illustrate why the OLS estimates of covenant strictness on firm efficiency are biased downward in the presence of simultaneity. To understand the effect of simultaneity in OLS estimates, consider the following simple system of equations, where y is firm efficiency and x is covenant strictness:

$$y_{t+1} = \beta x_t + u_{t+1} \tag{1}$$

$$x_t = \alpha y_{t+1} + v_t. \tag{2}$$

The coefficient β represents the effect of covenant strictness on firm efficiency. To get an unbiased and consistent estimate of $\hat{\beta} = \beta + Cov(x_t, u_{t+1})/Var(x_t)$, we need the exogeneity condition $Cov(x_t, u_{t+1}) = 0$. Roberts and Whited (2012), in a simple system of equations as in (1) and (2), illustrate how in the absence of the exogeneity condition, the bias is given from

the formula shown below and depends on the signs and relative magnitudes of the coefficients α and β:

$$\frac{Cov(x,u)}{Var(x)} = \frac{\alpha(1-\alpha\beta)Var(u)}{\alpha^2 Var(u) + Var(v)} \tag{3}$$

Assume stricter covenants have a positive impact on firm efficiency, so the true β is positive. Also, the existing literature suggests more profitable firms tend to get looser covenants (see, Nini et al. (2009), Rauh and Sufi (2010)), and therefore α is negative. Then from equation (3), it becomes evident that the bias term is negative and the estimated coefficient $\hat{\beta}$ of covenant strictness is negatively biased.

B.2. IV/2SLS Regressions

To obtain the IV/2SLS estimates, I start by estimating the following first-stage regression similar to Murfin (2012):

$$Strictness_{i,t} = \alpha_j + \alpha_t + X'_{i,t}b + \gamma Defaults_{i,t-} + \epsilon_{i,t}. \tag{4}$$

In this regression, subscript i refers to the firm, j refers to the three-digit SIC code of the firm, t refers to the loan's origination year, and t- refers to the six month period before the loan issuance. If γ is positive one default to the lead arranger's loan portfolio six months prior to this loan increases covenant strictness by γ, or in other words, increases the probability that the borrower will violate at least one covenant over the next quarter by γ.

In the second-stage regression, I exploit the variation in covenant strictness coming from the number of defaults to lenders' loan portfolios and estimate the effect of covenant strictness

on future efficiency. The second-stage regression therefore is

$$\Delta(ROA)_{i,t+1} = \alpha_j + \alpha_t + X'_{i,t}b + \delta\widehat{Strictness}_{i,t} + \eta_{i,t}. \quad (5)$$

The dependent variable in the second-stage regression is the change in firms' profitability one year after the origination date of the loan. The coefficient δ next to the predicted strictness is the estimate for the change in firms' operating efficiency due to changes in covenant strictness. The main measure of efficiency that I examine is profitability (ROA), but I also use the same empirical methodology for the other profit and cost-efficiency measures. Along with the point estimates of covenant strictness, I use the Anderson and Rubin (AR) Wald test to test the null hypothesis that the instrument is not relevant and weak.

II. Empirical Results

In this section, I present the empirical evidence from estimating the causal effect of covenant strictness on operating efficiency. The main variable of interest is the change in profitability (ROA) one year after the loan. In the first part of this section, I verify the validity of the IV by regressing debt covenant strictness on the number of defaults on the portfolio of the lead arranger 180 days prior to the origination of the loan. In the second part, I present the results from IV/2SLS regressions of profitability on debt covenant strictness.

A. First Stage: The Effect of Lender Defaults on Covenant Strictness

To extract variation in covenant strictness that is exogenous to firm characteristics, I first regress covenant strictness on the number of recent defaults in the loan portfolio of the lead

arranger as in equation (4). A positive and significant γ in this regression indicates an increase in the number of defaults in the portfolio of the lead arranger shortly before making a new loan leads to an increase in covenant strictness of new loans. To ensure lenders change the strictness of debt covenants independently of firm characteristics, I construct the default-count variable so that it includes only defaults coming from firms in different industries and different states from the current borrower. This procedure does not significantly affect the strength of the instrument (relatedness), but makes the exclusion restriction more plausible.

Table II shows the results of estimating equation (4) following the methodology of Murfin (2012). This step is important in proving the instrument is relevant. The baseline regression in the first column has no fixed effects and includes controls only for firm characteristics, such as debt/assets (control for leverage), fixed-charge coverage (cash flow), current ratio (liquidity), and total assets (size), loan characteristics such as loan maturity, the number of participating lenders, the amount of the loan, and the loan spread. The coefficient of lender defaults is positive and statistically significant, which implies shocks in the portfolio of the lead lender affect the strictness of covenants in new loans. The economic magnitude of the estimated coefficient in the second column suggests a one-standard-deviation increase in lenders' defaults (=2.2) increases covenant strictness approximately by 1.8% (=2.2*0.82), or in other words, the median firm is 1.8% more likely to violate a covenant in the next quarter.[8]

[Insert II here]

The results in Table II suggest the IV satisfies relatedness. In the first-column regression of Table II, using no loan or lender characteristics or fixed effects, the F-statistic is significant

[8] A 1.8% increase in strictness means that over a one-year horizon, the probability that the median firm will violate at least one covenant rises from 54.7% to 59.2%. The probability of violating a covenant one year after the beginning of the loan is calculated using the following formula: $\sum_{t \leq T} p(1-p)^{t-1}$, where p stands for covenant strictness, or the probability of violating a covenant in the next quarter.

at the 1% level. To isolate other macroeconomic factors, or industry and firm characteristics that could affect covenant strictness, I use year, industry, rating, loan purpose, and lender fixed effects in the middle column. The role of fixed effects in the regressions is to absorb the variation in covenant strictness explained within a certain year, within a certain lender, a certain industry, rating group, and loan purpose. The introduction of these fixed effects does not significantly change the magnitude or the statistical significance of the effects of lenders' defaults in covenant strictness.

Lenders may change their risk profile after experiencing defaults in their portfolio, and may shift toward safer firms. If lenders indeed switch to safer or more profitable firms after suffering more defaults, the effect of defaults on lenders portfolios should yield the opposite effect on covenant strictness, because safer borrowers can get loans with looser covenants, and therefore defaults would lead to either no effect or looser covenants. To address the possibility that unobserved firm characteristics drive the results, the last column on Table II presents the first-stage-regression results using firm fixed effects. In this regression, the coefficients are driven from variation of strictness within the firm, and suggest that a larger-than-average number of defaults in the portfolio of the lender leads to an increase in the strictness of this firm's debt covenants. The panel dataset is unbalanced, and more than half the firms in the sample have at most two loans. As a result, the time-series variation within firms is not sufficient to exploit using firm fixed effects. For the remainder of my empirical analysis, therefore, I use industry (SIC-3) fixed effects instead of firm fixed effects, even though with firm fixed effects, the economic magnitudes of the second-stage regressions increase. Finally, using lender fixed effects, I also control for the effects of unobserved lender characteristics on debt contracting.[9]

[9]Another common method researchers use in the literature is demeaning the dependent variable (default count) by subtracting the average default count by lender. Gormley and Matsa (2014) assert that demeaning the dependent variable produces inconsistent estimates, and recommend using fixed effects instead. For this

A.1. Financial Crisis Subsample

The regressions in Table II do not include loans originated during the financial crisis of 2007-2009. During the crisis, a greater number of corporate defaults in the portfolio of a lead lender might not necessarily reflect the lender's inability to appropriately screen and monitor its borrowers, but could be the outcome of an ailing economy. Therefore, even if lenders do not experience a larger than average number of defaults in their portfolio, they might still impose stricter covenants due to an overall tightening of credit standards during the financial crisis.[10] As a result, the number of defaults in the lead lender's portfolio (the IV) does not have a statistically significant effect on debt covenant strictness (regression results not tabulated). Therefore, to ensure that the IV remains valid throughout the sample period, the empirical analysis in this paper excludes the financial crisis.

B. The Causal Effect of Covenant Strictness on Firm Performance

With a valid instrument for covenant strictness, I regress the change in profitability one year after the origination of the loan on the first-stage estimates of covenant strictness as in equation (5). The first three columns of Table III present the OLS regression results using the endogenous IV of interest, covenant strictness. The last three columns present the IV/2SLS regression results after using an instrument for covenant strictness. In all regressions, I use industry (SIC-3), lender, year, loan purpose, and rating fixed effects. In columns 3 and 6, I replace industry fixed effects with more restrictive borrower fixed effects. I cluster standard errors at both the firm and lender level. According to Petersen (2009), clustering at the firm level addresses the bias in the standard errors due to time-series correlation within firms, and

reason, I use lender fixed effects for all regressions.

[10] See the Federal Reserve survey of senior loan officers (Board of Governors of the Federal Reserve System)

clustering at the lender level addresses the bias from correlation within lenders. To control for mean reversion and non-linearity in the regressions, I also include lagged profitability and its squared value.

[Insert Table III here]

The results from the OLS regressions in the first three columns of Table III indicate stricter covenants do not affect future profitability. The regression in the first column uses the full sample, and the results suggest covenant strictness has a negative and statistically insignificant effect on profitability. However, this empirical strategy causes difficulty in distinguishing whether restrictive covenants have no effect on firm performance, or whether the negative correlation between ROA and covenant strictness (simultaneity) creates a downward bias for OLS estimates.

The results change considerably once we focus on variation in covenant strictness that comes from the number of defaults in the lender's loan portfolio, and is therefore plausibly exogenous to firm characteristics. Column 4 of Table III presents the results of the full sample IV/2SLS regressions. The effect of covenant strictness on profitability is positive and economically large. To evaluate the relative magnitude, an increase in covenant strictness by 10%, which is equivalent to the difference in strictness between A- and BBB-rated firms, leads to a 1.3% increase in profitability.

I also report the p-value from the Anderson-Rubin (AR) (1949) Wald test, which is robust to weak instruments. The null hypothesis in this test is that the IV is not relevant, and therefore, rejecting the null becomes harder as the instrument becomes weaker. In this sense, the AR p-value in column 4 of Table IV implies that we can reject the null hypothesis that the instrument is not relevant and weak.[11]

[11] Stock and Yogo (2005) simulate critical values based on which we can reject the null that an instrument

B.2. Covenant Strictness and Borrower-Lender Relationship

When a lender makes covenants on a new loan more restrictive after experiencing a larger number of defaults in its loan portfolio, borrowers could choose another lender offering slightly looser covenants. Why do borrowers, whose performance has not changed, accept stricter covenants instead of seeking a different lender?

A potential reason for borrowers to accept stricter covenants from their lender is that firms want to maintain their existing relationship with their lender. Banks that have a past credit relationship with a firm have private information about theses firms. As a result, due to the adverse-selection problem, switching to another lender is costly for firms (see Greenbaum et al. (1989), Sharpe (1990), and Rajan (1992)). For instance, Petersen and Rajan (1994, 1995) show that borrowing from many different lenders increases the cost of debt and reduces the amount of credit available to firms, whereas Bharath et al. (2011) find that firms obtain better loan terms by borrowing from a lender with an existing relationship. The key result in Boot (2000) and Boot and Thakor (2000) is that relationship lending is immune to banking competition and mostly affects transaction lending.

If borrowers with good credit quality pick lenders that offer looser covenants, the estimates from the full-sample regressions are likely to be biased downward. I hypothesize that stricter covenants should have a larger effect on the operating efficiency of firms that have an existing relationship with their creditor, because these firms are less likely to switch to a different lender after banks start making covenants more restrictive. To proxy for relationship lending between firms and the lead arranger, I restrict my attention to the group of firms that have borrowed

is weak if the minimum eigenvalue of the first-stage F-statistic matrix, known as the Cragg-Donald F-Statistic, is above these critical values. The Cragg-Donald F-statistic in column 4 is 9.2, which lies between the 10%-15% thresholds reported in Stock and Yogo (2005) (see Table 5.2) and implies the instrument would be considered weak if we limited the size of an IV Wald test to at most 0.1 above its nominal value. The evidence in this section suggest that the instrument is not obviously weak.

at least twice from the same lead arranger in the past.

In Table III, I show the regression results for the group of firms that have an existing relationship with a bank. Columns (2) and (3) present the estimation results of OLS regressions in the subsample of firms that have an existing relationship with their creditor, using industry and firm fixed effects, respectively. The OLS regression results within this subsample are similar to the full-sample OLS estimates in equation (1); debt covenant strictness does not appear to affect firm performance.

In columns (5)-(6), the results of the IV/2SLS regressions are considerably different. Focusing only on the group of firms that maintain their lending relationships, I find the economic effect of strictness on profitability increases considerably. Specifically, a 10% increase in strictness leads to a 2.5% increase in profitability over the next four quarters. The AR p-value in column 4 is such that we reject the null that the instrument is not relevant and weak at the 1% level, whereas the Cragg-Donald statistic is 11.37 and the Kleibergen-Paap Lagrange multiplier F-statistic is 38.233.[12] Overall, these statistics suggest the regressions do not suffer from the weak instrument problem. Using firm fixed effects in column (6), the results remain robust. Specifically, an increase in debt covenant strictness above the firm's average has a statistically and economically large positive effect on firm performance. Taken together, the results in Table III, suggest that stricter covenants have a positive effect on firm performance.

B.2. The Effects of Covenant Strictness on Long-Run Profitability

The empirical evidence suggests stricter debt covenants lead to an increase in firm profitability one year after loan origination. However, strict debt covenants may benefit the firm

[12] According to Stock and Yogo (2005), the critical values for the Cragg-Donald statistic at the 5% significance level is 8.96 when the worst bias relative to OLS estimates is 15%. Therefore, I can only reject the null that the bias relative to OLS estimates is 15% or more.

in the short-term, but at the expense of long-run profitability. I investigate this possibility in Table IV, by testing the effects of debt covenant strictness on firm performance two and three years after the loan origination date. Columns 1-3 in Table IV present the respective results of OLS regressions of firm profitability after one year, two years, and three years on debt covenant strictness. Columns 4-6 present the respective estimates from IV/2SLS regressions.

[Insert IV here]

The regression results in Table IV suggest stricter covenants have a positive effect on firm profitability one year after the origination of the loan, but have no significant long-run effects on firm performance. Because firms renegotiate their loan terms frequently, covenant thresholds could change after renegotiation, which is consistent with a lack of long-run effects of covenants on firm performance. For instance, Roberts and Sufi (2009b) argue that almost 90% of loans are renegotiated prior to their stated maturity leading to considerable amendments to their initial terms. Denis and Wang (2014) present similar evidence, and find renegotiations can lead to either more restrictive or looser contract terms, depending on the post-renegotiation environment and the performance of the firm. The ability to renegotiate loan contract terms highlights the special role of private debt agreements in mitigating information asymmetry between lenders and borrowers, by setting tight loan covenants ex ante, and relaxing them ex post through loan renegotiations (see Garleanu and Zwiebel (2009)).

C. The Effects of Covenant Strictness on Cost Efficiency and R&D

The evidence so far suggests an increase in debt covenant strictness has a positive effect on profitability. In this section, I investigate in more detail how stricter covenants affect profitability. The DuPont identity is a helpful starting point in this analysis. Specifically, I break

down profitability (EBITDA/Assets) into two components, profit margins (EBITDA/Sales) and asset turnover (Sales/Assets), and then test how exogenous variation in covenant strictness affects these efficiency measures.

[Insert V here]

The regression results in the first two columns of Table V suggest the increase in profitability is driven by an increase in profit margins. The coefficient of covenant strictness in the first column is 0.0015 and indicates a 10% increase in strictness leads to a 1.5% increase in profit margins. However, covenant strictness does not affect asset turnover. One explanation is that changes in asset turnover require time and long term-adjustments for the firm. Therefore, it seems intuitive that changes in profit margins drive the observed effect on profitability, at least in the short run, because firms can more easily improve their performance either by increasing prices or by reducing cost.

To verify whether firms indeed reduce cost, I also study how covenants affect two of the most common measures of cost efficiency: cost of goods sold over sales, and administrative expenses over sales. I find that stricter contracts do not have a significant effect on administrative expenses, but they reduce the cost of goods sold. In particular, a 10% increase in debt covenant strictness leads to a 1.3% decrease in operating cost. The empirical evidence suggests therefore that debt covenants increase profitability mostly by reducing cost. However, although statistically weak, the regression in column (5) suggest that stricter covenants also have a negative effect on R&D expenses. Specifically, one within-firm standard deviation increase in covenant strictness ($\approx 10\%$) leads to a 0.58% reduction in R&D, or a reduction of one third of its standard-deviation.

D. The Effects of Covenant Strictness on Earnings Manipulation and Risk Shifting

To ensure compliance with stricter covenants and avoid a costly violation, managers may try to manipulate earnings to show temporary earnings or sale increases (see DeFond and Jiambalvo (1994) Burgstahler and Dichev (1997)). In this case, tighter covenants may not necessarily cause an increase efficiency, but, instead, managers may manipulate earnings to loosen credit constraints. However, banks are also more likely to make covenants more restrictive for firms that are more likely to manipulate earnings. To address this simultaneity, I use exogenous variation in covenant strictness to test directly whether stricter covenants lead to an increase in earnings management.

To test whether firms engage in earnings management, I estimate discretionary current accruals using Jones (1991), and its modified version from Dechow et al. (1995), and regress discretionary accruals on the IV for covenant strictness. The results in Table VI suggest that an exogenous increase in covenant strictness does not have a significant impact on earnings management.

[Insert VI here]

I also investigate whether stricter covenants induce managers to increase the riskiness of the projects they undertake. To measure operational risk, I calculate the trailing standard deviation of four quarterly changes in ROA. Although relatively noisy, this measure derives risk based on profitability instead of financial prices. The evidence in Table VII suggest that strictness covenants also lead to a reduction in operational risk, although the effect becomes statistically only after six quarters.

[Insert VII here]

Taken together, the results in this section are not consistent with the managerial myopia hypothesis. First, the positive effects of covenants on performance do not reverse after two or three years (see Table IV), and there is no evidence suggesting that managers manipulate earnings, or take on more risk.

III. The Governance Role of Debt

A. The Interplay of Debt Governance with Corporate Governance Mechanisms

The positive effects of loan covenants on operating performance raise the following question: if stricter loan covenants improve profitability, why firms do not demand stricter debt covenants in the first place? The optimal set of financial covenants should balance agency conflicts between creditors and shareholders in a way that maximizes the value of the firm (Smith and Warner (1979)).

Managers, however, who negotiate the strictness of loan covenants with creditors, have an incentive to set covenants looser than what would be optimal for the firm to protect their job, maintain their flexibility, and maximize their personal utility (Donaldson (1963) and Myers (1977)). Due to the separation of ownership and control, therefore, it is possible that managers might select debt contract terms that may not be optimal for the firm. Consequently, changes in covenant strictness could lead to significant efficiency gains for the firm, especially when firms lack alternative governance control mechanisms to protect shareholders' interests.

The corporate governance literature has numerous examples that highlight how agency conflicts between managers and shareholders lead to considerable inefficiencies and reduce firm value (Bertrand and Mullainathan (2003), Bebchuk and Cohen (2005), Chhaochharia and

Grinstein (2007), Cohen and Wang (2013)). I hypothesize that if stricter covenants improve operating efficiency by disciplining managers, covenants should mostly affect firms that lack alternative governance mechanisms to protect shareholders' interests (poorly governed firms). This hypothesis is based on theoretical and empirical evidence that highlights the monitoring and disciplining role of debt.[13]

To test this hypothesis, I split the sample into two groups: firms with high and firms with low agency conflicts between managers and shareholders. Specifically, a firm is poorly governed (i.e., has high agency conflicts between shareholders) in the following cases: the fraction of its shares owned by blockholders is below the sample median; the number of anti-takeover provisions is above the sample median; the firm's concentration in its product market is above the median; and the share of independent directors is below 50%. I examine the effect of debt covenant strictness on profitability separately for well-governed and poorly governed firms, and present the results of all regressions in Table VIII.

[Insert Table VIII here]

I first investigate the effect of covenant strictness on profitability of firms with high and firms with low institutional blockholder ownership. Blockholders are large shareholders that own more than 5% of the firm's outstanding shares. Institutional and large investors are usually active shareholders that have a greater incentive to monitor managers and are more likely to intervene when managers do not run the firm efficiently (see Shleifer and Vishny (1986), La Porta et al. (2002), and Gillan and Starks (2007)). Therefore, high institutional blockholder ownership is a common proxy in the corporate governance literature for institutional monitor-

[13] Without being exhaustive, the following papers discuss the role of covenants in monitoring and aligning the incentives of the manager with shareholders: Holmstrom and Tirole (1997), Park (2000), Dichev and Skinner (2002), Gorton and Winton (2003), Ryan and Wiggins (2004), Roberts and Sufi (2009b), and Rauh and Sufi (2010)).

ing, or high internal governance (see Cremers and Nair (2005), Cremers et al. (2007), Chen et al. (2007), Edmans (2009), and Fich et al. (2015)). If stricter debt covenants have a positive effect on firm profitability due to increased monitoring of the manager, increasing covenant strictness should have a larger effect on profitability of firms without large shareholders.

The results of Table VIII suggest an increase in covenant strictness has a large positive effect on profitability when large shareholders are not present. In the first column of Panel A in Table VIII, the effect of debt covenant strictness on firm profitability is almost twice as large if we focus only within firms with low blockholder ownership. Specifically, I find that in firms whose institutional blockholder ownership is below the sample median, a 10% increase in covenant strictness leads to a 2.1% increase in profitability one year after the loan. Moreover, if stricter debt covenants increase firm efficiency and profitability because of the increased monitoring of management, stricter debt covenants should not affect firms that already have a strong form of internal governance. The results in Panel B, Table VIII, are consistent with this hypothesis, suggesting an increase in covenant strictness does not have a statistically significant effect on profitability of firms with high blockholder ownership.

Gompers et al. (2003) provide evidence that firms with more shareholder rights perform better, and Bertrand and Mullainathan (2003) find that when managers are protected from takeovers, they tend to run firms less profitably and less efficiently. Therefore, if strict debt covenants act as a disciplining device for managers, stricter debt covenants should have a larger effect on firms with more anti-takeover provisions and fewer shareholder rights. To test this hypothesis, I collect data on the number of governance provisions for each firm (G-index). The governance index, first constructed by Gompers et al. (2003), is a count measure from 1-24 based on the number of anti-takeovers provisions and the (lack of) shareholder rights.[14]

[14] Andrew Metrick has generously made the data available on his website.

Matching data on shareholder rights significantly reduces my sample, because a large portion of firms in Dealscan are private.

In the second column of panels A and B of Table VIII, I test whether strict debt covenants have a larger effect on performance if managers are protected from takeovers. I group firms in high- and low-governance groups based on whether the governance index is above or below the sample median. The IV/2SLS regression in panel A suggests an increase in debt covenant strictness has a positive effect on profitability, but the effect is statistically significant only for firms whose G-index is above the median.

I also hypothesize that managers are less likely to improve efficiency when they are insulated from competitive threats (see Giroud and Mueller (2011)), and therefore an exogenous increase in covenant strictness should have a larger effect on these firms. According to Shleifer and Vishny (1997), competition is one of the strongest forms of corporate governance mechanisms, forcing firms to minimize costs and improve efficiency. To this end, I split firms into two groups based on the level of competition in their product market. To proxy for competition, I use a text-based measure of competition from Hoberg and Phillips, *TNIC HHI* (see Hoberg and Phillips (2010a), Hoberg and Phillips (2010b)).[15]

Table VIII (third column) presents the results of IV/2SLS regressions of firm profitability on debt covenant strictness for the subsample of firms that operate in product markets with low competition (Panel A) and high competition (Panel B). The results for the low-governance subsample in Panel A suggest a 10% increase in strictness leads to a 2.8% increase in firm profitability. On the contrary, in Panel B, an increase in debt covenant strictness for firms

[15]Unlike traditional industry classifications, Hoberg and Phillips use textual analysis based on 10K reports to define industries based on the product description of each firm. Using these more refined definitions of industries, which are updated yearly and have the same coarseness as SIC-3 industry classification, the authors calculate the Herfindahl Hirchman index (HHI).

that operate in highly competitive product markets does not yield an economically large or statistically significant effect on firm profitability. In this sense, the results in the third column of Table VIII suggest debt covenant strictness acts as a corporate governance substitute, and are similar to Giroud and Mueller (2010, 2011), who show corporate governance mechanisms matter more when competition in firms' product market is lacking.

I next investigate whether stricter debt covenants have a larger effect on performance of firms whose board is dominated by inside directors. Monitoring the manager is the most important task for outside directors (Byrd and Hickman (1992)). I hypothesize that if the majority of directors are not independent (outsiders), managers are less likely to optimize firm performance, and therefore an increase in debt covenants strictness should matter more for this type of firms. Specifically, in the last column of Table VIII, I test how variation in debt covenant strictness affects firms with a majority of inside directors (panel A) and firms with a majority of independent directors (panel B). The results suggest an increase in debt covenant strictness does not have a significant effect on the profitability of firms with a majority of independent directors (>50%). Focusing on the subsample of firms with a majority of inside directors changes the results considerably. Specifically, I find a 10% increase in debt covenant strictness leads to a 1.9% increase in profitability, although the effect is statistically significant only at the 10% level. These results complement the previous findings and are consistent with the idea that debt covenants act as a governance substitute, leading to a positive and economically large increase in firm performance.

B. The Effects of Covenant Strictness on Performance Through Incentives

Previous studies examine the association between covenant violations and firm outcomes. For instance, Chava and Roberts (2008), Roberts et al. (2009), and Nini et al. (2012) find that covenant violations allow creditors to intervene with firms' financial policy and affect their performance. Covenant violations are also associated with a reduction in employment and establishment closures (see Falato and Liang (2016) and Ersahin et al. (2016)).

This paper is different from previous empirical studies focusing on the effects of covenant violations. By excluding covenant violations and the effects of creditor intervention on firm policies, I use exogenous variation in covenant strictness at loan origination to investigate the treatment effect of covenants through incentives. Although the evidence in the previous section are consistent with this interpretation, I also use a more direct method to test whether covenants affect firm performance through incentives.

To test this hypothesis I identify whether a loan package contains performance pricing terms. Performance pricing terms link the cost of debt with firm performance; when firms perform poorly interest rates increase, and when firm performance improves interest rates decline. Creditors use performance pricing as a "carrot and stick" incentive mechanism to motivate managers to exert higher effort (see Manso et al. (2010)). Therefore, stricter covenants should have a larger effect on firms whose loans do not have performance-linked spreads.

[Insert Table IX here]

In Table IX I split firms into two groups, based on whether their cost of debt is linked with the debt to cash flow ratio, or interest coverage ratio. The regression results support the hypothesis that stricter covenants affect performance through incentives. Specifically, an increase in covenant strictness leads to a statistically significant effect on profitability only for

firms whose contracts do not contain cash flow based pricing grids (column 2). This result, therefore, is consistent with the hypothesis that stricter covenants affect firm efficiency through incentives.

IV. Robustness

A. Matching

As a source of exogenous variation in covenant strictness, I use the number of defaults on the portfolio of the lead arranger recently prior to making a new loan. To construct the IV, I calculate for each bank in the sample the number of firms that defaulted on a bond payment. Unlike violations on private agreements, large defaults on public debt are important events for a lender that significantly affect its assessment of its screening and monitoring process. However, this methodology creates an imbalance between firms that borrow from a lead arranger that has suffered at least one default (treated group) and firms that borrow from a lead arranger that is less likely to have made loans to firms that issue bonds (control group). Smaller banks make loans to smaller firms, which are less likely to issue bonds. As a result, firms in the treated group, or firms whose lenders tend to experience defaults in their portfolio, are more likely to be larger and have different characteristics than firms in the control group (firms whose lenders suffer no defaults).

Does the firm-bank match possibly generate a selection bias that drives the results in this paper? Although I explicitly control for firm and bank characteristics in the regressions, to further address this concern, I combine IV regressions with fixed effects on a matched sample. Specifically, I match firms whose lead arranger in the six-month period leading to the new loan had experienced at least one default (treated group) with firms whose lead arranger had

not suffered from any defaults (control group), on the basis of the following characteristics: firm size, book leverage, market-to-book value, past profitability, lead-lender capitalization, and assets. Barber and Lyon (1996) argue that test statistics of empirical methods based on measures of operating performance are well specified when treated firms are matched to a control group of similar pre-treatment performance. Matching firms on these characteristics following the methodology of Abadie and Imbens (2006), I limit my sample in the common support of the treated and control group, and use IV/2SLS regressions to estimate the effect of covenant strictness on firm performance. The second panel of Table X verifies that firms in the treated and control group have similar characteristics, including past performance, and therefore these basic covariates are well balanced.

[Insert Table X here]

Focusing only on the matched sample, the top panel of Table X presents the results from the OLS regression of profitability on covenant strictness (first column) and instrumented covenant strictness (right column). Consistent with the evidence in Table III, an increase in covenant strictness has a positive effect on profitability only once I address the endogeneity between covenant strictness and firm efficiency using an IV. Overall, the results in the matched sample confirm the empirical findings in the previous sections, and suggest an increase in covenant strictness has an economically and statistically significant positive effect on profitability, even after addressing potential confounding factors attributed to significant differences between firms in the treated and control groups.

B. Robustness Tests

I test whether the IV/2SLS methodology spuriously generates a positive effect of covenant strictness on operating performance. I randomly assign defaults in the portfolio of the lead arranger, and simulate 1,000 regressions of profitability on covenant strictness (instrumented) as in column 4 of Table III. Because the IV is random, we should see a statistically significant change in profitability in fewer than five out of 100 simulations (to satisfy the 5% confidence interval). Out of 1,000 simulations, the statistic is greater than 1.96 in only seven instances (Figure 1 in the online Appendix shows the distribution of t-statistics). The simulation results suggest the statistical significance of the IV/2SLS results fall within the desired confidence interval, and the observed results in the previous sections are not the outcome of chance.

[Insert Table XI here]

I also perform a Durbin-Wu-Hausman test (as seen in Hausman (1978)) to examine if estimates from OLS regressions are biased. The results from this test determine whether we need an instrument to identify the relationship between covenant strictness and profitability. To perform this test, I take the residuals from the first-stage regression of covenant strictness on the IV (the defaults count measure), and then include the residuals as a control variable in a regression of profitability on debt covenant strictness. If a significant correlation exists between the unexplained portion of covenant strictness and profitability in the second stage, it would strongly suggest the presence of endogeneity. In Table XI, the residuals from the first-stage regression have a significantly negative correlation with profitability. This result implies the unexplained portion in covenant strictness in a normal OLS framework will cause a downward bias in our estimates.

V. Conclusion

In this paper, I estimate the causal effects of restrictive loan covenants on firm profitability. As an instrument for covenant strictness, I use the number of defaults in the portfolio of the lead arranger leading up to the origination of a new loan. Focusing on syndicated loan contracts, a major source of external finance for firms, I find stricter debt covenants cause an economically and statistically large increase in profitability, and a reduction in operating cost. However, stricter covenants improve firm performance only when large shareholders have no presence on the firm's board, when managers are entrenched, when inside directors dominate the board, or when firms face softer competition in their product market. The results support the view that stricter debt covenants act as a governance substitute and mitigate agency costs generated by the separation of ownership and control.

To my knowledge, this attempt is the first to estimate the causal effect of loan covenants on firms' operating efficiency. The main advantage of the IV approach is that it allows us to generalize the results to a broader spectrum of firms and does not limit the external validity of the results to only a specific type or group of firms. For instance, previous studies exploit threshold discontinuities—such as covenant violations—and estimate the effects of creditor intervention on firm policies on the subsample of poorly performing firms that violate their covenants. This paper suggests creditors can affect firm performance not only through direct intervention, but also indirectly through incentive contracts. The IV in this paper provides exogenous variation in the design of covenants at origination across a broad spectrum of firms, and allows us to identify hidden and potentially important economic relationships between the design of incentive contracts and firm performance.

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APPENDIX: Definition of Variables

This table provides details for the variables used throughout the paper. Accounting data are from Compustat's quarterly file, and loan data are from LPC Dealscan. I winsorize all variables at the 1st and 99th percentile.

Variable Names	Description
Defaults (180 days)	The number of firms whose S&P credit rating changed to default or selective default while they still had an active loan 180 days prior to a new loan made by their lead arranger.
	Firm Characteristics
Book Leverage	Total Debt / Book Assets
Current Ratio	Current Assets / Current Liabilities
Debt/Tangible Net Worth	Total Debt / Tangible Net Worth
EBITDA	Sum of rolling four-quarter operating income before depreciation
EBITDA / Assets	EBITDA / Book Assets (ROA)
Fixed Charge Coverage	EBITDA / (Rolling 4-quarter Interest and Related Expenses + Debt in current liabilities one year prior)
Market Equity	Stock Price × Shares Outstanding
Market-to-Book	(Market Equity + Total Debt + Preferred Stock liquidating value - Deferred Taxes and Investment Tax Credits) / Book Assets
EBITDA / Sales	EBITDA / Sum or rolling four-quarter sales
Sales/Assets	Sum of rolling four-quarter sales / Book Assets
Tangible Net Worth	Net PPE / Book Assets
Total Debt	Short-term debt + Long-term debt
Z-Score	3.3×Pre-tax Income/Assets + 0.999×Sales/Assets + 1.4 ×Retained Earnings/Assets + 1.2 × (Current Assets - Current Liabilities)/Assets +0.6×Mkt Equity/Total Liabilities
	Loan Characteristics
Loan Spread	The All-in-Drawn Spread from Dealscan, for each dollar borrower draw, excluding fees
Covenant Number	Total number of financial covenants in the loan contract
Loan Amount	The sum of all amounts in a certain loan package
Loan Maturity	The number of months between the earliest loan origination date and the latest maturity date in a certain loan package
Loan Purpose	Indicator variables for the following categories reported in DealScan: corporate purposes, debt repayment, working capital, takeover, CP backup, or other
Loan Participants	The total number of participating banks (non lead-arrangers) in a certain loan package
Contract Strictness	Indicates the probability that the firm will violate at least one of its covenants in the next quarter. To construct the measure, I follow the methodology of Murfin (2012). To construct the measure I use covenant definitions from Demerjian and Owens (2014) and also include additional ratios often used in debt contracts
	Governance Characteristics
Blockholder Ownership	The share of a firm's shares owned by shareholders with at least 5% of the firm's outstanding shares (Source: Thomson Reuters 13F)

G-Index	Governance index from Gompers et al. (2003). The measure ranges from between 1-24, with higher values representing lower shareholder rights
TNIC-3-HHI	Measure of product market competition constructed by Hoberg and Phillips (2010a) based on firm pairwise similarity scores from text analysis of firm 10K product descriptions. Data are available at http://cwis.usc.edu/projects/industrydata/.
Board Independence	The share of independent directors in the board of a company (Source: Boardex)
	Bank Characteristics
Bank Assets	Bank Assets (Sources: Compustat NA Bank, Compustat Global, Bankscope)
Bank Capitalization	Shareholder equity / Bank assets (Sources: Compustat NA Bank, Compustat Global, Bankscope)

Tables

Table I: Summary Statistics
This table presents summary statistics of firm, loan, governance, and bank characteristics. Accounting information is from Compustat. Loan information is from LPC Dealscan. Bank information is from Compustat NA Bank, Compustat Global, and Bankscope. Governance variable are from various sources cited in the Appendix. All variables are described in detail in the Appendix.

Variable	**N**	**Mean**	**SD**	**10%**	**50%**	**90%**
			Firm Characteristics			
Total Assets(m.$)	3701	3381.8	6482.1	157.0	1184.8	8418.1
EBITDA/Assets	3701	0.15	0.07	0.07	0.14	0.23
Market Value/Book Value	3701	1.42	0.93	0.64	1.16	2.44
Has SP rating	3701	0.50	0.50	0.00	1.00	1.00
Tangibility	3700	0.35	0.24	0.08	0.29	0.74
Book Leverage	3701	0.27	0.15	0.07	0.26	0.47
Altman-Z	3701	4.75	3.00	1.40	4.31	8.86
			Loan Variables			
Cov.Strictness	3701	22.79	20.79	0.04	18.31	52.91
Maturity (months)	3701	49.92	18.75	21.00	60.00	61.00
Amount(m.$)	3701	475.20	679.14	40.00	250.00	1100.00
Secured	3701	0.47	0.50	0.00	0.00	1.00
Number of Participants	3701	9.76	8.14	1.00	8.00	21.00
Number of Lead Arrangers	3701	1.57	1.06	1.00	1.00	3.00
			Governance Variables			
Blockholder Ownership	2720	0.18	0.14	0.00	0.17	0.37
G-index	1643	9.16	2.57	6.00	9.00	13.00
TNIC-3-HHI	3135	0.20	0.20	0.04	0.13	0.47
Independent Directors(%)	1423	0.56	0.26	0.11	0.64	0.83
			Bank Characteristics			
Bank Assets(b.$)	3409	843.6	754.9	111.0	619.9	2210.9
Bank Capitalization	3409	0.078	0.022	0.05	0.080	0.107

Table II: First-Stage - OLS regressions of covenant strictness on lender defaults
This table presents the estimation results from OLS regressions of covenant strictness. In all regressions the dependent variable is covenant strictness of the loan contract; it ranges from 0-100 and represents the probability that the firm will violate at least one covenant over the next quarter. To construct the loan covenant strictness measure I follow Murfin (2012), see the Appendix Table for detailed explanation of its construction. The independent variable of interest is the number of outstanding loans in the portfolio of the lead arranger that defaulted 180 days prior to the origination of the new loan. In all regressions, I include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). All variables are defined in the Appendix. Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) Cov Strictness	(2) Cov Strictness	(3) Cov Strictness
Lender Defaults (past 180 days)	0.893***	0.825***	0.908***
	(0.207)	(0.217)	(0.302)
$(\text{EBITDA/Assets})_{t-1}$	-54.093***	-55.214***	-58.918***
	(4.787)	(6.640)	(8.800)
Ln(Assets)	-3.088***	-0.679	1.630
	(0.571)	(0.667)	(1.983)
Book Leverage	45.807***	48.504***	50.999***
	(2.741)	(4.123)	(5.938)
Altman Z-score	-0.557***	-0.431**	-0.223
	(0.211)	(0.218)	(0.410)
Observations	3379	3379	3379
Adjusted R^2	0.299	0.390	0.560
Firm-Bank Controls	Yes	Yes	Yes
Year,Lender,Purpose,Rating FEs	No	Yes	Yes
Industry FEs	No	Yes	No
Firm FEs	No	No	Yes

Table III: OLS vs IV/2SLS regressions of firm profitability on covenant strictness

This table presents the estimation results from OLS (1-3) and IV/2SLS (4-6) regressions of changes in profitability (ROA). The dependent variable is the change in EBITDA/Assets one year after the loan origination date. The independent variable of interest in OLS regressions is the strictness of debt covenants (see the Appendix for measure details). In IV/2SLS regressions, the instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. Columns (1) and (4) use the full sample of loans, and columns (2)-(3) and (5)-(6) use the sample of firms that have an existing relationship with the lead lender (at least two loans). In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	OLS			**IV/2SLS**		
	(1)	(2)	(3)	(4)	(5)	(6)
	ΔROA$_{t+1}$	ΔROA$_{t+1}$	ΔROA$_{t+1}$	ΔROA$_{t+1}$	ΔROA$_{t+1}$	ΔROA$_{t+1}$
Cov.Strictness	-0.0000	-0.0000	-0.0002			
	(0.0001)	(0.0001)	(0.0001)			
Cov.Strictness (Instr)				0.0013**	0.0025**	0.0027**
				(0.0007)	(0.0010)	(0.0013)
Ln(Assets)	-0.0130***	-0.0178***	-0.0524***	-0.0126***	-0.0150***	-0.0507***
	(0.0026)	(0.0040)	(0.0094)	(0.0026)	(0.0040)	(0.0136)
Book Leverage	0.0607***	0.0434*	0.0130	-0.0087	-0.1022	-0.1269*
	(0.0137)	(0.0251)	(0.0387)	(0.0375)	(0.0640)	(0.0765)
Altman Z-score	0.0103***	0.0073***	0.0060***	0.0108***	0.0082***	0.0076**
	(0.0009)	(0.0020)	(0.0021)	(0.0010)	(0.0017)	(0.0032)
Observations	2969	1466	1466	2969	1466	1466
R^2	0.182	0.255	0.464	-	-	-
AR p-value	-	-	-	0.001	0.00	0.001
Firm-Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	No	Yes	Yes	No
Firm FEs	No	No	Yes	No	No	Yes

Table IV: IV/2SLS regressions of firm profitability (year +1,+2,+3) on covenant strictness

This table presents the estimation results from OLS and IV/2SLS regressions of changes in profitability (ROA) one, two, and three years after loan origination, respectively. The independent variable of interest is an instrument for the strictness of debt covenants (for details, see the Appendix). The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the loan origination date. In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	OLS			**IV/2SLS**		
	(1) $\Delta(\text{ROA})_{t,t+1}$	(2) $\Delta(\text{ROA})_{t,t+2}$	(3) $\Delta(\text{ROA})_{t,t+3}$	(4) $\Delta(\text{ROA})_{t,t+1}$	(5) $\Delta(\text{ROA})_{t,t+2}$	(6) $\Delta(\text{ROA})_{t,t+3}$
Cov.Strictness	-0.0000	-0.0000	-0.0005**			
	(0.0001)	(0.0001)	(0.0002)			
Cov.Strictness (Instr)				0.0013**	0.0014	0.0023
				(0.0007)	(0.0013)	(0.0019)
Ln(Assets)	-0.0130***	-0.0287***	-0.0363**	-0.0126***	-0.0283***	-0.0320**
	(0.0026)	(0.0093)	(0.0160)	(0.0026)	(0.0088)	(0.0145)
Book Leverage	0.0607***	0.0449**	0.0611	-0.0087	-0.0280	-0.0849
	(0.0137)	(0.0201)	(0.0500)	(0.0375)	(0.0679)	(0.1158)
Altman Z-score	0.0103***	0.0115***	0.0185***	0.0108***	0.0120***	0.0189***
	(0.0009)	(0.0021)	(0.0033)	(0.0010)	(0.0019)	(0.0031)
Observations	2969	2746	2080	2969	2746	2080
Firm-Bank Controls	Yes	Yes	Yes	Yes	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes	Yes

Table V: The effect of covenant strictness on cost efficiency and R&D expenses

Starting from the regression on the left to the right, the dependent is: change in EBITDA over sales (profit margins), change in sales over assets (asset utilization), change in cost of goods sold over sales, change in administrative expenses over sales, and change in R&D expenses over assets. In column (5), I multiply the change in R&D expenses by 100, so the estimated coefficients represent the percentage change in R&D over assets. In all columns, the change in the dependent variable is based on the date of loan origination and the year after. The instrumental variable is the number of defaults the lender has suffered in its portfolio 180 days prior to the loan origination date. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) (Ebitda/Sales)	(2) (Sales/Assets)	(3) (Cost of goods/Sales)	(4) (Adm.Expense/Sales)	(5) (R&D/Assets)
Cov.Strictness (Instr)	0.0015**	-0.0016	-0.0013***	0.0010	-0.0577*
	(0.0007)	(0.0054)	(0.0004)	(0.0014)	(0.0329)
Ln(Assets)	-0.0017	0.0334***	-0.0031*	-0.0040*	0.0329
	(0.0021)	(0.0109)	(0.0017)	(0.0022)	(0.1623)
Book Leverage	-0.0196	0.1728	0.0541***	-0.0512	2.1905
	(0.0373)	(0.2373)	(0.0115)	(0.0623)	(1.6307)
Altman Z-score	0.0064***	-0.0152**	-0.0037***	-0.0005	0.0352
	(0.0010)	(0.0077)	(0.0011)	(0.0016)	(0.0409)
Observations	2981	3052	3027	2696	1127
Firm-Bank Controls	Yes	Yes	Yes	Yes	Yes
Year-Lender-Purp.-Rating FEs	Yes	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes	Yes

Table VI: Do stricter debt covenants affect earnings manipulation?
The dependent variable in column (1) is discretionary current accruals as in Jones (1991) method. In column (2), the dependent variable is discretionary current accruals using the modified Jones (1991) model as in Dechow et al. (1995). The independent variable is an instrument for the strictness of debt covenants. The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)
	Discretionary Cur. Accruals	Discretionary Cur. Accruals–modified
Cov.Strictness (Instr)	0.005	0.006
	(0.006)	(0.007)
Observations	2871	2866
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes

Table VII: Do stricter debt covenants affect risk shifting?
The dependent variable in all regressions is the trailing standard deviation of four quarter changes in Ebitda/Assets (ROA). In columns (1)-(3), the dependent variable is the trailing standard deviation of quarterly changes in ROA four quarters, six quarters, and eight quarters after the origination of the loan, respectively. The independent variable is an instrument for the strictness of debt covenants. The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
	ROA Volatility(4qtr)	ROA Volatility(6qtr)	ROA Volatility(8qtr)
Cov.Strictness (Instr)	-0.0015	-0.0041**	-0.0052**
	(0.0016)	(0.0021)	(0.0022)
Observations	3001	2856	2752
Firm-Bank Controls	Yes	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes

Table VIII: OLS vs. IV/2SLS regressions of firm efficiency on covenant strictness. Low Governance Firms (Panel A) vs High Governance Firms (Panel B)

This table presents the estimation results from IV/2SLS regressions of changes in profitability (ROA). The dependent variable in all regressions across both panels is the change in EBITDA/Assets one year after the loan origination date. The independent variable of interest is an instrument for the strictness of debt covenants. The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. Regressions in Panel A contain only firms that at the date of loan origination have low governance, and in Panel B firms with high governance. A firm is poorly governed if its ownership from large shareholders is below the median (*Blockholders Low*), shareholder rights are low (*G-index high* below median), competition in its product market is below the median (*Competition Low*), and the share of independent directors on the board is less than 50% (*Board independence high*). In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Panel A: Low Governance Firms			
	(1)	(2)	(3)	(4)
	(Blockholders Low)	*(G-index High)*	*(Competition Low)*	*(Board Independence Low)*
	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$
Cov.Strictness (Instr)	0.0021**	0.0024**	0.0028**	0.0019*
	(0.0011)	(0.0010)	(0.0011)	(0.0011)
Observations	981	828	1397	348
Firm-Bank Controls	Yes	Yes	Yes	Yes
Year-Purpose-Rating FEs	Yes	Yes	Yes	Yes
Lender FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes

(Table VIII—*Continued*)

	Panel B: High Governance Firms			
	(1)	(2)	(3)	(4)
	(Blockholders High)	*(G-index Low)*	*(Competition High)*	*(Board Independence High)*
	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$
Cov.Strictness (Instr)	-0.0025	-0.0002	-0.0010	-0.0009
	(0.0018)	(0.0015)	(0.0020)	(0.0009)
Observations	1249	590	1265	926
Firm-Bank Controls	Yes	Yes	Yes	Yes
Year-Purpose-Rating FEs	Yes	Yes	Yes	Yes
Lender FEs	Yes	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes	Yes

Table IX: OLS vs IV/2SLS regressions of firm profitability on covenant strictness - Performance Pricing

This table presents the estimation results from IV/2SLS regressions of changes in profitability (ROA). The dependent variable is the change in EBITDA/Assets one year after the loan origination date. The independent variable of interest in is strictness of debt covenants (see the Appendix for measure details). The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. Column (1) contains firms whose loan spread is linked to their debt to cash flow ratio, or interest coverage ratio. In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)
	(With Performance Pricing)	*(Without Performance Pricing)*
	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$
Cov.Strictness (Instr)	0.0012	0.0021**
	(0.0015)	(0.0009)
Observations	1293	1675
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes

Table X: OLS and IV/2SLS regressions of profitability on covenant strictness in a matched sample
The top panel of this table presents the estimation results from OLS (left column) and IV/2SLS (right column) regressions of changes in firms' profitability one year after the loan origination date on covenant strictness (left column) and its instrument (right column), respectively. I match treated firms (whose principal bank has suffered at least one default in the past 180 days prior to making a loan) with a group of control firms (principal bank has suffered no defaults in the past 180 days prior to the loan origination date) based on size, book leverage and market to book, lagged ROA, lead-lender capitalization, and assets. The bottom panel presents the standardized differences of these variables across treated and control units. The dependent variable is the change in EBITDA/Assets one year after the loan origination date. The independent variable of interest in the OLS regression is covenant strictness. In the IV/2SLS regression, the instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	Matched Sample	
	OLS	*IV/2SLS*
	$\Delta(\text{ROA})_{t+1}$	$\Delta(\text{ROA})_{t+1}$
Cov Strictness	0.0001	
	(0.0001)	
Cov Strictness (Instr)		0.0021**
		(0.0009)
Observations	1243	1243
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes

Covariate Balance	(Defaults=0)	(Defaults>0)	Standardized Diff.	p-value
Ln(Assets)	7.42	7.55	0.13	0.100
MB	1.39	1.45	0.06	0.294
BL	0.27	0.26	-0.01	0.112
ROA_{t-1}	0.15	0.15	0.00	0.895
Capitalization	0.07	0.07	0.00	0.236
Ln(Bank Assets)	13.55	13.54	-0.01	0.715

Table XI: Hausman-Durbin-Wu test of endogeneity

This table presents the estimation results from OLS regressions of changes in profitability, as in Table III (column 5). The dependent variable is the change in EBITDA/Assets a year after the start of the loan. The instrumental variable for contract strictness is the number of defaults in the lender's loan portfolio 180 days before the origination of the loan. The regression includes as an independent variable the residuals from the first-stage regression of contract strictness on the number of lenders' defaults. In all regressions, I include as controls the lagged dependent variable and its squared value. The regressions also include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

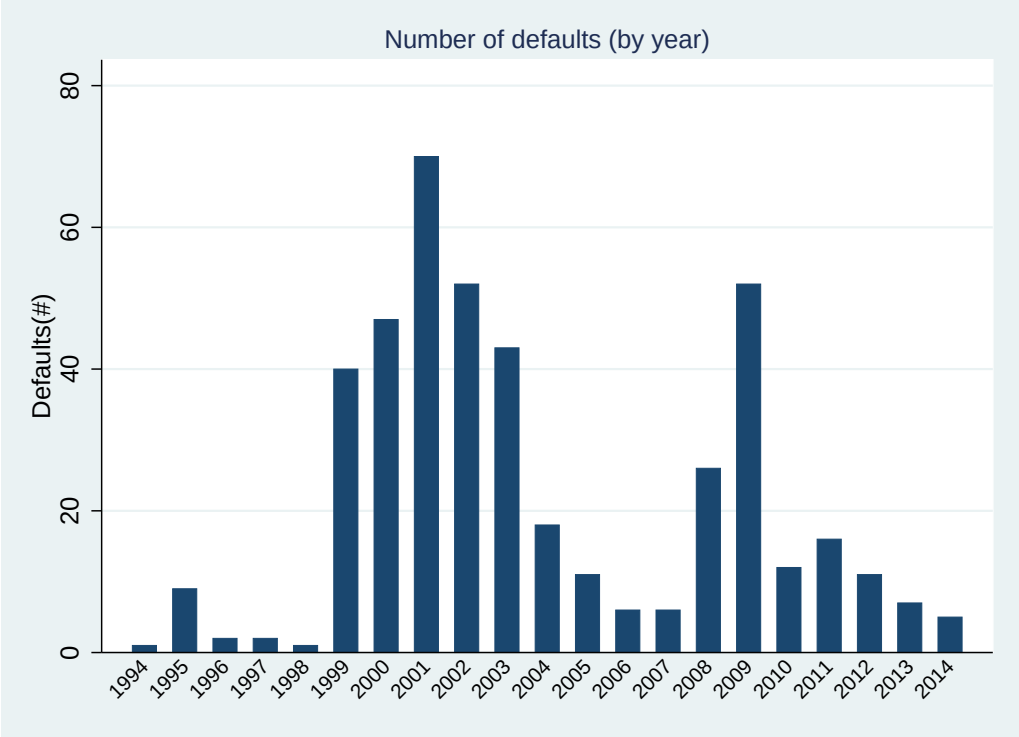
	$\Delta(\text{ROA})_{t+1}$
Cov.Strictness	0.0016**
	(0.0008)
Strictness Residual (Hausman)	-0.0016**
	(0.0008)
Ln(Assets)	-0.0092**
	(0.0038)
Book Leverage	-0.0100
	(0.0385)
Altman Z-score	0.0110***
	(0.0009)
Observations	2976
Adjusted R^2	0.181
Firm-Bank Controls	Yes
Year,Lender,Purpose,Rating FEs	Yes
Industry FEs	Yes

Online Appendix

The Online Appendix contains additional supporting figures and tables not included in this paper. You can find it following this link: https://sites.google.com/site/is10rice/research

Figure 1: Default Distribution

(a) Distribution of yearly corporate defaults from 1990-2014. A firm is in default when its S&P credit rating is downgraded to default or selective default.



(b) Distribution of yearly corporate defaults across SIC-1 industries and across years from 1990-2014. A firm is in default when its S&P credit rating is downgraded to default or selective default.

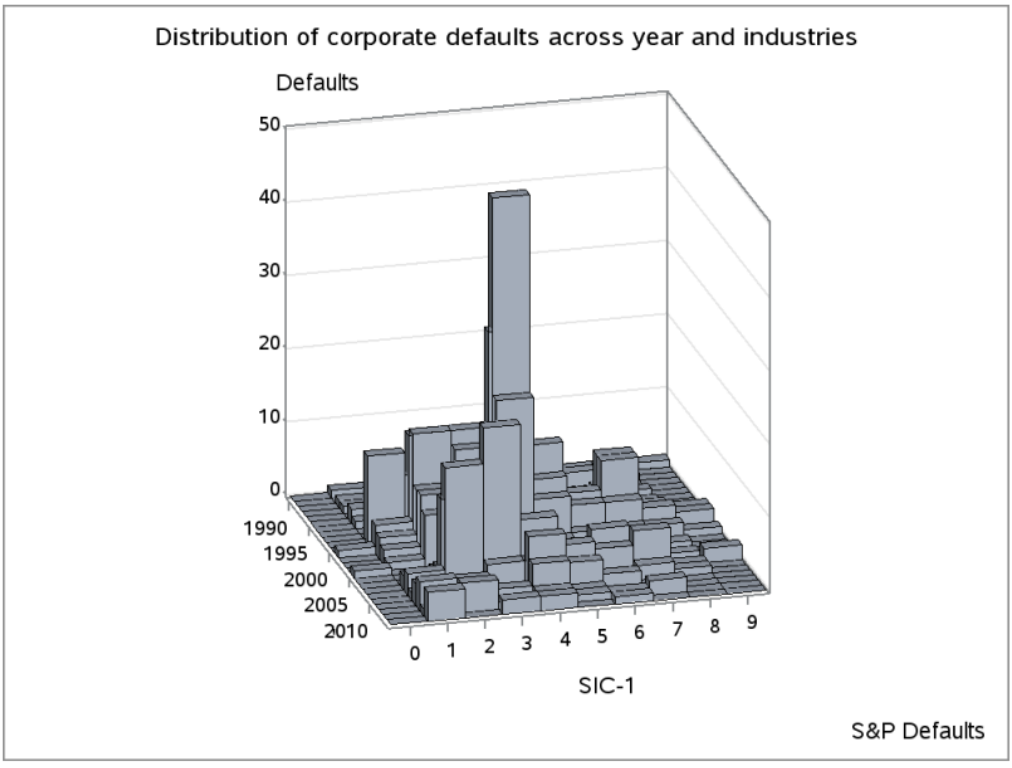


Figure 2: This histogram illustrates the distribution of the t-statistics of 1,000 simulated IV/2SLS regressions similar to column 3 of Table III. The dependent variable in the regressions is the change in profitability one year after the start of the loan, and the instrumental variable is the number of defaults in the lead-lender's loan portfolio in the past 180 days. The simulation results suggest that only 10 out of 1,000 replications incorrectly reject the null of no change in profitability.

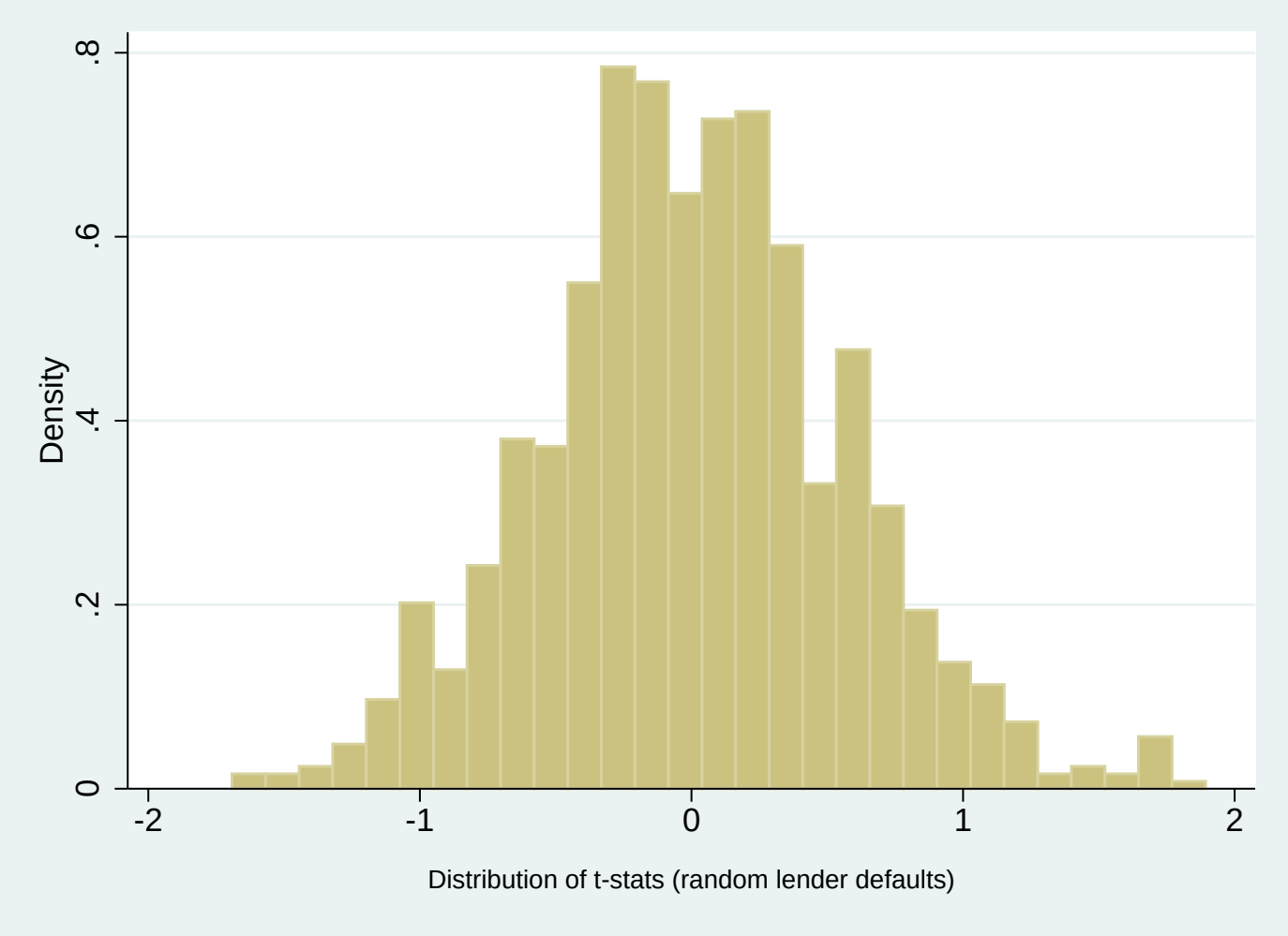


Table A.I: First-Stage - OLS regressions of covenant strictness on the total size of defaulting firms in lender's portfolio

This table presents the estimation results from OLS regressions of covenant strictness. The dependent variable is loan covenant strictness; it ranges from 0-100 and represents the probability that the firm will violate at least one covenant over the next quarter. For a more detailed explanation on the construction of the strictness measure, see the Appendix Table. In regressions 1-3, the independent variables of interest are: the total value of equity, the total value of debt, and the total value of assets of all firms that defaulted during the 180 day period leading up to the origination of a new loan by the lead arranger. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1) Cov.Strictness	(2) Cov.Strictness	(3) Cov.Strictness
Ln(Lender Defaults-Equity)	0.353**		
	(0.161)		
Ln(Lender Defaults-Debt)		0.228***	
		(0.079)	
Ln(Lender Defaults-Assets)			0.227***
			(0.079)
$(\text{EBITDA/Assets})_{t-1}$	-55.439***	-55.439***	-55.426***
	(6.569)	(6.602)	(6.592)
Ln(Assets)	-0.597	-0.616	-0.618
	(0.688)	(0.684)	(0.683)
Book Leverage	48.271***	48.500***	48.460***
	(4.200)	(4.215)	(4.209)
Altman Z-score	-0.415*	-0.403*	-0.406*
	(0.226)	(0.225)	(0.226)
Observations	3296	3296	3296
Adjusted R^2	0.391	0.391	0.391
Firm-Bank Controls	Yes	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes

Table A.II: Regressions of covenant strictness on lenders' defaults. Profit sensitivity
This table presents the estimation results from OLS regressions of covenant strictness. The dependent variable is the strictness of loan covenants, ranging from 0-100, and represents the probability that the firm will violate at least one covenant over the next quarter. The independent variable of interest is the number of outstanding loans in the portfolio of the lead arranger that have defaulted 180 days prior to the origination of the new loan. The variable *High Profit* is a dummy variable that takes the value of 1 if a firm's profitability at the time of contracting is above the median, and zero otherwise. The interaction term (*High Profit(X)Lender Defaults*) captures the difference in lenders' sensitivity to covenant strictness between highly profitable and less profitable firms. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)
	Cov.Strictness	Cov.Strictness
High Profit(X)Lender Defaults	-0.3663	-0.6396
	(0.4661)	(0.4563)
High Profit	-4.0802***	-3.0332**
	(1.0046)	(1.2651)
Lender Defaults (past 180 days)	0.9946***	1.1875***
	(0.3736)	(0.3979)
Ln(Assets)	-2.3718***	0.9775
	(0.8181)	(2.3675)
Book Leverage	47.1032***	52.0181***
	(3.9034)	(5.8557)
Altman Z-score	-0.4793**	-0.2591
	(0.2068)	(0.4234)
Observations	3388	2499
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes

Table A.III: 2SLS regressions: Do stricter debt covenants affect wages, advertising, and employees?
The dependent variable in regressions (1)-(3) is the four change in, respectively: wages divided by sales, advertising expenses divided by sales, and the number of employees divided by sales. To construct these ratios I use data from Compustat. The independent variable is an instrument for the strictness of debt covenants. The IV for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)	(3)
	$\Delta(\text{Wage})_{t+1}$	$\Delta(\text{Advertising})_{t+1}$	$\Delta(\text{Employees})_{t+1}$
Cov.Strictness (Instr)	-0.409	-0.052	-0.008
	(0.746)	(0.169)	(0.008)
Observations	263	869	2851
Firm-Bank Controls	Yes	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes	Yes
Industry FEs	Yes	Yes	Yes

Table A.IV: Do stricter debt covenants affect innovation?
The dependent variable in regressions (1) and (2) is difference in the number of applications for patents that firms file one year after the origination of the loan. Data for patent applications in column (1) is from NBER, and in column (2) from Kogan et al. (2012). The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)–NBER data	(2)–KPSS data
	Δ(Patent Filings)	Δ(Patent Filings)
Cov.Strictness(Instr)	-0.348*	-0.166
	(0.183)	(0.348)
Observations	741	1341
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes

Table A.V: Do stricter Debt/Ebitda covenants affect firm performance?
The independent variable is an instrument for the strictness of debt covenants. The instrumental variable for debt covenant strictness is the number of defaults in the lead arranger's loan portfolio in the past 180 days before the origination of the loan. In the second stage regression, one unit change in the independent variable represents a 10% change in covenant strictness. The regressions include firm controls (size, leverage, cash flows, liquidity, tangible net worth, and market-to-book value), loan controls (maturity, amount, participants, collateral, and spread), and bank controls (size and capitalization). Standard errors are reported below each regression coefficient, and are clustered at the firm and lender level. Significance at the 10%, 5%, and 1% level is indicated by *, **, and ***, respectively.

	(1)	(2)
	Debt/Ebitda Tightness	$\Delta(\text{ROA})_{t+1}$
	First-Stage	*Second-Stage*
Lender Defaults (past 180 days)	0.109***	
	(0.039)	
Debt/Ebitda Tightness (instr)		0.010*
		(0.006)
Observations	3232	2969
Firm-Bank Controls	Yes	Yes
Year,Lender,Purpose,Rating FEs	Yes	Yes
Industry FEs	Yes	Yes