External Governance and Debt Structure

Sreedhar T. Bharath, and Michael Hertzel*

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Abstract

This paper examines how external governance pressure provided by both the product market and the market for corporate control affects the type of debt that firms issue. Consistent with a governance substitution effect, we find that (i) an exogenous increase in governance pressure from the product market has a significant negative impact on the use of bank financing over public debt issuance, and (ii) an exogenous decrease in governance pressure from the takeover market has a significant positive impact on the use of bank financing. Tests using changes in the strictness of loan covenants provides corroborative evidence. Also consistent with bank specialness in providing governance, we find that a bank loan issue causally increases total factor productivity of firms by 1% to 1.6% per year over a bond issue for up to four years after the issuance. We interpret these findings as consistent with the notion that firms endogenously substitute among alternative governance mechanisms in devising a governance structure that allows external capital to be raised at the lowest possible cost and that demand for creditor governance depends on the relative strength of alternative external governance mechanisms.

KEYWORDS: DEBT STRUCTURE, CORPORATE GOVERNANCE, PRODUCT MARKET COMPETITION, PRODUCTIVITY, BANK LOANS , PUBLIC DEBT, NATURAL EXPERIMENT.

JEL CLASSIFICATIONS: G21, G34.

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1 Introduction

The traditional view of corporate governance focuses on the influence that shareholders can exert on managerial decision-making and largely ignores the role that creditors play in the governance process. According to the traditional view, creditors are only active in payment default states, are focused primarily on potential agency conflicts with shareholders, and play only a minor role in governance aimed at reducing managerial slack that arises from the manager-shareholder agency conflict.[1] There is growing evidence consistent with an alternative view that argues that creditors, and banks in particular, have influence that extends outside of payment default to cases of technical default, i.e., where firms have violated debt covenants.[2] As with payment default, technical default results in a transfer of control to creditors and extant evidence shows that creditors impose tighter restrictions on managerial discretion following covenant violations, and that these tighter restrictions affect firm behavior in ways that benefit equityholders as well as debtholders.[3] Moreover, there is evidence of creditor influence that goes beyond the direct effect that renegotiated covenants have on firm behavior. For example, Nini, Smith, and Sufi (2012) report an increase in CEO turnover following covenant violations and suggest that this reflects, in part, informal behind-the-scenes creditor influence.[4]

The evidence on the broader role that creditors play in the overall governance process, suggesting that creditor governance can be effective in controlling managerial slack, raises the question of whether firms exploit this expanded role of creditor influence when devising a governance structure that would allow external capital to be raised at the lowest possible cost. As Nini, Smith, and Sufi (2012) put it "Our results suggest that effective creditor

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[1]Debt *does* have a disciplinary role in the traditional view, but only in the sense that interest payments force managers to disgorge free cash flow (that might otherwise be used for perks) and to return to watchful capital markets rather than bankroll new projects out of existing cash on hand. The key distinction here is that it is the required interest payments that enforce the discipline; the creditors themselves are passive observers in the governance process.

[2]Denis and Wang (2014) show a high frequency of loan renegotiations absent both payment and technical default indicating that creditor influence extends even more broadly outside of default states.

[3]See, for example, Roberts and Sufi (2009) and Nini, Smith, and Sufi (2009).

[4]Anecdotal evidence of active creditor involvement prior to bankruptcy is provided in DeAngelo, DeAngelo, and Wruck (2002) (the case of L.A. Gear) and in Baird and Rasmussen (2006) (the case of Krispy Kreme.)

interventions can boost, or even *substitute* [emphasis added] for, equity-centered governance mechanisms." In this paper, we provide new evidence on the expanded view of creditor governance, by investigating whether firms do indeed substitute between creditor governance and other governance mechanisms aimed at reducing managerial slack. We refer to the possibility that firms endogenously trade-off creditor governance with other governance mechanisms as the "substitution of governance mechanisms" hypothesis.[5]

Our investigation focuses on the decisions that firms make regarding the sourcing of new debt and relies on a broad academic literature that considers debt placement structure, or the *type* of debt that firms issue. One choice in this framework is between borrowing privately from banks (or non-bank private institutions) versus issuing debt in the public market. A large theoretical literature provides a variety of explanations of the costs and benefits of bank versus public debt.[6] A widely held view is that one of the *potential* benefits of bank borrowing is that it allows for an *ex post* governance effect that is not possible when borrowing from widely dispersed investors in the public market. Despite a voluminous empirical banking literature, and especially given the recent findings on the expanded influence of banks in the governance process, there is surprisingly little evidence on whether firms choose bank loans with an eye towards optimizing overall governance structure.

The empirical research on debt structure is largely based on analysis of firms' existing mix of debt claims.[7] Our empirical methodology builds on the incremental approach used in Denis and Mihov (2003), hereafter DM, to analyze the determinants of the source of *new* debt issues.[8] Although, as noted in DM, there are advantages and disadvantages to the incremental approach, an important advantage for our purposes is that it facilitates natural experiments (described below) we use to test for governance mechanism substitution effects.

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[5]We borrow this term from Avedian, Cronqvist, and Weidenmier (2014), which reports evidence consistent with a "substitution of governance mechanisms" hypothesis by showing that, following the creation of the SEC, there was a 30 percent reduction in board independence ...suggesting substitution between market-based (board) governance and government-sponsored (SEC) governance. Other papers that provide evidence consistent with the idea of substitution among governance mechanisms include Aggarwal and Samwick (1999), Cremers, Nair, and Peyer (2008), Giroud and Mueller (2010), and Lel and Miller (2014).

[6]See, for example, Diamond (1984, 1991), Fama (1985), Rajan (1992), Chemmanur and Fulghieri (1994), and Park (2000).

[7]See, for example, Houston and James (1996), Johnson (1997), Krishnaswamy, Spindt, and Subramaniam (1999).

[8]Multinomial logit estimates in Denis and Mihov (2003) show that the primary determinant of debt source is a firm's credit quality with high credit quality firms issuing public debt, lower credit quality firms issuing bank debt, and the lowest credit quality firms issuing non-bank private debt. They also present evidence that the level of asymmetric information and project quality explain debt source.

To test for potential substitution effects, we examine whether external governance pressure imposed by both the product market and the market for corporate control (also known as the takeover market) affect how firms source new debt. The central hypothesis of our study is that strong external governance, as measured by an unfettered and active takeover market, as well as by a competitive product market, reduces the demand for the governance provided by banks and, thus, increases the likelihood that firms will issue debt publicly as opposed to borrowing from banks.

To provide a baseline measure, and preliminary evidence, of the effect of product market competition on the choice between bank loans and public debt, we expand the Denis and Mihov (2003) empirical model of debt choice by including measures of industry competitiveness (based on the Herfindahl-Hirschman Index) as explanatory variables. Using both logit and linear probability estimation procedures, we find a significant negative relation between the intensity of product market competition and the likelihood that firms choose bank loans over public issuance when raising new debt capital. Depending on the specification, we find that firms in competitive industries are up to 9% less likely to source new debt from banks. This evidence is consistent with the hypothesis that the governance provided by a competitive product market reduces the demand for the governance associated with bank borrowing.

Well-known endogeneity concerns make it difficult to determine whether there is a causal link going from industry structure to debt choice. One possibility is that the debt choices that firms make actually affect industry structure.[9] That is, causation may be reversed. Another possibility is that the negative correlation between the choice of bank debt and product market competitiveness may be due to omitted factors (not observable to the econometrician) that affect a firm's debt choice and the competitiveness of the industry it operates in. To address endogeneity concerns, our central tests are based on quasi-natural experiments designed to capture exogenous variation in external governance pressure from both the product market and the market for corporate control.

In our first experiment, we use changes in industry-level import tariff rates as an exogenous source of variation in a firm's competitive environment.[10] Over the past several

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[9]This possibility is suggested by the literature showing that firms can use financial structure to influence product market outcomes (see, for example, Brander and Lewis, 1986, and Bolton and Scharfstein, 1990.)

[10]Previous studies that have used reductions in industry-level import tariffs to identify exogenous increases in the intensity of product market competition include Fresard (2010), Valta (2012), Fresard and Valta (2014), and Lin, Officer, and Zhan (2014).

decades, the softening of trade barriers (e.g., the 1989 Canada-U.S. free trade agreement and the 1994 North American Free Trade Agreement) has resulted in significant decreases in import tariff rates that foreign firms must pay to enter U.S. markets. In effect, import tariff rate reductions facilitate foreign import penetration in U.S. markets resulting in an exogenous increase in competitive pressure that U.S. firms face. If *ex post* creditor governance is an important factor in the decision calculus of firms considering bank loan financing, we should expect to see relatively less bank borrowing following import tariff changes that serve to increase industry competition.

Using import data for U.S. manufacturing industries, we identify 91 large tariff rate reductions between 1975 and 1998 in 74 unique 4-digit SIC industries. For our sample of rate reductions, the average tariff falls by 1.96%, which represents an average of a 6% reduction in tariffs. Using these tariff rate reductions to capture exogenous increases in the intensity of product market competiveness, we find that increased product market competition has a significant negative impact on a firm's reliance on bank debt financing. Based on our coefficient estimates, firms issuing new debt were 11% less likely to choose bank loans over public debt issuance following an increase in industry competitiveness. This finding is consistent with the view that industry competition enforces discipline on managers to reduce slack and maximize profits and that the increase in competitive pressure lowers the need for alternative substitute governance mechanisms such as the creditor governance that comes with bank borrowing.

Our second natural experiment focuses on how governance pressure from the market for corporate control affects firm decisions on sourcing new debt. To identify an exogenous source of variation in takeover market governance pressure, we use the passage of 30 state-by-state antitakeover business combination (BC) laws that served to raise the cost of making hostile takeovers. By reducing the threat of hostile takeovers, BC laws weaken external governance from the takeover market. If firms substitute between creditor- and equity-centered governance mechanisms, we should expect to see a greater reliance on banks for new debt issues following the passage of BC laws.[11]

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[11]See Bertrand and Mullainathan (2003) for a discussion of the restrictions in BC laws and how they reduce the threat of takeovers. We note here that Bertrand and Mullainathan (2003) suggests in the conclusion that antitakeover legislation could be used in future work to investigate the dynamics of corporate governance, asking for example, "Does the number of large shareholders rise to partially compensate for the reduction in threats of hostile takeovers?" In spirit, our analysis of whether there is substitution into bank (i.e., large creditor) governance follows that suggestion.

We find that following the enactment of antitakeover BC laws, firms were significantly more likely to raise debt through a bank loan as opposed to a public debt issue. More specifically, we find that firms were 26% more likely to choose bank loans following the passage of business combination laws. This finding is consistent with the substitution hypothesis prediction that an exogenous decrease in external governance pressure provided by the market for corporate control will be met with an increased reliance on alternative external governance mechanisms such as the creditor governance that comes with bank borrowing. As a check on our results, we also present evidence showing that our findings are robust to concerns raised in Catan and Kahan (2014) and Karpoff and Wittry (2015) regarding the use of BC laws to measure exogenous changes in takeover protection.

Using the same quasi-natural experiments, we develop a novel and independent test of the external governance substitution hypothesis that examines the strictness of bank loan covenants (using data from Murfin (2012)) for firms that took out bank loans both before *and* after the external governance shocks. Consistent with the substitution hypothesis, we find that bank loans covenants were tightened following the passage of BC Laws that reduced external governance and were loosened following the import tariff reductions that served to increase external governance. We view these results as providing strong independent confirmation of the governance substitution hypothesis.

We investigate the cross-sectional nature of our sample to further characterize the effect of changes in external governance pressure on firm debt choice and to bolster support for the validity of our natural experiments. We first investigate whether external governance shocks have different effects in competitive and non-competitive industries. Previous research finds evidence consistent with the hypothesis that external governance shocks should have a less significant impact on firms in competitive industries where there is already significant pressure to reduce slack and improve efficiency. Consistent with this hypothesis, we find strong evidence that the governance mechanism substitution effects we document for the full sample are driven by the subsample of firms operating in non-competitive industries, i.e., we do not observe substitution effects for firms in competitive industries.

Another cross-sectional test we conduct examines whether external governance shocks have differing effects on firms in industries characterized by long-term relationships between customers and suppliers. Cremers, Nair, and Peyer (2008) suggest that the incentives for firms to display good governance are important in these "relationship" industries where

firms attempt to establish and maintain long-term relationships with key stakeholders. The importance of good governance in relationship industries suggests that external governance shocks should generate larger substitution effects in these industries. We find some evidence consistent with this conjecture; industry governance shocks yield larger substitution effects for firms in relationship industries as compared with firms in other industries.

Taken collectively, our evidence suggests that firms recognize the *ex post* monitoring benefits of bank borrowing and tend to choose bank loans over public bond issuance following an external governance shock that increases the demand for creditor involvement in the governance process. While previous research has shown that active bank involvement in governance outside of payment default can benefit the firm overall (i.e., shareholders as well as debtholders), the basis for our choice tests is that banks are special in their ability to perform that function. Our last set of tests provides evidence on bank specialness, by examining and comparing the effects of loan and bond issuances on the subsequent productivity of the issuing firms.

Our tests for the real effects of loan and bond issuances are based on panel regressions of firm-level total factor productivity (TFP) on loan and bond indicator variables that allow for a comparison of a firm's TFP in the year of, and the three years following, a debt issuance to a firm's TFP in years when there are no debt issuances. While the tests, which also include an instrumental variable approach to address concerns about endogeneity, show no difference between the effect of loan issuance and bond issuance on TFP in the year of issuance, the effect of loan issuance on TFP is significantly greater than the effect of bond issuance in the three years following the debt issuance. We interpret these findings as consistent with the view that banks are special in their ability to provide external governance and that firm decisions to substitute into bank governance have real effects.

Our paper contributes to several strands of literature. First, our central finding that firms substitute between bank-centered and equity-centered governance mechanisms provides a new perspective and additional evidence consistent with the view that creditor governance influence (i) extends beyond payment default states, (ii) is not narrowly focused on the agency conflicts between shareholders and bondholders, and (iii) can benefit stockholders as well as debtholders. The evidence of a substitution effect, taken together with our finding that bank loans appear to have real effects on firm productivity, constitute new evidence on the "specialness" of banks. Our findings also add to the literature on the debt structure

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of public firms and, in particular, the choice between bank loans and public debt issuance. Previous evidence on the importance of *ex post* governance in explaining debt choice is limited. To our knowledge, ours is the first study to examine the effect of exogenous shifts in pressure from alternative external governance mechanisms on the choice between bank loans and public debt. Our study also contributes to the literature that investigates the governance implications of competitive product markets and open markets for corporate control. For example, our findings that substitution effects are diminished in competitive markets provides new evidence on the importance of product market governance pressure. Finally, our findings are consistent with the endogenous nature of corporate governance as originally argued in Demsetz and Lehn (1985), and provide some insight into the dynamics of corporate governance.

The remainder of the paper proceeds as follows. Section 2 describes the sample selection and the data. Section 3 presents the empirical results from the baseline regressions, the quasi-natural experiments, and cross-sectional analyses. Section 4 presents an analysis of the relation between debt issuance and post-issuance firm productivity. Section 5 concludes.

2   Data and Variables

2.1   Sample Construction

To investigate the effect of external governance pressure from takeover and product markets on the source of new debt capital, we begin by assembling a large dataset of bank loans and public debt issues over the period 1982-2010. The bank loan data is obtained from the DealScan database and public bond data is from the Securities Data Corporation (SDC) dataset and the Mergent FISD dataset. Accounting data and information on company headquarters comes from Compustat; industry information and competition measures are from the Hoberg-Phillips Data Library (http://alex2.umd.edu/industrydata/). Summary statistics are presented in Table 1.

2.2   Measuring product market competition

Our measures of product market competition are based on the Herfindahl-Hirschman Index (HHI) of industry concentration. A higher level of HHI implies greater industry concentration

and thereby less intense competitive pressure. Because of problems with using HHI based on Compustat data (see Ali, Klasa, and Yeung, 2012) we rely on two other measures of HHI: (i) the fitted HHI from Hoberg and Phillips (2010) that accounts for privately held firms by combining Compustat data with Herfindahl data from the Commerce Department and employee data from the Bureau of Labor Statistics and (ii) the Text-based Network Industry Classification (TNIC) HHI from Hoberg and Phillips (2014). Because of the possibility of a non-linear relation between HHI and debt choice, we employ two indicator variables based on HHI. *Competition1* is an indicator variable set equal to one for all firms below the median value of TNIC HHI. *Competition2* is calculated in a manner similar to *Competition1* except that we substitute missing values of TNIC HHI with the fitted HHI.

2.3 Control variables

In examining the relation between external governance pressure and the source of new debt, we control for firm characteristics including firm size, market-to-book, fixed assets, leverage, profitability, and credit risk. These variables have been employed in earlier studies to capture differences in information asymmetry, project and credit quality, and growth opportunities. The definitions of these variables are in Table 2.

3 Empirical Results

3.1 Baseline estimation

To provide a baseline measure, and preliminary evidence on the effect of product market competition (as a governance device) on the choice between bank borrowing and public debt issuance, Table 2 presents logit (columns 1-3) and linear probability firm fixed effect model (columns 4-6) estimates of the likelihood of a firm issuing a loan as a function of industry competitiveness and firm-specific variables.[12] The dependent variable in all specifications is an indicator variable set equal to one if the firm issued a loan in that year and zero if it issued a bond.

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[12]Because of advantages and disadvantages with both estimation procedures, we follow Nini, Smith, and Sufi (2009) and report results for both logit and linear probability model specifications. We note that the coefficient estimates across the two procedures are, for the most part, qualitatively similar.

As our baseline specification, and for the purpose of comparison with the findings in Denis and Mihov (2003), columns 1 and 4 present specifications that do not include our HHI measures of industry competitiveness. The signs, significance, and interpretation of the coefficients of the explanatory variables in the baseline specification are the same as in Denis and Mihov (2003). Consistent with the hypothesis that firms with lower levels of information asymmetry raise new debt in the public market, we find that firm size (measured by total assets) and the fixed asset ratio are both significantly positively related to the probability that firms raise new debt in the public market. The proxies for project quality and credit quality are significantly positively related to the probability of choosing public debt: more profitable firms are more likely to choose public debt whereas firms facing a higher likelihood of bankruptcy (Altman Z-Score less than -1.81) are more likely to choose bank loans; firms with investment grade debt are more likely to choose public debt whereas firms without a debt rating are more likely to borrow from banks. Finally, as in Denis and Mihov (2003), we find no significant relation between debt source and the firm's market-to-book ratio.[13]

The remaining columns report specifications in which measures of industry competition (below median HHI) are included as explanatory variables. Columns 2 and 5 present results for the specifications that include *Competition1*, which is the measure of industry competition based only on the TNIC HHI data. Columns 3 and 6 report results for the specifications that include *Competition2*, which is our expanded definition of industry competition where missing values of TNIC HHI are replaced with fitted HHI values.

In all four specifications, we find that the coefficients on the competition variables are negative and statistically significant. The negative coefficient estimates imply that, controlling for previously documented determinants of loan choice, firms in more competitive industries are significantly less likely to raise new debt through a bank loan. (We note that the coefficients on the control variables are largely unchanged when the competition variables are included in the model.) The results appear to be economically significant as well. Considering, for example, the linear probability model estimate with the more restricted definition of competition in column 5, firms in competitive industries are approximately 5% less likely to choose a bank loan over a public debt issue. This translates to a little over 9% when measured relative to the average likelihood of a bank loan, which is 53% in our sample.

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[13]The market-to-book ratio is used to capture the importance of growth opportunities. The extant literature finds mixed results on the relation between market-to-book and the use of bank debt.

Although the baseline results are consistent with the hypothesis that product market competition and bank loan monitoring are substitute governance mechanisms, potential concerns with endogeneity bias make it difficult to identify a causal link going from industry structure to debt choice. One possibility is that debt choice may be endogenous to industry structure. Thus, while a negative relation between industry competitiveness and the choice of bank debt may indicate that there is less demand for the governance provided bank monitoring when there is a high level of external governance being provided by a competitive product market, a negative relation may also arise if the choice of bank debt affects industry structure. That is, causation may be reversed. In addition, a negative correlation between the choice of bank debt and product market competitiveness may arise, even if there is no causal relation between them, to the extent that a firm's debt choice and the competitiveness of the industry it operates in are affected by factors that are not observable to the econometrician.

3.2 Quasi-natural experiments

To address concerns about endogeneity, we use (and combine) two quasi-natural experiments to isolate the causal effects of product market governance and takeover market governance on firm decisions regarding the issuance of bank versus public debt. A novel aspect of our two quasi-natural experiments is that they identify exogenous shocks that have differing external governance effects; i.e., one experiment considers an increase in external governance pressure whereas the other identifies an exogenous decrease.

3.2.1 Quasi-natural experiment 1: Reductions in industry-level import tariffs

In the first experiment, following Fresard (2010) and Valta (2012), we use reductions in industry-level import tariffs as exogenous events that serve to increase the intensity of product market competition. Reductions in import tariffs increase competitive intensity by lowering the cost of entering U.S. markets, thereby increasing the competitive pressure that domestic firms face from foreign rivals.[14] In addition to addressing concerns about reverse

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[14] According to the international trade literature, reductions in trade barriers have led to major changes in the competitive landscape of industries. (See Tybout (2003) for a survey.) See Bernard, Jensen, and Schott (2006) for specific evidence showing that the lowering of trade barriers leads to increased competitive pressure from foreign rivals. Consistent with tariff rate reductions leading to import penetration Valta (2012) reports that although import tariffs in his sample decrease, on average, between 1.5% and 3%, import penetration significantly increases from 19.5% to 24.1%.

causality, the import tariff reduction experiment mitigates the omitted variable problem as rate reductions occur for different products at different points in time.

Following the approach in Fresard (2010), we measure reductions in import tariffs at the (4-digit SIC) industry level, using product-level U.S. import data compiled by Feenstra (1996), Feenstra, Romalis, and Schott (2002) and Schott (2010). For each industry-year over our sample period (1982-2010), we compute the *ad valorem* tariff rate as the duties collected by U.S. Customs divided by the Free-on-Board value of imports.[15] To insure we identify substantial increases in product market competitiveness, we focus our analysis on large reductions in import tariff rates. Accordingly, we define that a significant tariff rate reduction occurs for industry i in year t when it is larger than two times the median annual tariff rate change for that industry over our sample period. In addition, to ensure that we are not including transitory tariff rate reductions, we exclude tariff rate cuts that are followed by equivalently large increases in tariff rates over the following three years.

3.2.2 Quasi-natural experiment 2: Enactment of business combination laws

In our second experiment, following Bertrand and Mullainathan (2003) and Giroud and Mueller (2010), we use the passage of 30 business combination (BC) laws (on a state-by-state basis) as a source of exogenous variation in governance pressure from the market for corporate control. By reducing the threat of hostile takeover, BC laws reduce governance pressure from the market for corporate control. Our data on the state-by-state enactment of business combination (BC) laws is from Bertrand and Mullainathan (2003). We also implement the suggestions of Karpoff and Wittry (2015) to address concerns regarding the use of BC laws to measure exogenous changes in takeover protection.

3.2.3 Empirical method for the quasi-natural experiments

Our investigation of the effects of changes in external governance on firm debt choice is based on differences-in-differences tests that rely on three dummy variables. For each firm in industry j we define a dummy variable $ImportPenetration_{j,t}$ that is set equal to one if industry j has experienced a tariff reduction at time t that is larger than two times the median tariff rate reduction in that industry, and zero otherwise. We also set *Import*

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[15]Due to changes in the coding of imports that took place in 1989, we exclude that year in our calculations.

Penetration equal to one for three years after the tariff rate shock at time t, provided that there are no reversals in tariffs over this period. $BCLaw_t$ is a dummy variable that takes the value of one if a firm is incorporated in a state that has passed a BC law by year t. To improve power and to provide a parsimonious measure of an external governance shock, our third dummy variable combines the import tariff reductions and the passage of BC laws in a single positive external governance shock. The combined dummy, *Industry Governance Shock*, equals *Import Penetration – BC Law*. As defined, when *Industry Governance Shock* equals one it is capturing the effect of an increase in external governance. To examine the differences-in-differences effects of external governance shocks on firm debt choice we include these dummy variables in the logit and linear probability model specifications of debt choice reported in Table 2.

3.2.4   Results from the quasi-natural experiments

Table 3 reports the results of our quasi-natural experiments. Columns 1 through 4 present the conditional logit estimates and columns 5 through 8 present estimates of the fixed effects linear probability model specifications. Because of convergence problems with non-linear models, we do not include the Denis and Mihov (2003) control variables in the logit specifications. We note that, although there are disadvantages with the linear probability model approach, the ability to include the control variables has the advantage of allowing us to estimate the impact of external governance shocks on debt choice beyond the effects that these shocks may have on firm-specific characteristics that have been shown to affect the source of new debt.

Results for the import tariff reduction experiment are presented in columns 1 and 5. In both the logit and linear probability model specifications, the coefficient estimates on *Import Penetration* are negative and statistically significant. The negative coefficients imply that firms are less likely to choose bank loans over public debt issuances following significant tariff rate reductions. The coefficient estimates also suggest an economically significant effect. For example, the coefficient estimate of -0.06 in the fixed effects linear probability model specification implies that bank loans are 6% less likely following a competitive shock. Given that the mean likelihood of a bank loan is 53%, this effect represents approximately an 11% decrease in the likelihood of a bank loan when evaluated relative to the mean.

The evidence that firms are less likely to choose bank loans following an increase in the

intensity of product market competition is consistent with the view that industry competition enforces discipline on managers to reduce slack and maximize profits and that increased industry competition lowers the need for alternative governance mechanisms such as the external monitoring that comes with bank borrowing; i.e., the import tariff results are consistent with the hypothesis that firms substitute between alternative governance mechanisms.

The results for the BC Law experiment are reported in columns 2 and 6. In both specifications, the coefficient estimates on *Business Combination Laws* are positive and significant implying that firms are significantly more likely to choose bank loans over public debt issuances following the passage of antitakeover business combination laws that make hostile takeovers more difficult. The passage of BC laws also appears to have a highly significant economic impact on debt source. For example, the coefficient on *Business Combination Laws* is 0.258 in the fixed effects linear specification model implying that firms were approximately 26% more likely to choose bank loans following the passage of laws that reduced the threat of hostile takeovers. When evaluated relative to the mean likelihood of a bank loan, this translates into approximately a 48% increase in the likelihood of choosing a bank loan when issuing new debt.

The evidence that firms are more likely to choose bank loans following the passage of BC laws that have the effect of insulating managers from takeover market discipline is consistent with the idea that firms choose the closer monitoring and restrictive covenants of bank debt as a credible signal that they will pursue an optimal investment policy. In contrast with the case of import penetration, where increased external governance from the product market led to a substitution away from bank governance, here the decrease in external governance provided by the takeover market led to a substitution *into* bank governance. In addition, columns 3 and 7 show that both of the substitution effects are still significant when both dummy variables are included in the specifications. Taken together the results are consistent with the hypothesis that firms will substitute among alternative governance mechanism in optimizing overall governance structure.

Finally, columns 4 and 8 report results from the combined experiment that incorporates both the import tariff reductions and the passage of BC laws in such a manner as to identify a single *positive* external governance shock. For both the logit and linear probability model specifications, the estimated coefficient on *Industry Governance Shock* is negative and highly significant. The coefficient estimate from the linear probability model specification, for

example, implies that firms were 15% less likely (30% less likely when evaluated relative to the mean) to choose bank financing following a shock that served to increase external governance pressure from other sources. Given the results of our separate experiments, it is not surprising that this finding is also consistent with the substitution hypothesis. As noted earlier, the main rationale for the combined experiment is to provide a parsimonious external governance shock measure and to improve power for empirical testing.

3.2.5 Robustness of the BC Law experiment results

In a recent paper, Karpoff and Wittry (2015) (KW, hereafter) argue that tests based on BC laws that attempt to examine the influence of takeover vulnerability on firm decisions may be misspecified depending on their sensitivity to a number of concerns tied to institutional features of the laws' passage. In this section, we discuss the KW concerns and show that our BC law experiment results are robust to the suggestions made by KW to improve identification.

The first issue raised in KW concerns the identifying assumption that the passage of a BC law represents an exogenous increase in the level of takeover protection. The implicit assumption here is that the level of takeover protection was low prior to the BC laws' passage. However, KW point out that until 1982, 38 states had first-generation antitakeover laws that provided extremely high takeover protection (until their repeal by a Supreme Court decision, *EDGAR vs MITE Corp.*, in 1982), thus invalidating the identifying assumption that takeover protection was low prior to the BC laws' passage. By not including data before 1982 in our empirical tests, we sidestep this first concern.

Also tied to the identifying assumption that BC laws generate an increase in takeover protection, KW argue that because firm-level takeover defenses can effectively serve as substitutes for state antitakeover laws, the passage of BC laws may have no appreciable effect on takeover protection for firms with takeover defenses in place. We address this concern in two ways. First, we note that this argument merely serves to bias the experiment against finding significant results. That is, we find results despite this bias. Second, as reported by Gompers, Ishii and Metrick (2003), firm-level takeover defenses, once instituted, are sticky and are rarely changed. By specifically including firm fixed effects in our regressions, the effect of firm-level takeover defenses in place at the time of the BC law passage are differenced out in our tests.

In addition to BC laws, KW notes that there were a host of other antitakeover laws that were passed during the post-1982 sample period. These include control share acquisition, fair price, poison pill, and directors duties laws. KW argues that there is little theoretical justification for focusing only on business combination laws to the exclusion of other types of antitakeover laws. In specifications 2, 4, 5, and 6 of Panel B Table 3, we consider each of these four laws, passed by various states at different points in time, in turn to be the most effective (instead of the BC law in our original tests), in affording takeover protection and repeat our basic choice tests. For each of the antitakeover laws, we find that firms are more likely to issue bank loans after the laws passage (the coefficients on the law dummy variables are all positive and statistically significant), providing support for the KW suggestion that all of these laws might have had an impact on takeover protection levels of firms. In specification 8, we combine all the law passages into one continuous variable (averaging across the five dummy variables) to allow for the simultaneous impact of all antitakeover laws on the firm-level debt issuance decision. (This is similar in spirit to figure 3 of KW.) We find that this composite variable strongly predicts firm-level debt issuance decisions.

Another concern raised in KW is that the incremental impact of an antitakeover law can depend crucially on the legal environment. KW identifies three court rulings (in addition to the 1982 *MITE* decision) that had large effects on the takeover protections offered by various antitakeover laws. We include all of these court rulings and their interaction with the laws, as per the KW suggestion, in our tests in specifications 1 (*Amanda* Decision), 3 (*CTS* decision), and 7 (*Unitrin* Decision). The results in all these specifications indicate that the firm's decision to issue bank debt is robust to the inclusion of the Supreme Court decisions on the interpretation of these laws.

Finally, KW note that empirical researchers can improve their tests by recognizing that many firms do, in fact, opt out of their state laws' coverage or are required to opt into coverage for state antitakeover laws as in Georgia and Tennessee. We specifically exclude Georgia and Tennessee firms in our tests in specification 9 and find that our results continue to hold.

We believe that the robustness tests and discussion above should bolster confidence in our conclusion that firms substitute into alternative external governance mechanisms when state antitakeover laws reduce the external governance provided by the takeover market. For purposes of exposition, we focus on BC laws throughout the remainder of the paper, but

note that our results generally hold when the other antitakeover laws are considered.

3.2.6 Covenant tests of the external governance substitution hypothesis

The results of our quasi-natural experiments provide strong support for the hypothesis that a firm's choice of debt type reflects its need for external governance. To summarize, we find that firms switch their choice of debt type to bank loans after a shock that reduces external governance pressure and switch away from bank loans to public debt issues following shocks that increase external governance pressure.

In this section, we develop a novel and independent test of the external governance substitution hypothesis that focuses on the strictness of bank loan covenants for firms that contracted bank loans both before *and* after the external governance shocks used in our quasi-natural experiments. Our quasi-natural experiments do not meaningfully include these firms in measuring substitution effects, since a continuation of using bank loans implies no change in bank loan governance. In other words, we consider the change in the extensive margin of debt usage in those tests. With the covenant strictness tests we examine the change in the intensive margin of debt usage. We posit that when firms continue to rely on bank loans, we can measure changes in bank loan governance by changes in the strictness of loan covenants. We use the measure of loan covenant (contract) strictness developed in Murfin (2012). In effect, the Murfin covenant strictness measure approximates the probability that the lender will receive contingent control via a covenant violation and thus be in a position to exert governance influence.

There are several desirable features of the covenant strictness tests. By focusing on firms that do not switch the type of debt as in earlier estimations, this test provides an independent assessment of the governance substitution hypothesis using a totally new set of firms, and a dependent variable (covenant strictness) tightly linked to theory.[16] Further, the two quasi-natural experiments have opposite predictions on this measure: tariff reductions leading to import penetration will increase competition and external governance and should thus lead to a decrease in covenant strictness, while BC laws (or other antitakeover laws) which decrease external governance should lead to an increase in covenant strictness.

Table 4 reports the results of the covenant strictness tests. We run firm fixed effects

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[16]See, for example, Rajan and Winton (1995) on the use of loan covenants as a contractual device that increases the incentive and ability for banks to exert a governance role.

regressions of loan covenant strictness on our external governance shock variables. While specification 1 shows that banks tightened their covenants (increasing the probability of a covenant violation by 10.6%) after the passage of BC laws, specification 2 shows that banks loosened their covenants (decreasing the probability of a covenant violation by 4.3%) after the import penetration due to reduction in tariffs. These results continue to be statistically and economically significant in specification 3 where we include both experiments in the same specification. Using all takeover laws as suggested by KW (2015) yields the same inference in specifications 4 and 5. Overall, the covenant strictness results provide strong independent confirmation of the governance substitution hypothesis.

3.3   Cross-sectional analysis of the effect of external governance

In this section we investigate the cross-sectional nature of our sample to further characterize the effect of external governance shocks on firm debt choice and to bolster support for the validity of our natural experiments.

3.3.1   Competitive vs. non-competitive industries

Previous research provides evidence consistent with the likelihood that external governance shocks will have more significant effects for firms in less competitive (more concentrated) industries. For example, Giroud and Mueller (2010) test whether the corporate governance provided by the market for corporate control matters for competitive industries using the passage of the 30 state-by-state BC laws that we study here. Consistent with the hypothesis that product market competition mitigates managerial agency problems, they find that firms in non-competitive industries have significant declines in operating performance following the laws' passage, whereas there are no significant changes in performance for firms in competitive industries.

In a similar fashion, we expect an external governance shock to generate a smaller substitution effect for firms in competitive industries as these firms are already exposed to significant product market discipline. Table 5, Panel A, reports logit firm fixed effects differences-in-differences estimates from our natural experiments broken out for competitive and concentrated industries. In specifications 1 and 2, a competitive industry is defined as one for which our expanded definition of industry competition, *Competition2*, is below

the median HHI and a concentrated industry is where *Competition2* is above the median HHI. In specifications 3 through 6, we define a competitive industry as an industry in the lowest quartile of HHI and a concentrated industry as an industry in the highest quartile of HHI. The table also reports chi-square tests (and p-values) for differences between firms in concentrated and competitive industries.

Turning first to the combined experiment, columns 1 and 2 show that the coefficients on *Industry Governance Shocks* are -1.1 and -.60, respectively, for firms in concentrated and competitive industries. Although both coefficients are negative and significant, the chi-square test (p-value = .09) shows a greater substitution effect for firms in concentrated industries. The difference in the substitution effect is more dramatic when we compare coefficient estimates for firms in the upper quartile versus firms in the lower quartile of HHI as reported in columns 3 and 4. The results from this comparison suggest that the overall finding for the full sample, that firms are less likely to choose bank loans over public debt issuance following an increase in external governance is driven by the subsample of firms in the most concentrated industries. More specifically, the coefficient for *Industry Governance Shock* is negative and significant for the subsample of firms in the most concentrated industries, but is not significantly different from zero for firms in competitive industries. The difference between these coefficient estimates is highly significant (p-value = 0.0.) These findings are consistent with the idea that import penetration by rival foreign firms has a more significant disciplining effect on firms in concentrated industries, which, in turn, leads to a greater substitution away from alternative external monitoring mechanisms such as bank loans.

The results for the individual experiments are reported in columns 5 and 6 and mirror the findings of the combined experiment. The findings for the full sample, that an increase in competitive pressure associated with import tariff reductions decreases reliance on bank financing, appears to be driven by firms in non-competitive industries.

3.3.2 Relationship vs. non-relationship industries

Cremers, Nair, and Peyer (2008) provide evidence consistent with the idea that effective corporate governance is more important in "relationship" industries where firms attempt to establish and engage in long-term business relationships with their customers and/or suppliers. According to this view we should expect to see a larger and more significant reaction to an exogenous change in external governance for firms in relationship industries.

We test this prediction using the same set of two-digit SIC code relationship industries as in Cremers et al. (2008). If the incentive for good governance is strongest in relationship industries, then a decrease in external governance should lead to a stronger substitution effect in these industries.

Results, reported in Table 5, Panel B provide some evidence, albeit mixed, consistent with this prediction. While the results from the combined, *Industry Governance Shock*, experiment show a greater substitution effect for firms in relationship industries, the findings from the individual experiments suggest that the combined result is driven by the subsample of firms in the BC law experiment. That is, there appears to be no difference between firms in relationship and non-relationship industries in the import tariff experiment. The results for the BC Law experiment, however, are striking. Following a decrease in takeover pressure, firms in relationship industries are significantly more likely to substitute into bank governance than firms in non-relationship industries (p-value = 0.0). The results are consistent with the idea that signaling good governance by submitting to bank monitoring is especially important in industries where firms need to develop and maintain long-term relationships with stakeholders.

3.4 Results in relation to earlier literature on the cost of debt

In this section, we discuss our findings in the context of earlier studies that examine how exogenous shocks to product and takeover markets affect the cost of debt using the same natural experiments that we use. While none of the earlier studies provide evidence on whether external governance shocks affect the *relative* cost of bank loans versus public debt issuances, the motivating arguments in these studies have implications for the source of new debt and the level of contract strictness. For example, Valta (2012) finds that bank loan spreads increase following industry-level import tariff reductions and concludes that this increase reflects increased competitive risk of incumbent firms due to increased competitive pressure from foreign rivals. If increased competitive risk explains higher loan spreads, extant theoretical arguments and empirical evidence suggest that this would tilt the choice of debt *towards* bank loans instead of *away* from bank loans as we document in this study. In addition, the increased competitive risk argument suggests that loan covenants should become stricter following import tariff reductions, whereas we find the opposite. Consideration

of our import tariff experiment findings in the context of the countervailing explanations of debt source and covenant strictness serves to bolster confidence in the economic importance of the governance substitution hypothesis.

Earlier studies of the relation between takeover market pressure and the cost of debt similarly suggest changes in post-BC law debt type and covenant strictness that are opposite of what we find in this study. These studies generally find a positive relation between takeover pressure and the cost of debt which is attributed the idea that the threat of takeover can be harmful for creditors, if a firm targeted for acquisition responds by leveraging up, making payouts to shareholders, selling liquid assets, and/or focusing firm operations through divestitures and spin-offs.[17] In the context of our analysis, firms facing higher takeover threats would use bank financing with tighter covenants, as banks are better able to curtail these activities and tighter covenants allow banks greater control. Accordingly, we should expect to see a decreased reliance on bank debt and tighter covenants following passage of BC laws that make takeovers more difficult. Our finding that firms tend to substitute into bank governance and that covenants become stricter following enactment of BC laws highlights the broader role that banks play in the governance process and provides additional support for the governance substitution hypothesis.

4   Debt issuance and subsequent firm efficiency

The analysis thus far suggests that firms recognize the *ex post* monitoring benefits of bank borrowing and tend to choose bank loans over bond issuance when there is an external governance shock that increases demand for creditor governance. While previous research has shown that active bank involvement in governance outside of payment default can benefit the firm overall (i.e., shareholders as well as debtholders), the basis for our choice tests is that banks are special in their ability to perform that function. In this section, we provide evidence on bank specialness, by examining and comparing the effects of loan and bond issuances on the subsequent productivity of the issuing firms.

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[17]The earlier studies investigate how impediments in the takeover process, either through state laws restricting takeovers and/or through firm level antitakeover provisions, affect bondholders and the cost of debt. For the most part, these studies find a positive relation between the threat of takeover and the cost of debt. See, for example, Klock, Mansi, and Maxwell (2005), Chava, Livdan, and Purnanandam (2008), Qiu and Yu (2009), and Francis, Hasan, John, and Waisman (2010).

In the analysis that follows, we consider the effect of debt issuance on firm-level total factor productivity (TFP). TFP is a quantitative measure of the efficiency with which firms turn inputs (capital and labor) used in the production process into outputs. We note that productivity and profitability are often used in the finance literature as measures of performance, but the calculation and interpretation of these measures differs. Whereas TFP measures the efficiency with which the firm turns inputs into outputs, profitability provides a measure of the return to shareholders. We focus our initial tests on changes in TFP following debt issuances for several reasons. First, the extant literature suggests that manager-shareholder agency costs are due to "quiet life" preferences of managers that are best detected by examining TFP. Second, and perhaps more important, evidence in Imrohoroglu and Tzel (2014) suggests that increases in TFP are associated with future decreases in ex ante discount rates. This relation is important for our study as our central hypothesis is based on the assumption that firms substitute between alternative governance mechanisms in order provide a governance structure that allows for the raising of external capital at the lowest possible cost. We use their TFP calculations in our estimations. For further details on the computation of these see Imrohoroglu and Tüzel (2014).

Table 6 presents firm fixed effects panel regressions that examine the effects of loan and bond issuance on firm-level TFP. The dependent variable in the panel regressions is firm-level TFP from Imrohoroglu and Tüzel (2014) (available on Selale Tüzel's website). The TFP estimates are calculated using the method of Olley and Pakes (1996) and are purged of industry and time effects. To measure the relation between TFP and debt issuance, we regress TFP on $Loan_t$ and $Bond_t$, which are indicator variables that take the value of one in the year ($t = +1$) of loan and bond issuance, respectively, as well as the three years after issuance ($t = +2$ to $+4$). The coefficient estimates on each of the issuance dummies measure TFP in that year relative to all years where there was no issuance, where we define no issuance years as all years except years $t = +1$ to $+4$. The sample period is January 1982 to December 2009.

Column 1 of Table 6 presents the results for bank loans and shows that coefficient estimates are positive and significant for the year of, and the three years following the bank loan. The coefficient estimate of .021 for year $+1$, for example, implies that TFP in that year was 2.1% higher than TFP in years where there was no issuance. The findings suggest that bank loan issuances are associated with a subsequent improvement in firm productivity

and are consistent with the role of banks as providers of effective governance.

Column 2 reports results for bond issuances. The coefficients are positive and significant for years 1 and 2, but are insignificant for years 3 and 4. Chi-square tests (and associated p-values) for differences between the coefficients for loan and bond issuances on a year-by-year basis suggest that bond issuances have a less significant relation with post-issuance TFP. While the tests show no difference between the effect of bond issuance and loan issuance on TFP in the year of issuance, the effect of loan issuance on TFP is significantly greater than the effect of bond issuance in the three years following the debt issuance. We interpret these findings as consistent with the view that banks are special in their ability to provide external governance and that firm decisions to substitute into bank governance have real effects.

A potential concern with our analysis of the relation between loan issuance and subsequent firm efficiency, as measured by TFP, is that causality may be reversed, i.e., it may be the case that anticipated improvement in firm efficiency leads to the decision to issue a bank loan. Another potential source of endogeneity is that a common unobserved factor explains both loan issuance and post-issue TFP. One potential solution is to identify an instrument that is correlated with the decision to issue a loan, but does not directly affect firm efficiency. One instrument we use is motivated by extant evidence suggesting that the Asian and Russian financial crises resulted in supply-side shocks that restricted the availability of bank debt relative to public debt, but did not have demand-side consequences that directly affected firm efficiency. To capture this effect, we use an indicator variable, *Crisis*, which takes the value of one for firm-years with fiscal year ends between July 1997 and December 1997 (Asian Crisis) and for firm-years with fiscal year ends between August 1998 and December 1998 (Russian Crisis). We also employ a second instrument, *Average loan spread*, which is the average all in spread drawn (AISD) of all completed deals in the Dealscan database in the month prior to the current security issuance. This pre-issue loan spread can affect the decision to issue a loan (via supply-side or demand-side channels) but should not be related to subsequent operating efficiency of the issuing firm.

Table 7 reports the results of our instrumental variable (IV) approach for controlling for the endogeneity of loan issuance. The dependent variable in the first-stage regression is a dummy variable, *Loan*, coded as one for a loan issuance and zero otherwise. In addition to the two instrumental variables, we also include the explanatory variables used to explain debt choice in our earlier analysis (defined in Table 2.) The coefficients on both instrumental

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variables are significant at the 1% level suggesting that *Crisis* and *Average Loan Spread* are sufficiently correlated with bank loan issuance to serve as viable instruments.

Since the dependent variable in the first stage is a binary variable (*Loan*), the first-stage logit is used to estimate the predicted probability of a bank loan, which, in turn, is used as an instrument in the second-stage estimation. The second stage estimation is also presented in Table 7.[18] The first-stage F-statistic is 986.92 and rejects the null that the coefficients on the instruments are insignificantly different from zero at the 1% level. The Kleibergen-Papp rk LM statistic is 697.95 and rejects the null that the model is under identified at the 1% level. Rejection of the null hypothesis of under identification implies full rank and identification.

In the second-stage (IV) regression, the coefficient of *Loan* is 0.156 (significant at the 1% level). Compared to the coefficient (0.021) on $Loan_{+1}$ reported in Table 6 which is a single year estimate, the effect of loan issuance on TFP is over three years is about seven times larger when it is instrumented using *Crisis* and *Average loan spread*. The results of our IV regression suggest that loan issuances are associated with significant causal improvements in issuing firm total factor productivity. This evidence provides a potential channel for the beneficial effects of governance due to bank loan choice. Since these tests do not measure the costs of bank loan issuances to equity holders, the net benefit to the latter is unclear. Of course, these results are consistent with a model where firms choose bank loans optimally and where the total benefits of governance outweigh the total costs.

5 Conclusion

Recent research provides evidence that banks become active in the governance process when firm performance deteriorates, often well before bankruptcy, and that this involvement can serve to reduce managerial slack and, thereby, benefit shareholders as well as debtholders. In this paper, we investigate an implication of these findings: that bank governance can substitute for alternative governance mechanisms aimed at reducing managerial slack. We use natural experiments to identify exogenous changes in external governance pressure from takeover and products markets, and find evidence consistent with a substitution effect. Using reductions in import tariffs to capture an exogenous increase in product market governance

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[18]Since the dependent variable in the second stage is industry and year fixed effects adjusted TFP, we do not explicitly include year fixed effects in the second stage.

pressure, we find that firms substitute away from bank financing. Using the passage of business combination laws to capture an exogenous decrease in governance pressure from the takeover market, we find that firms substitute into bank governance. We interpret these findings as consistent with the notion that firms endogenously substitute among alternative governance mechanisms in devising a governance structure that allows external capital to be raised at the lowest possible cost.

The basis of our tests for governance substitution effects is that banks are special in their ability to provide *ex post* governance that benefits both shareholders and debtholders. To provide evidence on bank specialness, we examine and compare the effects of loan issuances and bond issuances on post-issuance firm productivity. The results of these tests, which also include instrumental variable controls for endogeneity, show that loan issuances have a significantly more positive effect on post-issuance productivity than bond issuances. We interpret these findings as consistent with the view that banks are special in their ability to provide external governance and that firm decisions to substitute into bank governance have real effects. More broadly, these findings contribute to the literature debating bank specialness, which has largely relied on stock price reactions to bank loan announcements and has found conflicting results regarding *ex post* monitoring by banks.

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Table 1 : Characteristics of New Debt Issues : Summary Statistics

Total Debt Issues	54403
Loans	53%
Bonds	47%
Time Period	1982-2010

	Loan Issuers			Bond Issuers		
	N	Median	Mean	Mean	Median	N
Total Assets (Millions $)	27804	663	3770	11591	3858	12187
Market to Book Ratio	22372	1.40	1.73	1.69	1.42	9827
Fixed Assets/Total Assets	27804	0.28	0.33	0.44	0.41	12132
Inv. Grade Rating	27804		0.14	0.49		12187
Profitability	27558	0.13	0.13	0.14	0.14	12140
Book Leverage	27716	0.33	0.37	0.35	0.31	12146

Table 2 : Logit and Linear Probability Model with Firm Fixed Effects

This table presents logit (Columns 1-3) and linear probability firm fixed effects model (Columns 4-6) estimates of the probability of a firm issuing a loan as a function of industry competition and firm-specific variables. The dependent variable in the logit and the linear probability model is a dummy variable that equals one if the firm issued a loan in that year and zero if it issued a bond. Total assets and amount issued are measured in log of billions of dollars. The market-to-book ratio is the book value of assets minus the book value of equity plus the market value of equity divided by the book value of assets. The fixed assets ratio is the ratio of property, plant and equipment to total assets (TA). The investment grade rating is an indicator variable equal to one if the firm has an existing debt rating of BBB or higher, zero otherwise. Not rated is an indicator variable equal to one if the firm has no existing debt rating, zero otherwise. Altman's Z-score is calculated as Z=1.2 (Working Capital/Total Assets)+1.4 (Retained Earnings/Total Assets)+3.3 (Earnings Before Interest and Taxes/Total Assets)+0.6 (Market Value of Equity/Book Value of Liabilities)+0.999 (Net Sales/Total Assets). Profitability is defined as the average ratio of EBITDA/TA over the three years prior to issuance. We use the measure of industry competition based on a Herfindahl index (HHI) obtained using "text-based time varying network industry classifications" (TNIC) of Hoberg and Phillips (2014). We substitute missing values of TNIC HHI with the fitted HHI that accounts for privately held firms by combining Compustat data with Herfindahl data from the Commerce Department and employee data from the Bureau of Labor Statistics (BLS) as in Hoberg and Phillips (2010). Competition1 is a variable that assumes a value of one for all firms below the median of this HHI measure and zero otherwise. Competition2 is obtained similar to Competition1 except that it uses only the TNIC HHI data. The sample period is January 1982–December 2010. $***, **, *$ indicate significance at the 1%, 5%, and 10% levels, respectively. Numbers in parentheses under the coefficients are standard errors clustered by firm.

	(1)	(2)	(3)	(4)	(5)	(6)
Const.	6.375***	6.152***	6.430***	1.667***	1.765***	1.669***
	(.322)	(.357)	(.322)	(.072)	(.095)	(.073)
Competition1		-.218***			-.049***	
		(.062)			(.017)	
Competition2			-.150***			-.024**
			(.053)			(.012)
Total Assets	-.562***	-.519***	-.561***	-.102***	-.109***	-.101***
	(.033)	(.037)	(.033)	(.008)	(.011)	(.008)
Market to Book	.015	.002	.021	.007	.001	.007
	(.026)	(.026)	(.026)	(.005)	(.005)	(.005)
Amount	.338***	.325***	.342***	.073***	.075***	.073***
	(.028)	(.033)	(.028)	(.005)	(.006)	(.005)
Fixed Assets	-1.070***	-.832***	-1.023***	-.138**	-.123*	-.136**
	(.128)	(.145)	(.129)	(.056)	(.074)	(.056)
Investment Grade	-.413***	-.532***	-.426***	-.041*	-.069**	-.040*
	(.076)	(.087)	(.076)	(.021)	(.030)	(.021)
Not Rated	.277***	.410***	.268***	-.061***	-.070***	-.061***
	(.076)	(.092)	(.076)	(.015)	(.019)	(.015)
Z-Score	.264***	.195**	.275***	.057***	.030	.057***
	(.069)	(.077)	(.069)	(.014)	(.019)	(.014)
Profitability	-1.400***	-1.241***	-1.396***	-.263***	-.182*	-.259***
	(.416)	(.455)	(.413)	(.083)	(.107)	(.083)
Leverage	-.516***	-.694***	-.526***	.023	-.004	.024
	(.186)	(.188)	(.188)	(.031)	(.040)	(.031)
Obs.	30136	21333	30136	30136	21333	30136
Pseudo R^2/ R^2	0.166	0.169	0.167	.042	.040	.042

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Const.					1.636***	1.486***	1.486***	1.544***
					(.071)	(.073)	(.073)	(.072)
Import Penetration	-.515***		-.485***		-.059**		-.049**	
	(.185)		(.180)		(.025)		(.024)	
Business Combination Laws		.464***	.447***			.255***	.253***	
		(.153)	(.150)			(.026)	(.026)	
Industry Shocks				-.460***				-.151***
				(.117)				(.018)
Assets					-.098***	-.101***	-.101***	-.100***
					(.008)	(.008)	(.008)	(.008)
Market to Book					.007	.004	.004	.006
					(.005)	(.005)	(.005)	(.005)
Amount					.071***	.070***	.070***	.070***
					(.005)	(.005)	(.005)	(.005)
Fixed Assets					-.157***	-.137**	-.134**	-.137**
					(.054)	(.054)	(.054)	(.054)
Investment Grade					-.043**	-.055***	-.055***	-.050**
					(.020)	(.020)	(.020)	(.020)
Not Rated					-.065***	-.052***	-.052***	-.056***
					(.015)	(.015)	(.015)	(.015)
Z-Score					.055***	.051***	.052***	.054***
					(.013)	(.013)	(.013)	(.013)
Profitability					-.234***	-.227***	-.225***	-.225***
					(.080)	(.079)	(.079)	(.079)
Leverage					.014	.006	.007	.010
					(.030)	(.029)	(.029)	(.029)
Obs.	25127	25127	25127	25127	31864	31864	31864	31864
Pseudo R^2 / R^2	0.001	0.001	0.002	0.002	0.042	0.048	0.048	0.046

Table 3 Panel B: Robustness of Business Combination Laws Experiment Results to Alternate Antitakeover Laws

This table repeats the conditional logit regressions in Table 3, Panel A (Specifications 1-4) but replaces the Business Combination Laws natural experiment with other antitakeover laws suggested as equally important for governance by Karpoff and Wittry (2015). These laws include the Control Share Acquisition Laws, Fair Price Laws, Directors' duties Laws, and Poison Pill Laws whose definition and passage dates follow Karpoff and Wittry (2015). We also include a dummy variable that equals one after the date of the Supreme Court case decision for each law deemed to be decisive in its interpretation. These are the Amanda decision (for Business Combination laws), the CTS decision (for the Control Share Acquisition Law) and the Unitrin Decision (for the Poison Pill Law). All Antitakeover Laws is the average of the five dummy variables (Business Combination Law, Control Share Acquisition Law, Fair Price Law, Directors' duties Law and Poison Pill Law). In specification 9, we exclude firms incoporated in Georgia and Tennessee. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively. Numbers in parentheses under the coefficients are standard errors clustered by firm.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Business Combination Laws	1.204*** (.225)								
Control Share Acquisition Law		1.333*** (.203)	.167 (.469)						
Fair Price Law				1.422*** (.214)					
Directors' Duties Law					.568*** (.110)				
Poison Pill Law						.654*** (.132)	.798*** (.148)		
Amanda Decision	.529*** (.192)								
Business Combination Laws * *Amanda*	.021 (.237)								
CTS Decision			2.247*** (.185)						
Control Share Acquisition Law * CTS			-.243 (.453)						
Unitrin Decision							-.221* (.115)		
Poison Pill Law * *Unitrin*							-.044 (.166)		
All Antitakeover Laws								1.663*** (.168)	1.663*** (.168)
Obs.	24721	24721	24721	24721	24721	24721	24721	24725	24725
Pseudo R2	0.010	0.004	0.022	0.006	0.003	0.004	0.005	0.011	0.011

Table 4: Analysis on Covenant Strictness of Loans

This table examines the strictness of covenants on banks loans contracted by the same firms (in a firm fixed effects framework) before and after the natural experiments considered in Table 3, Panel A. Loan Covenant Strictness is the measure developed by Murfin (2012) which approximates the probability that the lender will receive contingent control via a covenant violation. Firms in this sample, that do not switch from bonds to loans or vice versa, offer an independent test of the creditor governance hypothesis. Import Penetration and Business Combination Laws are variables defined in Table 3, Panel A. All Antitakeover Laws is defined in Table 3, Panel B. $***,**,*$ indicate significance at the 1%, 5%, and 10% levels, respectively. Numbers in parentheses under the coefficients are standard errors clustered by firm.

	(1)	(2)	(3)	(4)	(5)
Const.	13.716***	20.334***	13.825***	16.644***	16.810***
	(2.323)	(.052)	(2.324)	(2.122)	(2.123)
Business Combination Laws	10.646***		10.646***		
	(3.799)		(3.799)		
All Antitakeover Laws				6.365*	6.261*
				(3.772)	(3.773)
Import Penetration		-4.270**	-4.270**		-4.214**
		(2.053)	(2.053)		(2.046)
Obs.	11493	11493	11493	11493	11493
Adj R2	0.002	0.0008	0.003	0.0007	0.001

Table 5 Panel A : Natural Experiments: Cross sectional differences in issuing activity by competitive and concentrated industries

The dependent variable in the conditional logit model (with firm fixed effects) is a dummy variable that equals one if the firm issued a loan in that year and 0 if it issued a bond. Import Penetration$_{j,t}$ equals one if industry j has experienced a tariff rate reduction at time t that is larger than two times the median tariff rate reduction in that industry, and zero otherwise. We also code the Import Penetration variable as a one for three years after the tariff shock at time t (provided there are no reversals of tariffs during this period). Tariff data are from Peter Schott's Web page. Business Combination Laws is a dummy variable that equals one if the firm is incorporated in a state that has passed a Business Combination Law. Industry Governance Shocks is defined as Import Penetration - Business Combination Laws. All other variables are defined in Table 2. The sample period is January 1982–December 2010. In specifications 1 and 2, a concentrated industry is one for which the Competition1 variable defined in table 2 assumes a value of 0 (above median HHI) and a competitive industry is one for which the Competition1 variable above assumes a value of one (below median HHI). In specifications 3-6, we define a competitive industry as an industry in the lowest quartile of HHI (used to generate the Competition1 variable in Table 2) and a concentrated industry as an industry in the highest quartile of HHI. $***, **, *$ indicate significance at the 1%, 5%, and 10% levels, respectively. Numbers in parentheses under the coefficients are standard errors clustered by firm.

	(1)	(2)	(3)	(4)	(5)	(6)
	Conc.	Comp.	Conc.	Comp.	Conc.	Comp.
Industry Governance Shocks	-1.067***	-0.563**	-1.412***	-0.294		
	(.158)	(.249)	(.174)	(.655)		
Import Penetration					-.830***	0.092
					(.228)	(.907)
Business Combination Laws					2.316***	0.718
					(.331)	(.969)
χ^2 Group 1 vs Group 2 diff.	2.92		10.83		4.25	
p.val of diff.	0.088		0.001		0.039	
χ^2 Group 1 vs Group 2 diff.					11.18	
p.val of diff.					0.001	
Obs.	11288	9755	8587	3296	8587	3296
$Pseudo\ R2$	0.007	0.001	0.010	0.001	.0012	0.0002

Table 5 Panel B: Natural Experiments: Cross sectional differences in issuing activity by relationship and non-relationship industries

The dependent variable in the conditional logit (with firm fixed effects) model is a dummy variable that equals one if the firm issued a loan in that year and zero if it issued a bond. Import Penetration$_{j,t}$ equals one if industry j has experienced a tariff rate reduction at time t that is larger than two times the median tariff rate reduction in that industry, and zero otherwise. We also code the Import penetration variable as a one for three years after the tariff shock at time t (provided there are no reversals of tariffs during this period). Tariff data are from Peter Schott's Web page. Business Combination Laws is a dummy variable that equals one if the firm is incorporated in a state that has passed a Business Combination Law. Industry Governance Shocks is defined as Import Penetration - Business Combination Laws. All other variables are defined in Table 2. The sample period is January 1982–December 2010. Relationship Industries are as defined by Cremers, Nair and Peyer (2008), where product market stakeholder relationships give greater incentives to monitor. All other industries are classified as non-relationship industries. $***, **, *$ indicate significance at the 1%, 5%, and 10% levels, respectively. Numbers in parentheses under the coefficients are standard errors clustered by firm.

	(1)	(2)	(3)	(4)
	Non-Rel.	Rel.	Non-Rel.	Rel
Industry Governance Shocks	-0.331**	-.838***		
	(.153)	(.186)		
Import Penetration			-0.371*	-0.477***
			(.213)	(.186)
Business Combination Laws			0.318***	1.577***
			(.115)	(.291)
χ^2 Group 1 vs Group 2 diff.	4.44		0.08	
p.val of diff.	0.035		0.772	
χ^2 Group 1 vs Group 2 diff.			10.95	
p.val of diff.			0.001	
Obs.	17735	7029	17735	7029
Pseudo R2	0.001	0.005	0.001	0.007

Table 6: Firm Fixed Effects, Panel Regressions: Effect of loan and bond issuance on the firm level total factor productivity.

The dependent variable in the panel regressions (with firm and year fixed effects) is the total factor productivity (TFP) calculated using the method of Olley and Pakes (1996) by Imrohoroglu and Tuzel (2014). TFP is calculated after purging industry specific time fixed effect each year. $Loan_{+1}$ ($Bond_{+1}$) represents the year of loan (bond) issuance and we include three years after the issuance date in our regressions. The sample period is January 1982–December 2009. $***, **, *$ indicate significance at the 1%, 5%, and 10% levels, respectively. Numbers in parentheses under the coefficients are standard errors clustered by firm.

	(1)	(2)	(3)
Const.	-.738***	-.737***	-.703***
	(.009)	(.009)	(.010)
$Loan_{+1}$	.021***		
	(.006)		
$Loan_{+2}$	.026***		
	(.006)		
$Loan_{+3}$	.019***		
	(.006)		
$Loan_{+4}$	.018***		
	(.006)		
$Bond_{+1}$		.027**	
		(.011)	
$Bond_{+2}$		.017**	
		(.008)	
$Bond_{+3}$		.003	
		(.012)	
$Bond_{+4}$		.003	
		(.018)	
No Issuance years			-.037***
			(.006)
Obs.	159611	159611	159611
R^2	.128	.128	.129

Specification 1 vs. Specification 2		Difference in Productivity Loan vs. Bond
χ^2: $Loan_{+1}$ vs. $Bond_{+1}$ diff.	0.13	-0.6%
p.val of diff.	0.716	
χ^2: $Loan_{+2}$ vs. $Bond_{+2}$ diff.	10.15	+0.9%
p.val of diff.	0.001	
χ^2: $Loan_{+3}$ vs. $Bond_{+3}$ diff.	12.73	+1.6%
p.val of diff.	0.000	
χ^2: $Loan_{+4}$ vs. $Bond_{+4}$ diff.	6.51	+ 1.5%
p.val of diff.	0.011	

Table 7: Accounting for Endogenity of loan issuance, Panel Regressions: Effect of loan issuance on the firm level total factor productivity.

The dependent variable in the panel regressions (with firm and year fixed effects) model is the issuance of a loan (coded as a one and zero otherwise, in the first stage)and total factor productivity (TFP) calculated using the method of Olley and Pakes (1996) by Imrohoroglu and Tuzel (2014) (in the second stage). TFP is calculated after purging industry specific time fixed effect each year. Average loan spread is the average all in spread drawn (AISD) of all completed deals in the Dealscan database in the month prior to the current security issuance. Crisis is an indicator variable denoting the Asian Crisis and the Russian Crisis. Crisis equals one for firms with fiscal year ends between July 1997 and December 1997 (Asian Crisis) and for firms with fiscal year ends between August 1998 and December 1998 (Russian Crisis) and zero otherwise. All other variables are defined in Table 2. The sample period is January 1982–December 2009. The first stage F-Statistic is a diagnostic of the instrument strength with a rule of thumb being First stage F should be above 10. Kleibergen papp rk LM statistic tests the null of under identification. Rejection of the null implies full rank and identification. $***, **, *$ indicate significance at the 1%, 5%, and 10% levels, respectively. Numbers in parentheses under the coefficients are standard errors clustered by firm.

	Loan
Total Assets	-.169***
	(.039)
Market to Book	.029
	(.021)
Amount Issued	.408***
	(.017)
Fixed Assets	-.572**
	(.258)
Investment Grade	-.230***
	(.074)
Not Rated	.228***
	(.068)
Z- Score	.124**
	(.059)
Profitability	.354
	(.544)
Leverage	-.051
	(.145)
Average Loan Spread$_{Month:-1}$	.006***
	(.0009)
Crisis	-.157***
	(.052)
Obs.	79579
Psuedo R^2	0.048

	TFP
Loan	0.1559***
	(0.0285)
Firm Fixed Effects	Included
First Stage Covariates	Included
First Stage F Statistic	986.92
Kleibergen Papp rk LM	697.95
LM Statistic (p-value)	(0.000)