TWO ESSAYS IN BANKING AND CORPORATE FINANCE

A Dissertation

by

MINGMING AO

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Chair of Committee,	Shane A. Johnson
Co-Chair of Committee,	Christa H.S. Bouwman
Committee Members,	Adam C. Kolasinski
	Wei Wu
	Senyo Tse
Head of Department,	Sorin M. Sorescu

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ABSTRACT

This dissertation attempts to address two research questions. In the first essay "Bank Regulation, Cost of Borrowing, and Product Market Predation on Borrowers", I study the effect of bank regulation on the microeconomic behaviors of industrial firms. Using a regulatory change by the Federal Reserve Board in 2014, a shock that radically modifies requirements for Foreign Banking Organizations (FBOs), relative to domestic Bank Holding Companies (BHCs), and a difference-in-differences approach, I find this new rule causes FBOs to significantly raise loan spreads, relative to domestic BHCs, during the post period. Such actions by FBOs have effects on the investment decisions and financial performance of their borrowers. To deter predation behaviors by their rivals, FBO borrowers cut prices to maintain sales growth. They also cut SG&A expenses and Capex to maintain operating profitability. Despite the strategic adjustments, FBO borrowers in highly competitive markets are still more vulnerable to predation by rivals, as evidenced by a higher probability of being acquired by either rivals or private equity firms. These findings show that bank regulation can have unintended consequences in shaping industry structure through product market competition channel.

In the second essay, "How does lender health affect covenant-violating borrowers?", co-authored with Adam C. Kolasinski and Jack Bao, we study the causal impact of lender health on covenant-violating borrowers. Using the Emerging Market Debt Crises of the late 1990s, a shock that directly impacted some U.S. banks but not their domestic borrowers, and difference-in-differences, we find that banks exposed to the crises become relatively more likely to be stringent with covenant violators. Such stringency has real effects, as covenant violators become more likely to suffer a distressed delisting if their lenders are crisis-exposed. We also find effects for lender health on borrower investments, but these effects are not specific to covenant-violating borrowers.

DEDICATION

To Mia and Marty

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I am deeply indebted to my committee members, Shane A. Johnson, Christa H.S. Bouwman, Adam C. Kolasinski, Wei Wu, and Senyo Tse for their invaluable guidance and continuous encouragement. I will for sure miss those conversations with them on Finance and many other unrelated topics, such as gym and cooking. I must also thank other Mays Finance faculty members outside of my committee. Their expertise in various areas of finance, their knowledge in teaching, and their kind personality all together make my time at College Station a precious memory of my life.

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Contributors

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Chapter 2 of this dissertation was completed by the student independently. Chapter 3 was co-authored with Professor Adam Kolasinski from Texas A&M University and Professor Jack Bao from University of Delaware.

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1. INTRODUCTION

The main theme of my dissertation centers on the interaction between banking and corporate finance, an area that grasps much attention since the recent financial crisis. Understanding how banking industry is linked to the non-financial sectors is both interesting and important, for academia and policymakers.

In the first essay, "Bank Regulation, Cost of Borrowing, and Product Market Predation on Borrowers", I study whether a change in bank regulation causes borrowers to pay higher cost for their syndicated loans and how the increased borrowing cost affect their product market behavior. Banking industry was at the heart of the financial crisis. During post crisis period, policymakers around the world are implementing a wide range of regulations, e.g. Dodd-Frank in the United States and Basel III in all G20 countries, in the hope that these regulations will help maintain the soundness of the financial network and foster sustainable economic growth. Earlier research on the impact of bank regulations on the real economy through the lending channel has primarily focuses on the quantity effect of credit supply. While examining the aggregate lending amount is an important first step, so is the price effect, as bank regulations are normally costly for banks to comply with. A recent research (See Hogan and Burns (2019)) published by Baker Institute at Rice University estimates that banks' total non-interest expenses increased by an average of more than $50 billion per year. These costs have to be beard by some stakeholders of banks. Whether bank regulations make credit more expensive for bank borrowers and whether the price effect is heterogeneous among borrowers is still unknown in the literature. I attempt to provide answers using a regulatory change by the Federal Reserve in early 2014 that imposed substantial compliance costs onto Foreign Banking Organizations (FBOs), relative to domestic Bank Holding Companies (BHCs). Using a differences-in-differences specification, I find FBOs significantly increase loan spreads after the rule change. The increased interest expenses amount to 2% - 4% of operating income, measured by EBIT, of a firm with median income level.

After establishing the causal effect of bank regulation on loan spreads, I turn to examine how

the increased financing cost affects borrowers' product market behavior. Increased borrowing cost put borrowers at disadvantage in their competition with industry peers. Rational predation theory from the industrial organization literature predicts that financial constraints will induce predatory behaviors by rivals (i.e.,Fudenberg and Tirole (1986), Bolton and Scharfstein (1990)). Expecting forthcoming strategic interactions with rivals, affected borrowers may alter their product market strategy to deter predation. I find that although FBO borrowers manage to maintain their sales growth, their gross margin suffers, implying that they cut prices to boost sales. They also cut SG&A expenses and Capex in order to maintain profitability. I further investigate whether FBO borrowers can survive predation and find they face significantly higher probability of being acquired by rivals or private equity firms, especially those operating in highly competitive markets. The M&A result has important implications for anti-trust enforcement because with more firms being acquired by rivals, the industries become more concentrated. To that end, the 2014 regulatory change on FBOs had unintended consequences in shaping industrial market structures.

In the second essay, "How does lender health affect covenant-violating borrowers?", co-authored with Adam Kolasinski and Jack Bao, we study the real effect of lender health on covenant violating borrowers. Financial covenants are an important tool that banks use to monitor corporate borrowers. Because bank loan contracts are generally long-term debt agreement, under normal conditions a shock to banking industry would not be transmitted to borrowers as long as borrowers are in compliance of debt covenant. However, when borrowers violate covenants, control rights are shifted to lenders. That being said, covenant might be a channel through which a shock to banking is transmitted to the real sector. Although the recent financial crisis is an exact example of banking shocks, we carefully avoid using it because both lenders and borrowers are hit hard during the crisis and it is extremely difficult to disentangle lenders' supply effect from borrowers' demand effect.

To achieve clean identification, we follow Chava and Purnanandam (2011) and Lo (2014) to exploit the emerging market sovereign debt and currency crises of 1998–1999, which affected Russia, Brazil and eventually the rest of Latin America, to run a quasi-natural experiment. This

shock adversely affected a subset of U.S. banks, but, unlike the Global Financial Crisis, it did not directly affect U.S. domestic borrowers. We find this exogenous adverse shock causes a significant increase in the odds that affected lenders resolve covenant violations stringently, relative to unaffected lenders. Furthermore, we find an exogenous shock to lender health has real effects: it increases the probability that covenant violations lead to borrower distressed delisting. The effect is mitigated when lenders are far from their minimum regulatory capital ratios before the negative shock to their health. We also find evidence of an effect on borrower investment, but this effect is not unique to covenant-violating firms. Near-violators also reduce their investment. We infer that investment effects apply more generally to unhealthy borrowers of unhealthy lenders, not just covenant-violating borrowers.

Our ability to identify a causal effect of bank health on the stringency with which covenant violations are resolved has important policy implications. In recent years, academics and policymakers have debated the need for additional regulations on banks, weighing the costs and benefits. Reliance on spurious (and general) correlations between bank health and stringent treatment of covenant violators could lead policymakers to incorrectly infer that a deterioration in bank health can impact the real economy through the covenant violation resolution channel. This, in turn, could lead to an overemphasis of the benefits of banking regulation. In contrast, by cleanly identifying a causal effect of bank health on covenant violation resolutions, our study provides reliable evidence that can inform policymakers of the extent that bank health matters in the commercial loan market.

2. BANK REGULATION, COST OF BORROWING, AND PRODUCT MARKET PREDATION ON BORROWERS

2.1 Introduction

I study the effect of bank regulation on borrowers' product market behavior. More specifically, I examine whether bank regulation causes an increase in the cost of bank loans, whether the effect is heterogeneous among borrowers, and how affected firms adjust their product-market strategies. The extent to which bank regulation affects economic activities has been brought into sharp focus since the global financial crisis. To make the financial system more resilient, policymakers around the world are implementing a wide range of regulations, e.g. Dodd-Frank in the United States and Basel III in G20 countries, in the hope that a healthier financial network will foster sustainable economic growth. Although there is an intense debate on the costs and benefits of bank regulation[1], little is known about the influence of bank regulation on borrowers' behavior. Earlier research on the impact of bank regulations on the real economy through the lending channel has focused primarily on the quantity effect instead of the price effect. While examining the aggregate lending amount is an important first step, so is the price effect, especially the heterogeneity of this effect among borrowers, which has not been examined. I attempt to begin filling this gap using a sample of syndicated loans to U.S. public firms between July 2009 and the end of 2016[2] matched with lender and borrower identities. The richness of the merged data enables a thorough causal analysis to address the questions of interest.

To establish a causal effect of bank regulations on loan prices, I use the Federal Reserve's approval of the final rule in 2014 to strengthen supervision and regulation of Foreign Banking

[1]For example, while Admati et al. (2013) argue that bank capital is not socially expensive, practitioners disagree. See https://www.brookings.edu/research/higher-bank-capital-requirements-would-come-at-a-price/ for a discussion by Douglas J. Elliott.

[2]I choose to start the sample period from July 2009 following the NBER's definition on business cycles to avoid the possible contamination by the financial crisis. Compared to studies using observations during the crisis, research using normal period is less confounded by the simultaneity issue between supply and demand as well as the interbank spillovers, thus providing a cleaner identification. Besides, how regulation affects the real economy is not a crisis-specific question. It is important at all times.

Organizations (FBOs) as an exogenous shock to the banking industry. This new rule establishes a series of enhanced standards for FBOs to help increase the resiliency of their U.S. operations, including liquidity, risk management, and capital. The most striking change among these new requirements is that FBOs with a significant U.S. presence are required to establish intermediate holding companies (IHCs) over their U.S. subsidiaries, and these IHCs become subject to the same risk-based and leverage capital standards applicable to domestic bank holding companies (BHCs). Overall, the new regulatory framework significantly increases the compliance costs of FBOs[3]. Although large U.S. bank holding companies are also required to comply with the enhanced prudential standards, many of these standards have previously been adopted or are in the process of being adopted. Hence, this new rule imposes a substantial marginal compliance burden onto FBOs, but not so much to domestic BHCs.

Using a difference-in-differences specification, I find that FBOs significantly increase loan spreads after the rule change relative to domestic BHCs, after controlling borrower, loan and macro characteristics. The increase in loan spreads ranges from 16 to 34 basis points, depending on the definition of the pre-period. This effect is substantial compared with the median loan spread of 200 basis points (mean is 248 basis points) for loans originated during 2009 and 2016.

After establishing the causal effect of bank regulation on loan spreads, I turn to examine how the increased financing cost affects borrowers. Because public firms need to disclose their credit agreements in the SEC filings, the cost disadvantage induced by bank regulation faced by affected borrowers is observable by rivals. Rational predation theory from the industrial organization literature predicts that financial constraints will induce predatory behaviors by rivals (i.e.,Fudenberg and Tirole (1986), Bolton and Scharfstein (1990)). Expecting forthcoming strategic interactions with rivals, affected borrowers may alter their product market strategy to deter predation. Focusing on the subset of borrowers who contract with banks during the pre-period and need to negotiate new loans after the regulatory change, I examine the dynamics of their investment and performance. I

[3]For example, Barclays disclosed in its 20-F filings for 2014 that "the Group currently believes that, in the aggregate, the final rules (and, in particular, the leverage requirements in the final rules that will be applicable to the IHC in 2018) are likely to increase the operational costs and capital requirements and/or require changes to the business mix of the Group's US operations, which ultimately may have an adverse effect on the Group's overall result of operations".

find that although FBO borrowers manage to maintain their sales growth, their gross margin suffers, implying that they cut prices to boost sales. They also cut SG&A expenses and Capex in order to maintain profitability.

To investigate whether FBO borrowers survive predation by adjusting their product market strategies, I examine their delisting probability during the post period and find they are still more likely to be acquired by rivals or private equity firms, especially those borrowers operating in highly competitive markets. Compared with borrowers of domestic BHCs, FBO borrowers are 11% more likely to be acquired. This magnitude is economically large, given the fact that a typical Dealscan borrower during the same period has a 24% probability of being acquired and for a typical CRSP firm the probability is 4.4%. To address the concern that the observed higher probability of being acquired might be due to characteristics associated with being an FBO borrower, I run a propensity score matching exercise and find quantitatively similar results. This delisting result has important implications for anti-trust enforcement because with more firms being acquired by rivals, the industries become more concentrated. To that end, the 2014 regulatory change on FBOs had unintended consequences in shaping industrial market structures.

This study makes three contributions. First, this is the first paper to causally quantify the pricing effect of bank regulation using data from the post-financial crisis period. While previous research on bank regulation largely focuses on the aggregate lending amount, (e.g., Bernanke, Lown, and Friedman (1991), Berger and Udell (1994)), the price effect is also important because changes in the cost of borrowing can force firms to alter their investment decisions, and the effect will ultimately be translated into the real economy in the form of economic growth and employment. Hubbard, Kuttner, and Palia (2002), Mattes, Steffen, and Wahrenburg (2013), and Santos and Winton (2019) are among several papers examining the loan price effect of bank capital, but they do not establish causality. Lambertini and Mukherjee (2016) use a syndicated loan sample between 1996 and 2015, but they focus only on loans issued by U.S. bank holding companies. This study is the first attempt to establish the price effect using a clean causal identification. In addition to the overall effect on loan spreads, I also investigate the heterogeneity among borrowers. The

finding that banks pass compliance costs to borrowers operating in highly competitive markets is consistent with Valta (2012). It is an important insight that updates the understanding of the asymmetric information problem between borrowers and banks by suggesting that banks incorporate the product market environment of borrowers to make pricing decisions.

Second, I examine how shocks to financing costs interact with product market competition to impact firms' investment decisions. Although there is a large literature on product market strategy and financing policy (See Fudenberg and Tirole (1986), Saloner (1987), and Bolton and Scharfstein (1990)), there are a limited number of papers studying how firms adjust their competition strategies after a shock to their financing costs. Chevalier (1995) and Phillips (1995) are two early papers on product market interaction with capital structure. Kovenock and Phillips (1997) investigate product market behavior of firms that substantially increases debt through leveraged buyouts and recapitalizations. Campello (2003) provide evidence of the effects of capital structure on product market outcomes using shocks to aggregate demand. Povel and Raith (2004) analyze the interaction of output market decisions and financing in a duopoly in which one firm cannot borrow funds to finance production. Finally, Fresard (2010) show the causal effect of cash reserves on product market performances. This paper bridges the literature in industrial organization and corporate finance by examining how firms interact with competitors when a bank regulatory shock alters their financing costs, and how these actions affect their performance and probability of surviving as an independent firm.

Third, this paper adds to the literature on banking and industrial structure. Previous research has analyzed the relation from the dimension of competition in the banking industry. Cetorelli and Strahan (2006) test how competition in local U.S. banking markets affects the firm-size distribution of non-financial sectors, and they find some firms may benefit while others may suffer, depending on the degree of competition. Bertrand, Schoar, and Thesmar (2007) use the French banking reform in 1985 to study how bank deregulation affects the real behavior of firms and the structure and dynamics of product markets. They find less government intervention is associated with an increase in firm entry and exit rates and a reduction in the level of product market concentration.

They suggest that the product market channel could be a micro foundation for the relationship between finance and growth as discussed in King and Levine (1993a), King and Levine (1993b) and Demirgüç-Kunt and Maksimovic (1998). My study is the first attempt to examine how a new bank regulation changes the industrial structure by examining borrowers' exit patterns.

2.2 Literature Review and Hypothesis Development

The interaction between banking and the real economy fits into the broader literature on finance and growth, which could be dated back to Schumpeter (1911). To identify whether and how banking causally affects activities in real sectors, researchers usually exploit exogenous shocks to the banking sector, usually macro-economic shocks[4] or regulatory shocks, to separate the supply effect from the demand effect. On the dimension of regulatory shocks, there could be enhanced regulation and deregulation. The bank branch deregulation occurred from the 1970s through the 1990s in the U.S. is widely used[5]. In the wake of the global financial crisis, a wide range of new and revised rules has been imposed on the U.S. banking industry [6]. Hanson, Kashyap, and Stein (2011) offer a detailed discussion of how a macro-prudential regulation regime might be designed.

Earlier discussion on the impact of bank regulation on the real economy through lending channel includes Bernanke, Lown, and Friedman (1991), Furlong (1992), Berger and Udell (1994), Peek and Rosengren (1995a), Peek and Rosengren (1995b) and Brinkmann and Horvitz (1995) with much attention being paid to credit quantity and less to the cost of credit, as the latter requires more detailed data on borrowers to control for demand and risk considerations. Hubbard, Kuttner, and Palia (2002), Mattes, Steffen, and Wahrenburg (2013) and Santos and Winton (2019)

[4]Examples of research exploiting economic shocks are Amiti and Weinstein (2018), Peek and Rosengren (2000), Gan (2007), Campello et al. (2011), Campello, Graham, and Harvey (2010) and Duchin, Ozbas, and Sensoy (2010). These studies focus on firm investment decisions. There are also papers with interest in employment effects, such as Michaels, Beau Page, and Whited (2018), Benmelech, Bergman, and Seru (2015), Bentolila, Jansen, and Jiménez (2017), Chodorow-Reich (2013) and Duygan-Bump, Levkov, and Montoriol-Garriga (2015).

[5]Strahan et al. (2003) reviews contemporaneous studies analyzing the deregulation effects. Among them Jayaratne and Strahan (1996) use the deregulation event to study the effect on the rate of real, per capita growth in income and output. Chava et al. (2013) use the same event to examine firms' innovation activities, which ultimately translate into an effect on economic growth.

[6]These include measures to strengthen and raise capital, reduce leverage, improve balance sheet liquidity and bring greater standardization and transparency to derivatives markets. They also include new rules around credit card availability and debit-interchange fees, along with heightened regulatory and judicial scrutiny of bank lending and other practices.

are among several recent papers examining the loan price effect of bank capital. Hubbard, Kuttner, and Palia (2002) use syndicated loans between 1987 and 1992. Mattes, Steffen, and Wahrenburg (2013) examine a sample of U.K. loans to both public and private firms between 1996 and 2005. Santos and Winton (2019) also focus on syndicate loans, but the sample period is from 1987 to 2007. Although post-crisis bank regulation has spurred fierce debate, there is no much research examining the loan price effect of regulation using samples during the post-crisis period.

Whether bank regulation causes an increase in loan spread is an empirical question. On one hand, regulatory rules aiming at providing higher safety margins lead to an increase in banks' operating costs[7], and banks may pass through the compliance costs to borrowers in consideration of short-term profitability. This argument is similar to Fama (1985) on who bears the reserve taxes on certificates of deposits. On the other hand, regulations induce banks to take less risk through more stringent criteria to screen borrowers so only safer ones remain in the bank loan market. In the latter case, we may not observe an increase in loan spreads once the borrower characteristics are controlled for. Conditional on that some firms are still able to obtain bank financing, it is empirically unknown whether bank loans are more expensive upon new regulatory rules. So the first hypothesis to test is whether bank regulation causes borrowers to pay a higher cost for bank credit.

The loan price effect, if any, may display heterogeneity. Motivated by the recent findings by Boubaker, Saffar, and Sassi (2018) that product market competition shapes debt choice of firms and competitive pressure from the product market leads firms to rely less on bank debt financing, I further examine the heterogeneity in loan price effects across borrowers facing high and low product market competition. Firms in a highly competitive environment usually rely less on bank loans. But if they still choose to borrow from banks, the contracting process may contain the exchange of private information that is not cost-efficient to be disclosed to the public capital market. Banks are aware of borrowers' competition pressure and the asymmetric information problem. So I hypothesis that banks will raise loan spreads for borrowers in highly competitive markets.

[7]Foreign banks disclose in their 2014 20-F filings that the rule change in 2014 leads to a significant increase in operating costs and adverse impact on their business operation.

The change in financing cost caused by bank regulation may have some real effect on borrowers. The rational predation theory as outlined by Fudenberg and Tirole (1986) argues financial constraints induce predatory behavior by rivals. Given that bank loan contracts have to be disclosed in firms' public filings and bank regulatory rules are also widely known, if bank regulations do alter the cost of borrowing, then the cost disadvantage faced by the affected borrowers is common knowledge known to their rivals. Expecting predation behaviors by competitors, affected firms may adjust their product market strategies. I test this hypothesis by analyzing the sales growth, profitability and investment decisions by affected borrowers. A natural question following the adjustment in product market strategy is whether firms can survive as independent firms. I test it by examining the probability of delisting of borrowers.

2.3 Institutional Background

Foreign Banking Organizations are key participants in the U.S. capital markets. According to Shared National Credits Reports, FBOs take 33% - 39% of the total SNC loan commitments in the period between 2008 and 2018, with a downward trend over time[8].

Historically regulation of foreign banks follows the so-called "National Treatment" [9] rule such that foreign banks operated their U.S. branches and subsidiaries mainly under rules set by their home country regulators. However, as it is made clear by the crisis, regulators at different jurisdiction may have opposing interests, making global coordinations during crisis time even harder. One piece of evidence is that during the financial crisis, parents of foreign banks failed to provide sufficient support for their U.S. branches and subsidiaries, leaving foreign banks to use the emergency

[8]More information about the Shared National Credits Reports could be found on the website of the Office of Comptroller of the Currency (OCC). According to OCC, "The SNC program was established in 1977 to provide an efficient and consistent review and classification of SNC, which includes any loan and or/formal loan commitment, and any asset, such as real estate, stocks, notes, bonds, and debentures taken as debts previously contracted, extended to borrowers by a federally supervised institution, its subsidiaries, and affiliates that aggregates to $20 million or more and is shared by three or more unaffiliated supervised institutions. Many of these large loan commitments are also shared with foreign banking organizations and nonbanks, including securitization pools, hedge funds, insurance companies, and pension funds." See https://www.occ.treas.gov/news-issuances/news-releases/2019/pub-snc-review-2018.pdf for the latest release

[9]https://www.theclearinghouse.org/banking-perspectives/2015/2015-q1-banking-perspectives/articles/national-treatment-of-foreign-banks provides a comprehensive discussion about the historical approaches.

facilities established by the Federal Reserve disproportionately[10].

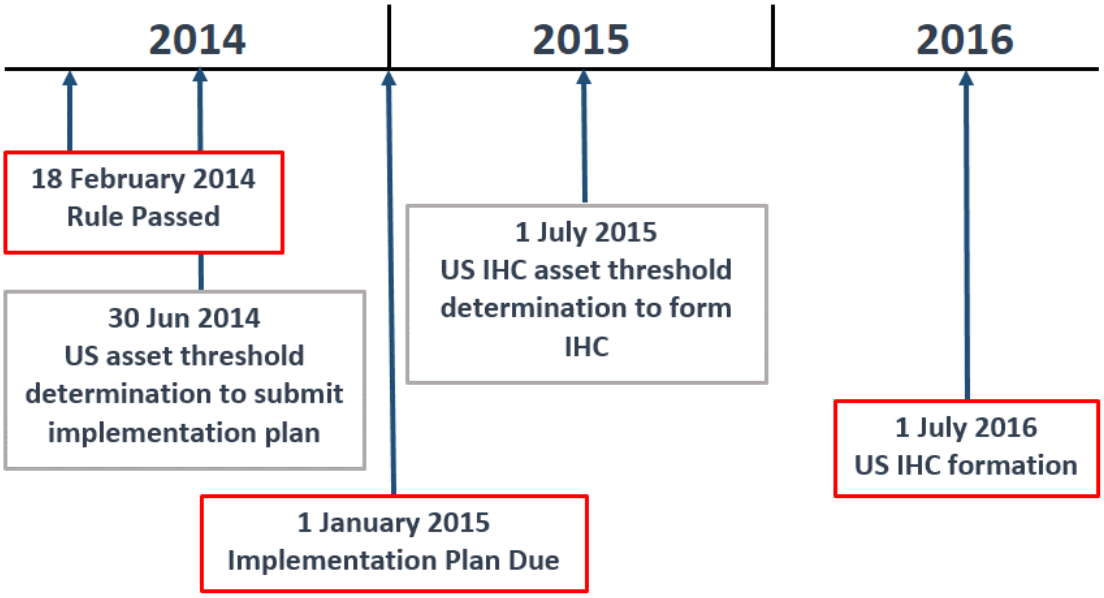
In response to these concerns, on December 14, 2012, the Federal Reserve Board proposed comprehensive rules for public comments to strengthen the oversight of the operations of FBOs. The proposal implements Sections 165 and 166 of the Dodd-Frank Wall Street Reform and Consumer Protection Act of 2010 by applying enhanced prudential standards to address the risks associated with the increased interconnectedness, complexity, and concentration of FBOs' U.S. operations. The period for public comments ended on March 31, 2013, and the final rule was approved on February 18, 2014.

Among all these compliance burdens brought about by the final rule, the requirement to establish an IHC for FBOs with total global consolidated assets of $50 billion or more and total U.S. non-branch assets of $50 billion or more generates the most opposition and is criticized by practitioners as being a "Dramatic Departure" to "National Treatment". According to the rule, the IHC must be a U.S. corporation formed under the law of the United States. The board of directors of the IHC must be subject to the fiduciary responsibilities equivalent to those of corporate directors. IHCs will be required to comply with the same risk-based capital and leverage requirements that are applicable to U.S. BHCs under Basel III. While the effective date for establishing the IHC is not until July 1, 2016, a potentially affected FBO is required to create an implementation plan and submit it to the Federal Reserve by January 1, 2015—which means a relatively short time period to make adjustment to FBO's U.S. operations. Figure 2.1 describe the timeline of implementation of the new rule.

Although in the same rule-making, certain of the enhanced prudential standards are also applicable for domestic U.S. bank holding companies with total consolidated assets of $50 billion or more, for large U.S. bank holding companies many of these enhanced prudential standards under the Dodd-Frank Act have previously been adopted or are in the process of being adopted under other rule-makings by the Federal Reserve. That being said, this new rule is an exogenous shock to FBOs, but not so much for domestic BHCs, so it provides a valid nature experiment setting to

[10]Felkerson (2011)provides a comprehensive investigation of Federal Reserves' bailout programs.

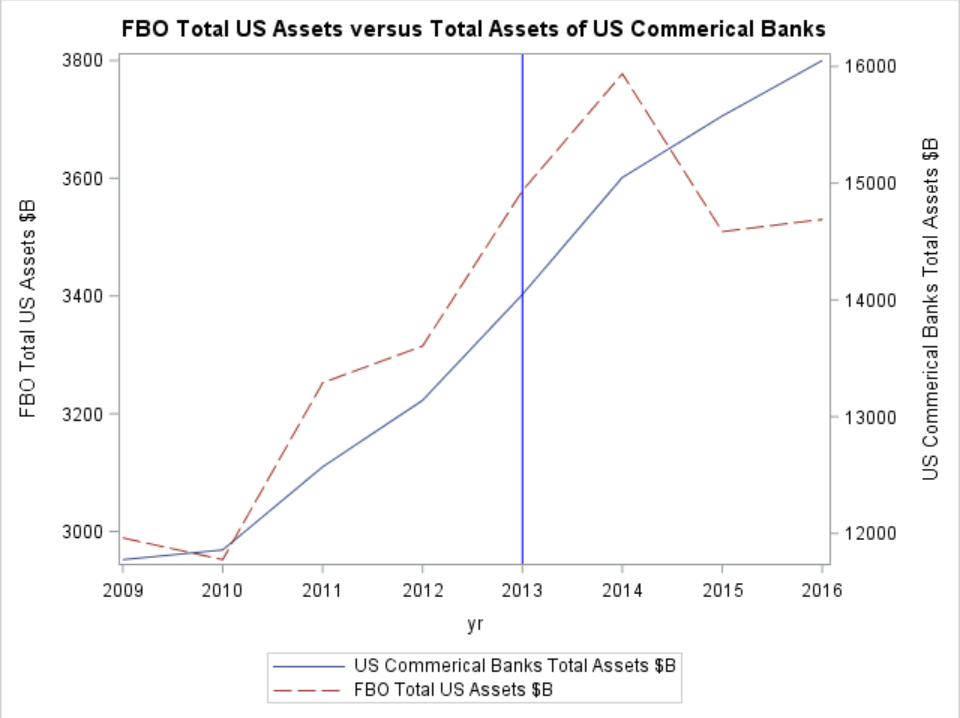
Figure 2.1: Timeline of Implementation of IHC requirement



study the causal effect of regulation on the real economy.

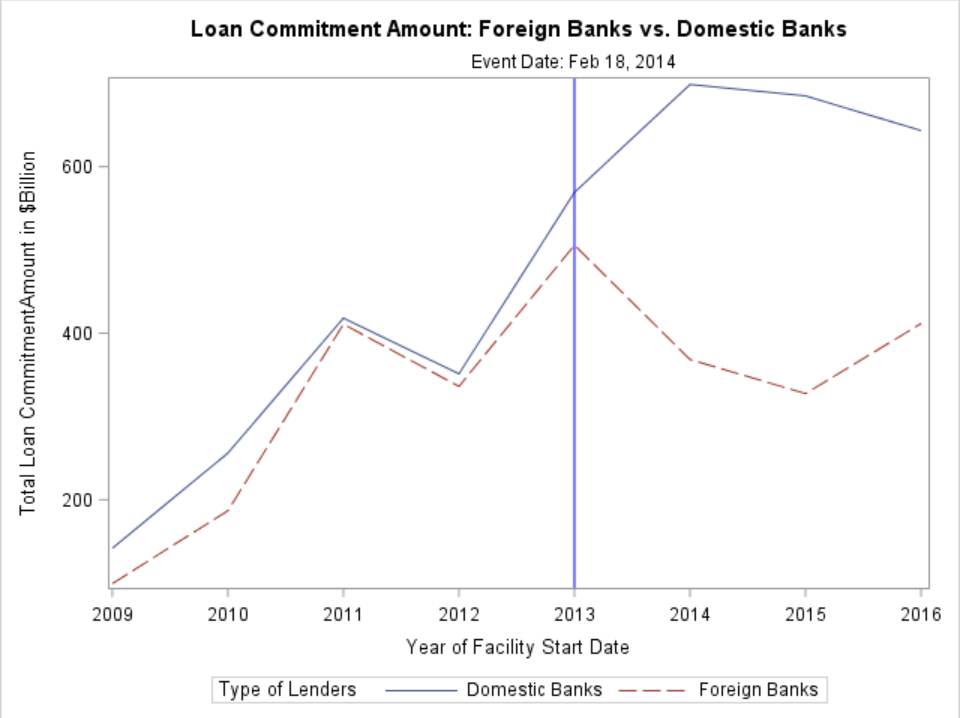
Using Structure and Share Data for U.S. Banking Offices of Foreign Entities provided by the Federal Reserve, I find FBOs dramatically shrink their assets in the U.S. from 2015, see Figure 2.2. While the shrinkage in their total U.S. assets is not observed until 2015, consistent with the time line of the IHC rule implementation, their adjustment in syndicated loan shares started in 2014, as shown in Figure 2.3.

Figure 2.2: Total U.S. Assets of FBO



Plot of FBOs' Total U.S. Assets and Total Assets of all US Commercial Banks by year. Data for US banks are from Federal Reserve Bank of St. Louis. FBOs' information is from the Structural and Share Data for U.S. Banking Offices of Foreign Entities from the Federal Reserve. The U.S. offices of foreign banking organizations consist of branches/agencies, representative offices, Edge Act corporations and subsidiaries.

Figure 2.3: Dealscan Loan Commitment Amount



The syndicated loan commitment amount over time lead by FBOs and domestic BHCs using data from LPC Dealscan.

2.4 Data

I start with all credit facilities in Thomson-Reuters' LPC Dealscan database originated between 1986 and Jan 2018. Because the focus of this study is the U.S. market, I keep only facilities whose country of syndication is USA, the currency is U.S. dollar, loan spread (Allindrawn) is not missing and the base rate is LIBOR[11]. I also drop financials and utilities (SIC 6000-6999 and 4900-4999). I then merge the trimmed dataset with Chava and Roberts (2008)'s link table to obtain borrowers' GVKEY. 68056 unique facilities between January 15, 1986 and August 11, 2017 remain. For these facilities, I focus on lead lenders that are identified based on leadarrangercredit in LENDERSHARES table in cases where lender are not the sole lender. In the case of sole

[11]I keep facilities whose base rate is LIBOR to make sure the loan spread is comparable. I also checked the loan pricing dataset and confirmed that allindrawn spread is the same regardless of base rate listed.

lender, the sole lender is designated as lead lender. This approach identifies lead lenders for 98% of all facilities. For the remaining 2% I use either agent credit, when appropriate, or manually ($< 0.2\%$) check loan allocation to determine lead lenders. For lender's information, Schwert (2018) provides a link table between the most active lenders[12] from DealScan with the identifiers of their respective bank holding companies (BHC) in Compustat. 33993 facilities by 5418 industrial borrowers between 1986 and early 2017 remain. 107 unique BHC are identified as lead arrangers for these facilities. Although the main analysis of this paper centers on the period between July 2009 and end of 2016, I do the mapping for all Dealscan loans because I need loans outside the main sample period for placebo tests. In the main sample period post-July 2009, there are 8612 unique facilities by 2096 unique borrowers. Forty-nine banks act as lead lenders for these facilities. There are thirty domestic BHCs and nineteen FBOs. Table 2.1 lists these lenders ranked by the number of facilities they lead.

Three groups of control variables are included in the regression analysis: facility, borrower and macro-economy. Details of definitions are listed in the Appendix A. Facility related variables are the loan spread (allindrawn), loan size, maturity, a dummy for whether the loan is secured, and a dummy for the existence of financial covenants. A loan type dummy (term loan or revolver or other) is also created to control for loan type fixed effects. I use data from Compustat and CRSP to construct borrower control variables following Chava, Huang, and Johnson (2017). The variables included are firm size, Market-to-Book, leverage, profitability, tangibility, modified Z-score, KZ index, distance-to-default, cash flow volatility, a dummy indicating whether a borrower has a lending relationship with a lender in past 5 years, a dummy for the existence of a bond issuer rating, and a dummy indicating whether a firm has bonds outstanding. All values of these variables are taken from the fiscal year just prior to loan origination. In addition to loan and borrower characteristics, I also include three macro variables to control for the overall economic conditions: effective federal funds rate, credit spread and term spread. Descriptive statistics are in Table 2.2. Compared to the average firm in Compustat, firms in my sample are slightly larger and

[12]The most active lenders refer to those lead arrangers of at least 50 loans or at least $10 billion in volume.

Table 2.1: List of 49 lenders in the main sample period

This table lists the names of bank holding companies that actively act as lead arrangers on the syndicated loan market between July 2009 to December 2016. 19 of them are FBOs and 30 are non-FBOs. The names shown are the same as in Compustata and are ranked based on the number of facilities lead.

NON-FBO	FBO
BANK OF AMERICA CORP	BARCLAYS PLC
JPMORGAN CHASE & CO	DEUTSCHE BANK AG
WELLS FARGO & CO	CREDIT SUISSE GROUP
CITIGROUP INC	ROYAL BANK OF CANADA
SUNTRUST BANKS INC	ROYAL BANK OF SCOTLAND GROUP
MORGAN STANLEY	MITSUBISHI UFJ FINANCIAL GRP
GOLDMAN SACHS GROUP INC	HSBC HLDGS PLC
U S BANCORP	BNP PARIBAS
PNC FINANCIAL SVCS GROUP INC	BANK OF MONTREAL
KEYCORP	UBS GROUP AG
FIFTH THIRD BANCORP	MIZUHO FINANCIAL GROUP INC
JEFFERIES GROUP LLC	BANK OF NOVA SCOTIA
REGIONS FINANCIAL CORP	TORONTO DOMINION BANK
CITIZENS FINANCIAL GROUP INC	SUMITOMO MITSUI FINANCIAL GR
SVB FINANCIAL GROUP	BBVA
COMERICA INC	SOCIETE GENERALE GROUP
BB&T CORP	BANCO SANTANDER SA
M & T BANK CORP	CANADIAN IMPERIAL BANK
BANK OF NEW YORK MELLON CORP	DBS GROUP HOLDINGS LTD
CIT GROUP INC	
HUNTINGTON BANCSHARES	
WEBSTER FINANCIAL CORP	
CITY NATIONAL CORP	
ALLY FINANCIAL INC	
ZIONS BANCORPORATION NA	
ASSOCIATED BANC-CORP	
MARSHALL & ILSLEY CORP	
NORTHERN TRUST CORP	
PEOPLE'S UNITED FINL INC	
SYNOVUS FINANCIAL CORP	

have higher market-to-book ratios. The mean leverage in my sample is around 32%, in line with that for typical public firms with bank debt outstanding. Firms in my sample are also slightly higher in profitability and tangibility, consistent with bank screening of borrowers. Distance-to-default is also larger, meaning that borrowers in my sample are, on average, healthier at loan origination than a typical public firm. Lender assets are also obtained from Compustat. One facility might have more than one lead arrangers and I rank them based on asset value and keep the largest lenders.[13] In the main sample, 32% facilities have an FBO as the largest lead arrangers, which is in line with the SNC reports.

2.5 Research Design and Empirical Results

2.5.1 Research Design

To test whether bank regulation affects the price of bank credit that borrowers pay, ideally I need an exogenous shock that affects a subgroup of lenders, but not others, while at the same time having no direct effect on firms that borrow from lenders in both groups. To this end, I exploit the passage of new rules on FBO regulation. This rule change, especially the IHC requirement, comes as a surprise as it differs from the traditional view of "National Treatment". It imposes substantial compliance costs onto FBOs, but does not affect domestic BHCs to the same degree. The IHC requirement requires dramatic structural change within the organization, re-design of IT system in order to collect, analyze and report information to regulators and human resources, etc. For FBOs that continue to lend, they face higher compliance cost than ever before and have incentives to pass the cost to borrowers to maintain their profitability. To test the hypothesis that bank regulation causally affects loan spreads, I run the below baseline regression:

$$\begin{aligned}\text{Loan Spread}_{i,j,t} =& \beta_0 + \beta_1 \text{FBO}_j \times \text{Post}_t + \beta_2 \text{Post}_t + \beta_3 \text{Loan Variables}_{i,j,t} \\ &+ \beta_4 \text{Borrower Variables}_{i,t} + \beta_5 \text{Macro Variables}_t + \alpha_j + \varepsilon_i\end{aligned} \tag{2.1}$$

[13] In a subset of loans where each lender's share in a loan is available, I confirm that in 80% cases the largest lender is also the one who takes the largest share of a facility. Because the shares taken by each lead arranger is not readily available for every facility in Dealscan, focusing on the largest lead arranger is the best way, though not perfect, to identify the most important players in a loan contract.

Table 2.2: Summary Statistics

This table reports summary statistics for lender, loan, macro and borrower characteristics during the sample period from July 01, 2009 to the end of 2016. Borrower and lender characteristics are measured at annual frequency right prior to loan initiation. Macro variables are measured as of loan origination date. Definitions of variables are in the Appendix A.

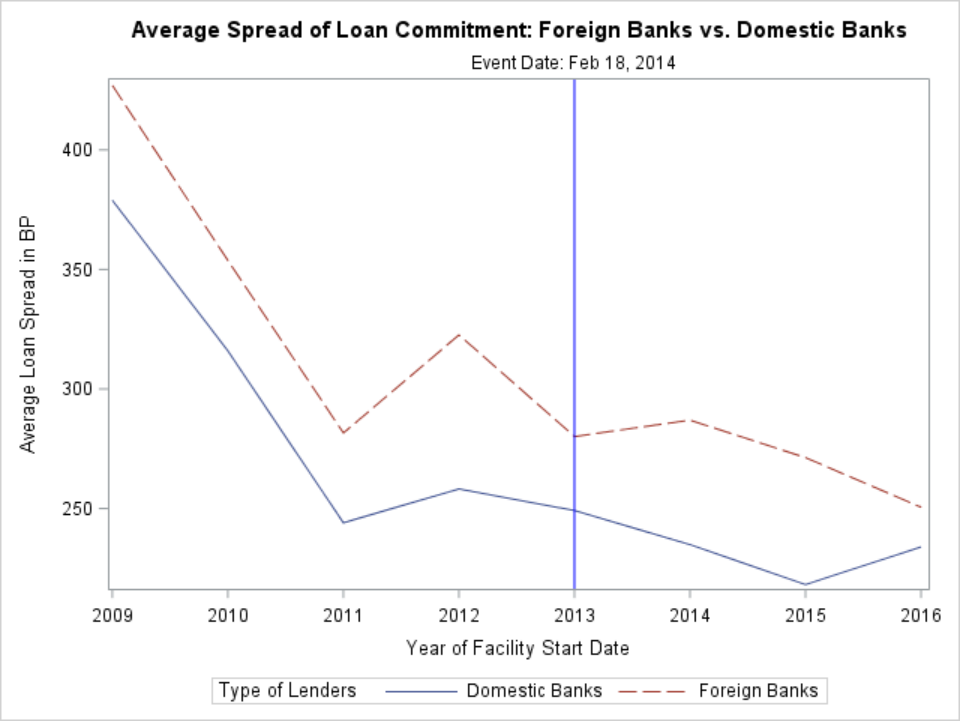
	N	Mean	SD	Min	P5	P25	P50	P75	P95	Max
Lender Variable:										
FBO	8,612	0.33	0.47	0	0	0	0	1	1	1
Log of Lender Assets	8,612	14.42	0.71	9.61	12.78	14.57	14.64	14.72	14.83	14.98
Facility Characteristics:										
AllInDrawn	8,612	248.14	145.56	10	100	150	200	300	525	1275
Log of Loan Maturity	8,559	3.95	0.46	0.69	2.57	3.89	4.11	4.11	4.44	5.20
Log of Loan Size	8,612	19.49	1.33	11.51	17.22	18.64	19.52	20.44	21.49	24.62
Dummy for secured loan	8,612	0.54	0.50	0	0	0	1	1	1	1
Dummy for financial covenant	8,612	0.50	0.50	0	0	0	1	1	1	1
Macro Variable:										
Federal Fund Rates	8,612	15.68	10.64	5	7	9	13	16	40	66
Credit Spread	8,612	99.95	23.95	53	63	83	96	118	142	177
Term Spread	8,612	131.82	47.19	46	60	90	138	161	219	266
Borrower Characteristics:										
Log of Borrower Assets	8,612	7.81	1.56	3.20	5.40	6.71	7.74	8.83	10.58	12.04
Market to Book	8,612	3.11	2.80	0.32	0.82	1.46	2.27	3.56	8.53	15.75
Leverage	8,612	32.17	22.45	0.00	0.00	15.89	29.14	45.36	75.15	121.99
Profitability	8,607	13.29	7.52	-10.65	3.01	9.08	12.58	16.73	26.46	41.31
Tangibility	8,612	28.26	24.70	0.53	2.62	8.77	19.23	42.52	82.12	90.17
Modified Z-score	8,316	1.65	1.22	-1.82	-0.07	0.85	1.57	2.39	3.85	5.26
KZ-index	8,366	-5.16	15.28	-101.94	-27.02	-5.07	-0.71	1.47	3.45	6.65
Cash Flow Volatility	8,214	30.78	769.47	0.00	0.02	0.07	0.15	0.45	16.74	57556.46
Distance to default	7,871	6.87	4.81	-2.02	0.50	3.47	6.09	9.40	16.09	21.96
Dummy for lending relationship	8,612	0.61	0.49	0	0	0	1	1	1	1
Dummy for SP rating	8,612	0.64	0.48	0	0	0	1	1	1	1
Dummy for bond outstanding	8,612	0.47	0.50	0	0	0	0	1	1	1

where $LoanSpread_{i,j,t}$ is the loan spread borrower i pays to lender j as of loan start time t. It comes directly from Dealscan data. FBO_j is a dummy variable indicating whether the lead lender is FBO. $Post_t$ takes the value of 1 if the facility start date is after February 18, 2014 and 0 if facilities are originated between July 1, 2009 and February 18, 2014. α_j is lender fixed effect, so I omit FBO_j because they are perfectly collinear with the lender fixed effects. Borrowers variables are computed as of the fiscal year end prior to loan origination. A positive coefficient, β_1, on the interaction between FBO_j and $Post_t$ would indicate that the new regulation causes treated banks to increase loan spreads during the post-period relative to untreated banks.

2.5.2 Parallel Trend Assumption

The validity of a difference-in-differences analysis relies on the assumption of parallel trends. That is, without the regulatory change, the loan spreads across these two groups of lenders would share a common trend. I validate the parallel trend assumption in three ways. I discuss the first two approaches here and leave the last one after I present the main results. First, I visualize the average loan spreads overtime for the treated and control lenders from 2000 to 2016. Figure 2.4 presents the results, and clearly there is a similar pattern in loan spreads, except during the European debt crisis period.

Figure 2.4: Average Loan Spread Over Time



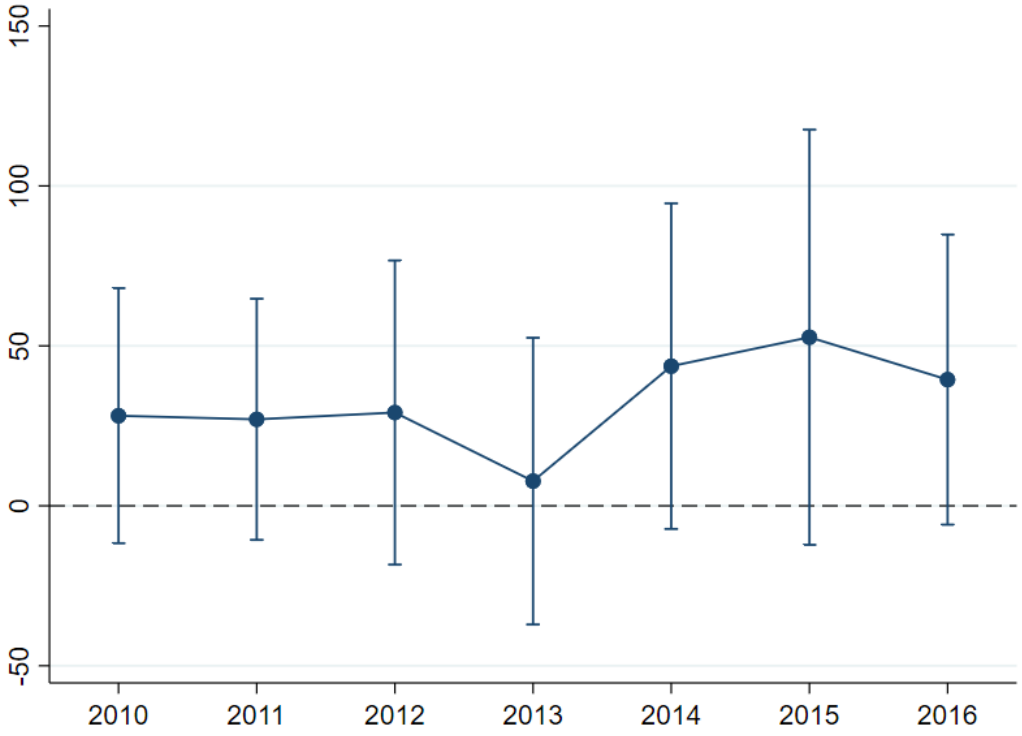
Average loan spread by FBOs and domestic BHCs using loan pricing data from Dealscan.

However, the raw plot of loan spreads does not take into account of the borrower and loan characteristics. A more reliable way to validate the parallel trend assumption is to conduct a generalized regression analysis, following Autor (2003), with a year dummy interacted with the FBO dummy. The model specification is given below:

$$\begin{aligned} \text{LoanSpread}_{i,j,t} =& \beta_0 + \sum_{t=2010}^{2016} d_t \times FBO_j \times Year_t \\ &+ Controls + \alpha_j + \gamma_t + \varepsilon_i \end{aligned} \tag{2.2}$$

Control variables are the same as in Equation 2.1. α_j is the lender fixed effect, and γ_t captures the year fixed effect. To estimate the model, I use all facilities originated between 2009 and 2016. If the loan spreads between these two groups of lenders share a common trend during the pre-period, all d_t, where $t \in (2010, 2013)$ are expected to be insignificant. The estimates of d_t and

Figure 2.5: Loan Spread Parallel Trend



This figure plots the estimates specified in equation 2.2 as a formal test of parallel trend of loan spread, using facilities starting in 2009 through 2016. The dots are point estimates and lines are 95% confidence intervals.

corresponding 95% confidence interval are presented in Figure 2.5. None of these coefficients are significant, indicating that loan spreads across the two groups share a common trend before the event.

2.5.3 Loan Price Effect

Now I turn to analyze the price effects of bank regulation by running regressions specified in Equation3.1. Table 2.3 display the results from the difference-in-differences specifications using different definitions for the pre-period. In column (1) the pre-period is from July 2012 to February 18, 2014. The reason to start from July 2012 is to avoid possible contamination by European Debt Crisis. In column (2), I start the pre-period from July 2009, which is the end of the recession, as defined by the NBER. I do not move the pre-period backward further to avoid contamination that

might be caused by the financial crisis. Column (3) also uses loans originated after the financial crisis but excluding the European Debt Crisis period. I follow Ivashina, Scharfstein, and Stein (2015) and define this crisis to begin in the second half of 2011 and end after the first half of 2012[14]. In column (4) and (5), I use two-year and one-year time window to define my sample period. The coefficients of interaction term are my main interest and are positive and significant in all columns, indicating on average borrowers pay a significantly higher price if they borrow from FBOs in the post-period, relative to those who borrow from non-FBOs, even after controlling all observable borrower, loan, and macro characteristics. The price difference ranges from 16 to 34 basis points, which corresponds to an average increase of $3.6 to $7.6 million per borrower[15]. For a borrower with median income, which is $173 million, during the post period, the increased interest expenses amount to 2% - 4% of their EBIT, which is not negligible.

As a third approach to validate the parallel trend assumption, especially the potential contamination by the European Debt Crisis, I run two placebo tests, using two-year and one-year sample period around the crisis. The parallel trend assumption implies that the coefficients on the interaction term should be statistically indistinguishable from zero, if my main findings are not driven by the European Debt Crisis. Indeed, in both specifications, the coefficients of interaction terms are not significant, as shown in column (1) and (2) of Table 2.4, providing consistent evidence for the parallel trend assumption.

Another concern of the increased loan spreads is the possibility that FBOs choose to lend to riskier firms during the post period so they charge higher prices to compensate themselves for risk-taking. To address this concern, I compare loan and borrower characteristics during the pre-period and post-period across the two types of lenders and then test whether the changes from pre-period to post-period are significant. Table 2.5 shows the results. Although there are differences in loan and firm characteristics between FBOs borrowers and domestic BHC borrowers, the differences do not change much across time, except in the dimension of leverage, cash flow volatility, and

[14]See Figure II in Ivashina, Scharfstein, and Stein (2015)

[15]This calculation is based on the total number of borrowers in the post period and total amount of loan commitment provided by FBOs during the post period.

Table 2.3: Loan Spread Regression

This table presents the results from the difference-in-differences loan spread regression, as specified in equation 3.1. The dependent variable is loan spread (Allindrawn). Column (1) - (5) use different pre-periods with specific starting date, as indicated. Definitions of control variables are in the Appendix A. Standard errors are two-way clustered by lenders and borrowers. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1)	(2)	(3)	(4)	(5)
Pre-period start from:	20120701	20090701	20090701* (exl EU debt crisis)	2-Yr Window	1-Yr Window
Post * FBO	20.70***	16.31**	20.54***	19.77***	33.92**
	(3.70)	(2.33)	(2.97)	(3.04)	(2.58)
Post Dummy	3.18	-15.98***	-17.34***	3.00	-10.05
	(0.53)	(-3.55)	(-4.23)	(0.43)	(-1.49)
Log of Loan Size	-18.50***	-19.32***	-19.53***	-18.69***	-18.97***
	(-4.04)	(-5.10)	(-5.07)	(-4.00)	(-4.63)
Log of Loan Maturity	4.09	2.56	2.35	3.43	11.98
	(0.71)	(0.46)	(0.48)	(0.50)	(1.45)
Log of Borrower Assets	-4.48**	-4.50**	-3.47**	-2.84	-2.18
	(-2.51)	(-2.71)	(-2.09)	(-1.32)	(-0.82)
Market to Book	1.24	0.86	0.76	1.80	0.48
	(0.92)	(0.92)	(0.79)	(1.17)	(0.24)
Leverage	0.24	0.21	0.17	0.35	0.36
	(0.99)	(1.20)	(0.91)	(1.69)	(1.63)
Profitability	-1.37***	-1.02***	-1.12***	-1.42***	-1.57**
	(-4.51)	(-4.50)	(-5.61)	(-3.16)	(-2.29)
Tangibility	0.14	0.08	0.08	0.14	0.07
	(1.04)	(0.83)	(0.79)	(0.81)	(0.48)
Cash Flow Volatility	0.00	0.00	0.00	0.00	-0.00**
	(0.87)	(0.92)	(0.73)	(1.35)	(-2.55)
Modified Z-score	-6.76***	-8.89***	-7.14***	-6.73***	-6.73***
	(-4.70)	(-5.45)	(-4.28)	(-4.14)	(-2.97)
KZ-index	-0.04	0.04	0.03	0.18	0.15
	(-0.32)	(0.42)	(0.32)	(1.48)	(1.07)
Distance to default	-3.95***	-4.41***	-4.68***	-3.39***	-3.24**
	(-6.60)	(-11.08)	(-9.71)	(-3.88)	(-2.43)
Credit Spread	0.10	0.28**	0.30**	0.21*	-0.08
	(1.18)	(2.10)	(2.40)	(1.79)	(-0.32)
Term Spread	-0.12	0.27***	0.26***	-0.14	0.08
	(-1.31)	(6.49)	(6.30)	(-1.17)	(0.70)
Federal fund rate	-0.90	1.71**	1.45*	-3.14	-11.83**
	(-1.32)	(2.21)	(1.84)	(-1.53)	(-2.32)
Dummy for Secured	Yes	Yes	Yes	Yes	Yes
Dummy for Financial covenants	Yes	Yes	Yes	Yes	Yes
Dummy for SP rating and bond	Yes	Yes	Yes	Yes	Yes
Dummy for lending relationship	Yes	Yes	Yes	Yes	Yes
Lender Fixed Effect	Yes	Yes	Yes	Yes	Yes
LoanType Fixed Effect	Yes	Yes	Yes	Yes	Yes
N	4770	7213	6212	4237	2260
Adj R_square	0.456	0.453	0.448	0.465	0.472

Table 2.4: Placebo Test for Loan Spread Regression

This table presents the results of placebo tests. Model specifications are the same as in equation 3.1. The dependent variable is loan spread (Allindrawn). Column (1) - (2) use all facilities originated within the two-year and one year window around the European Debt Crisis. Column (3) and (4) only use facilities by borrowers in highly competitive market, measured by product market fluidity. Definitions of control variables are in the Appendix A. Standard errors are two-way clustered by lenders and borrowers. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1) Full Sample 2-Yr Window	(2) Full Sample 1-Yr Window	(3) High Fluidity Sample 2-Yr Window	(4) High Fluidity Sample 1-Yr Window
Post * FBO	3.50	3.47	-10.42	-10.75
	(0.30)	(0.27)	(-0.71)	(-0.72)
Post Dummy	-11.43**	-10.54	-18.33	-8.35
	(-2.23)	(-1.16)	(-1.51)	(-0.56)
Log of Loan Size	-18.09***	-16.16***	-19.14***	-18.50***
	(-5.72)	(-4.95)	(-4.51)	(-4.84)
Log of Loan Maturity	-2.10	-3.13	-9.60	-12.73
	(-0.29)	(-0.27)	(-0.96)	(-0.76)
Log of Borrower Assets	-5.38*	-9.81**	-11.66**	-20.48***
	(-2.01)	(-2.22)	(-2.36)	(-2.78)
Market to Book	1.08	0.76	-1.90*	-1.06
	(1.51)	(1.10)	(-1.84)	(-0.95)
Leverage	0.35*	0.39*	-0.25	-0.49
	(1.99)	(2.01)	(-0.98)	(-1.50)
Profitability	-0.84**	-0.90*	-0.62	-0.68
	(-2.07)	(-1.74)	(-1.02)	(-1.13)
Tangibility	-0.04	0.03	0.05	0.13
	(-0.29)	(0.33)	(0.28)	(0.68)
Cash Flow Volatility	0.02	0.02*	0.03	0.03*
	(1.34)	(1.71)	(1.59)	(1.78)
Modified Z-score	-11.40***	-12.21***	-13.72***	-14.42*
	(-5.64)	(-4.27)	(-3.19)	(-1.90)
KZ-index	0.32**	0.19	-0.11	-0.07
	(2.52)	(0.83)	(-0.50)	(-0.67)
Distance to default	-4.96***	-3.73***	-6.12***	-6.00***
	(-8.41)	(-4.27)	(-5.06)	(-3.22)
Credit Spread	0.57***	0.21	0.70**	0.54
	(3.45)	(0.67)	(2.67)	(1.08)
Term Spread	0.23***	-0.02	0.13	0.03
	(4.84)	(-0.30)	(1.39)	(0.18)
Federal fund rate	4.95*	-4.34	1.90	-7.26
	(1.94)	(-1.42)	(0.37)	(-1.05)
Dummy for Secured	Yes	Yes	Yes	Yes
Dummy for Financial covenants	Yes	Yes	Yes	Yes
Dummy for SP rating and bond	Yes	Yes	Yes	Yes
Dummy for lending relationship	Yes	Yes	Yes	Yes
Lender Fixed Effect	Yes	Yes	Yes	Yes
LoanType Fixed Effect	Yes	Yes	Yes	Yes
N	3671	2009	1666	927
Adj R_square	0.461	0.470	0.465	0.477

distance to default. Actually, the sign of leverage and cash flow volatility is against the story of FBOs taking more risks. Distance-to-default is not a concern as long as I control for it in the loan spread regression. Overall, the higher loan spreads charged by FBOs during the post period is not driven by their risk-taking behaviors.

Table 2.5: Comparison: Loan and Borrower Characteristics

This table presents comparison of loan and borrower characteristics during the pre-period and post-period across two types of lenders, and the difference-in-difference from pre-period to post-period. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	Pre-period			**Post-period**			**Post - Pre**	
	Non-FBO	FBO	Dif	Non-FBO	FBO	Dif	Diff-in-diff	t-stat
Loan Characteristics:								
Log of Loan Maturity	3.91	3.99	0.08	3.96	4.00	0.04	-0.04	(-1.47)
Log of Loan Size	19.16	19.70	0.53	19.61	19.88	0.27	-0.27	(-1.41)
Dummy for secured loan	0.52	0.62	0.10	0.50	0.54	0.04	-0.06	(-1.56)
Dummy for financial covenant	0.53	0.50	-0.04	0.50	0.42	-0.09	-0.05	(-0.98)
Borrower Characteristics:								
Log of Borrower Assets	7.42	8.12	0.70	7.88	8.39	0.51	-0.19	(-0.91)
Market to Book	3.12	2.85	-0.26	3.26	3.13	-0.13	0.14	(0.90)
Leverage	29.04	36.28	7.24	31.91	35.68	3.78	-3.47**	(-2.10)
Profitability	13.73	13.14	-0.59	13.14	12.53	-0.60	-0.01	(-0.02)
Tangibility	29.18	29.41	0.23	26.77	27.01	0.24	0.01	(0.01)
Modified Z-score	1.81	1.39	-0.42	1.72	1.43	-0.29	0.13	(1.36)
KZ-index	-3.95	-5.35	-1.40	-6.21	-6.16	0.05	1.45*	(1.80)
Cash Flow Volatility	23.27	29.71	6.44	51.44	6.67	-44.76	-51.21**	(-2.36)
Distance to default	6.05	5.82	-0.23	8.43	7.50	-0.93	-0.70**	(-2.09)
Dummy for lending relationship	0.63	0.41	-0.22	0.73	0.58	-0.16	0.07	(1.35)
Dummy for SP rating	0.56	0.75	0.19	0.61	0.74	0.12	-0.07	(-1.17)
Dummy for bond outstanding	0.45	0.52	0.07	0.45	0.51	0.06	-0.01	(-0.17)

2.5.4 Heterogeneity in Loan Price Effect

To understand which set of borrowers are affected the most, I draw on Boubaker, Saffar, and Sassi (2018) who relates product market competition to firms' debt financing decisions. I hypothesize that treated banks will tend to increase spreads on borrowers subject to more product market competition. The rationale is that if a firm is operating in a highly competitive environment but

still choose to borrow from banks, then the bank loans might contain some critical information that is not feasible or too costly to disclose to the public capital market. Knowing that some borrowers may not easily switch to debt market or alternative lenders due to information consideration related to product market competition, a bank facing higher compliance burden will pass costs to this subset of borrowers. So I sort borrowers based on the level of their product market competition. To measure the intensity of competition, I use product market fluidity developed by Hoberg, Phillips, and Prabhala (2014). I first calculate as a reference the median value using all firm-year observations during 2009-2016 from Hoberg's database and then categorize borrowers in the main sample period into high or low group depending on whether their fluidity is above or below the referred median value. Separate regressions of equation 3.1 are run using high and low competition samples and results are shown in Table 2.6. The results in column (1) and (2) indicate that only borrowers facing higher product market threats are paying higher loan spreads. There is no such effect for borrowers with low product market fluidity, as shown in column (3) and (4). Similar to the placebo test for the full sample, I run a placebo test for the high fluidity subsample around the European Debt Crisis and found no difference across borrower of BHCs and FBOs during that period, as shown in column (3) and (4) of Table 2.4.

2.6 Product Market Competition

2.6.1 Borrowers and Rivals Strategy

In a perfect market, shrinking of bank lending and an increase in the cost of bank credit should not affect the investment and performance of borrowers as substitutionary funding is always available from other sections of the capital market. However, with market friction, such as asymmetric information, firm financing is segmented across equity, bank debt and bonds (i.e. Bolton and Freixas (2000)). In this sense, bank loans are special and the inability of firms to switch to alternative financing sources without incurring extra costs will translate into changes in investment decisions, especially when product market competition pressure is also considered. Since loan agreements have to be disclosed in firms' public filings and bank regulatory rules are widely

Table 2.6: Loan Spread Regression: Sort by Product Market Competition

This table presents the results from the difference-in-differences loan spread regression. Borrowers are sorted into two groups based on whether their product market fluidity is above or below the median value. The dependent variable is loan spread (Allindrawn). The starting date of the pre-period is as indicated. Definitions of control variables are in the Appendix A. Standard errors are two-way clustered by lenders and borrowers. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	High Fluidity	High Fluidity	Low Fluidity	Low Fluidity
	(1)	(2)	(3)	(4)
Pre-period start from:	20120701	20090701	20120701	20090701
Post * FBO	38.27***	26.75**	6.87	7.05
	(3.07)	(2.24)	(0.85)	(0.87)
Post Dummy	11.19	-4.54	-1.15	-24.23***
	(0.62)	(-0.63)	(-0.23)	(-5.71)
Log of Loan Size	-21.12***	-21.22***	-16.78***	-18.44***
	(-4.28)	(-5.36)	(-3.28)	(-4.33)
Log of Loan Maturity	2.35	-0.98	5.63	4.58
	(0.27)	(-0.14)	(0.64)	(0.57)
Log of Borrower Assets	-9.99**	-11.66***	-1.09	0.06
	(-2.35)	(-3.96)	(-0.27)	(0.02)
Market to Book	0.64	-0.48	1.59	1.38
	(0.33)	(-0.42)	(1.33)	(1.41)
Leverage	0.01	-0.28	0.41	0.63***
	(0.04)	(-1.27)	(1.55)	(3.05)
Profitability	-1.42**	-0.99***	-1.45**	-1.12**
	(-2.10)	(-2.74)	(-2.31)	(-2.33)
Tangibility	0.08	0.02	-0.01	-0.01
	(0.46)	(0.13)	(-0.07)	(-0.06)
Cash Flow Volatility	0.02***	0.02*	-0.00	-0.00
	(3.14)	(1.88)	(-0.68)	(-0.92)
Modified Z-score	-7.82**	-11.29***	-4.37**	-5.88***
	(-2.09)	(-3.31)	(-2.06)	(-3.13)
KZ-index	0.20	0.09	-0.08	0.06
	(0.79)	(0.49)	(-0.61)	(0.46)
Distance to default	-5.90***	-6.50***	-2.81***	-3.25***
	(-6.97)	(-7.61)	(-3.47)	(-6.98)
Credit Spread	-0.09	0.22	0.15	0.29**
	(-0.70)	(1.49)	(1.08)	(2.27)
Term Spread	-0.15	0.21***	-0.11	0.34***
	(-0.58)	(3.63)	(-1.62)	(8.11)
Federal fund rate	-1.99	0.14	-0.61	2.92***
	(-1.66)	(0.16)	(-0.55)	(3.08)
Dummy for Secured	Yes	Yes	Yes	Yes
Dummy for Financial covenants	Yes	Yes	Yes	Yes
Dummy for SP rating and bond	Yes	Yes	Yes	Yes
Dummy for lending relationship	Yes	Yes	Yes	Yes
Lender Fixed Effect	Yes	Yes	Yes	Yes
LoanType Fixed Effect	Yes	Yes	Yes	Yes
N	1929	3089	2933	4346
Adj R_square	0.485	0.472	0.440	0.451

known, affected borrowers' cost disadvantage is common knowledge that is also observable to rival firms. Knowing that their cost disadvantage can induce predation behavior by rivals, affected borrowers will alter their product market strategies to deter predation. In this section, I show the dynamics of borrower performance before and post the regulatory shock. Specifically, I focus on the subset of borrowers who borrow from either FBOs or domestic BHCs during the pre-period and the facility end date is after the event date so they need to renegotiate new debt contracts during the post-period when financing from FBOs becomes more difficult and expensive.

A key step before examining borrowers' strategy is to look for changes in behaviors of rival firms. When affected firms are hit by higher than before borrowing costs, rival firms may cut their price in the hope to gain more market share. Alternatively, they may increase their advertising expenses or SG&A expenses to boost sales. Or they may invest more in R&D or Capex to make products that can better differentiate themselves. To show the predation threat, I run the following regression to examine rival firms' strategy changes from pre to post period. Rival firms are defined using TNIC industry mapping developed by Hoberg and Phillips (2016).

$$\begin{aligned} \text{Y}_{i,j,t} =& \beta_0 + \beta_1 \text{FBO}_j \times \text{Post}_t + \beta_2 \text{Post}_t + \beta_3 \text{FBO}_j \\ &+ \beta_4 \text{Firm Variables}_{i,t} + \alpha_i + \varepsilon_i \end{aligned} \tag{2.3}$$

Where $\text{Y}_{i,j,t}$ are gross margin, SG&A expenses, Advertising expenses, Capex, and R&D. I control for firm size, leverage and market-to-book. i indexes rival firms and j index borrowers. Table 2.7 shows the changes in rivals strategy. It is clear that rival firms of FBOs borrowers lower their gross margin more than rival firms of BHCs borrowers do during the post period. The negative and significant β_1 could be viewed as preliminary evidence of rival firms' price cut strategy. There is no difference in SG&A expenses, advertising, Capex or R&D.

After showing the predation threat from rival firms, I turn to examine whether there is any change in borrowers' sales growth to see if their market shares are affected. Sales growth is measured on a quarter-over-quarter basis and adjusted by value-weighted TNIC industry growth rate so it is a relative measure benchmarked by rivals. To calculate the value-weighted industry aver-

Table 2.7: Rival Firms Predation Threat

This table presents the results from regression in Equation 2.3 using different dependent variables. Firm fixed effects are included and standard errors are clustered by firm and year. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

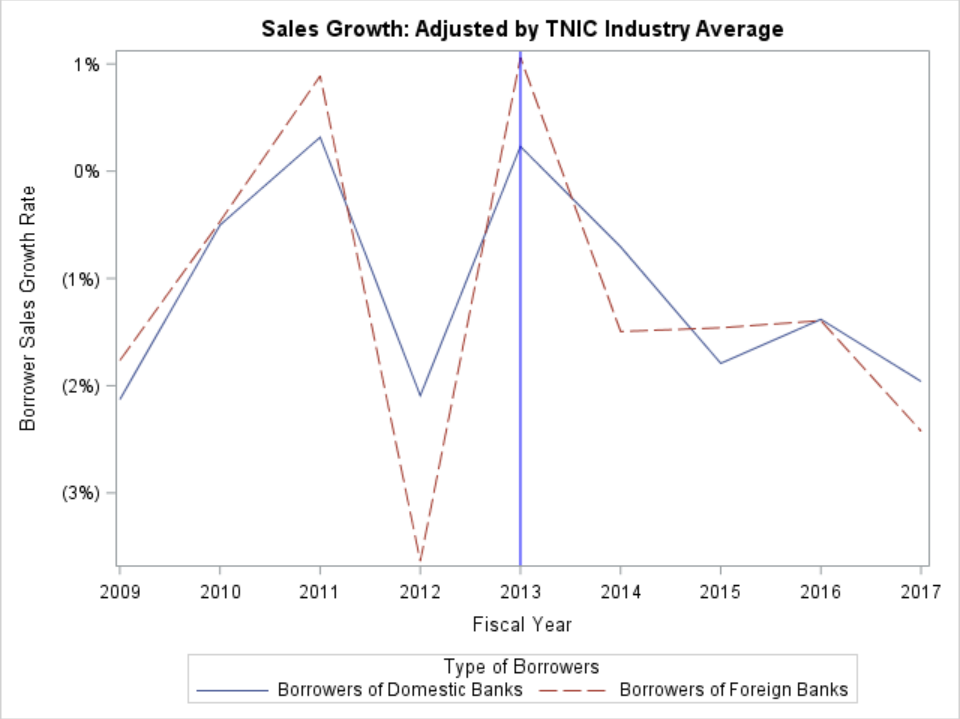
	(1) Gross Margin	(2) SG&A	(3) Advertising	(4) Capex	(5) R&D
β_1: Diff-in-Diff Coef.	-0.007***	-0.001	-0.000	-0.002	-0.009
	(-3.47)	(-1.02)	(-1.10)	(-1.21)	(-1.37)

age, for each borrower I calculate the sales growth of its rivals identified by Hoberg and Phillips (2010) and then weight rivals' sales growth by their market share. Then the industry weighted average is subtracted from the borrower's sales growth to arrive at the industry adjusted value. Figure 2.6 plots the industry-adjusted quarterly sales growth of these two types of borrowers between 2009 and 2017[16]. There is no significant difference in both pre and post period (also see column (3) in Table 2.8). Next, I turn to examine borrowers' gross margin, which is defined as (Sales-COGS)/Sales and adjusted by value-weighted TNIC industry ratios. Figure 2.7 plots the quarterly gross margin. Although borrowers of these two type of borrowers share a similar trend in the pre-period, gross margins of FBO borrowers deteriorate more than that of non-FBO borrowers during the post period. The difference-in-difference is -0.01 (t-stat=-2.25). The trend in sales growth and gross margin suggests that while FBOs borrowers manage to maintain their sales growth, their gross margin suffers, indicating they cut prices to protect their market share to fight potential predation behaviors.

Next, I examine whether the pricing strategy affects the operating profitability. I define operating margin as OIBDP/Sales and also adjust the ratios by value-weighted TNIC industry ratios. As

[16]I end the calculation in 2017 because TNIC data is not yet available after 2017

Figure 2.6: Pre-period Borrower's Sales Growth



This plot shows industry adjusted quarterly sales growth rate of borrowers of FBOs and non-FBOS, separately. Borrowers are those who borrow from either FBO or non-FBO (but not both) during July 2009 to February 18, 2014. Industries are defined by TNIC following Hoberg and Phillips (2010).

Table 2.8: Investment and Financial Performance of pre-period Borrowers

This table presents the differences in investment and performance during pre and post period across two groups of borrowers. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1) Capex	(2) SG&A	(3) Sales Growth	(4) Gross Margin	(5) Operating Margin
Diff_pre	0.031*** (5.58)	0.002 (1.10)	-0.001 (-0.22)	0.024*** (9.04)	0.020*** (8.24)
Diff_post	0.012** (2.13)	-0.005** (-2.21)	-0.001 (-0.24)	0.015*** (4.54)	0.016*** (4.93)
Diff-in-diff	-0.019** (-2.37)	-0.007** (-2.38)	0.000 (-0.02)	-0.010** (-2.25)	-0.004 (-1.11)

Figure 2.7: Pre-period Borrower's Gross Margin

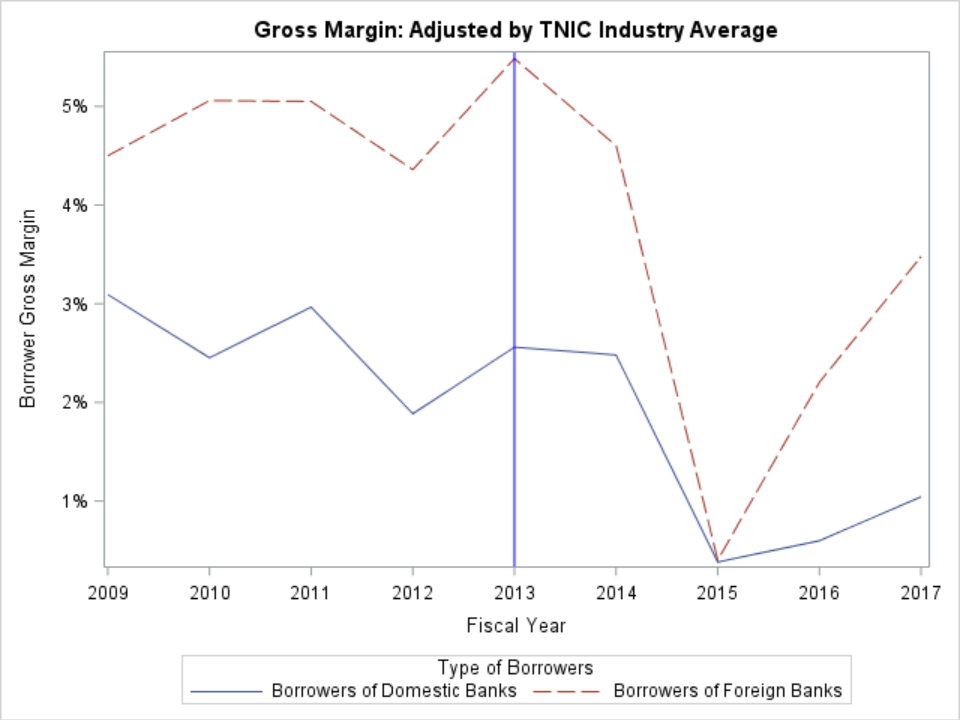
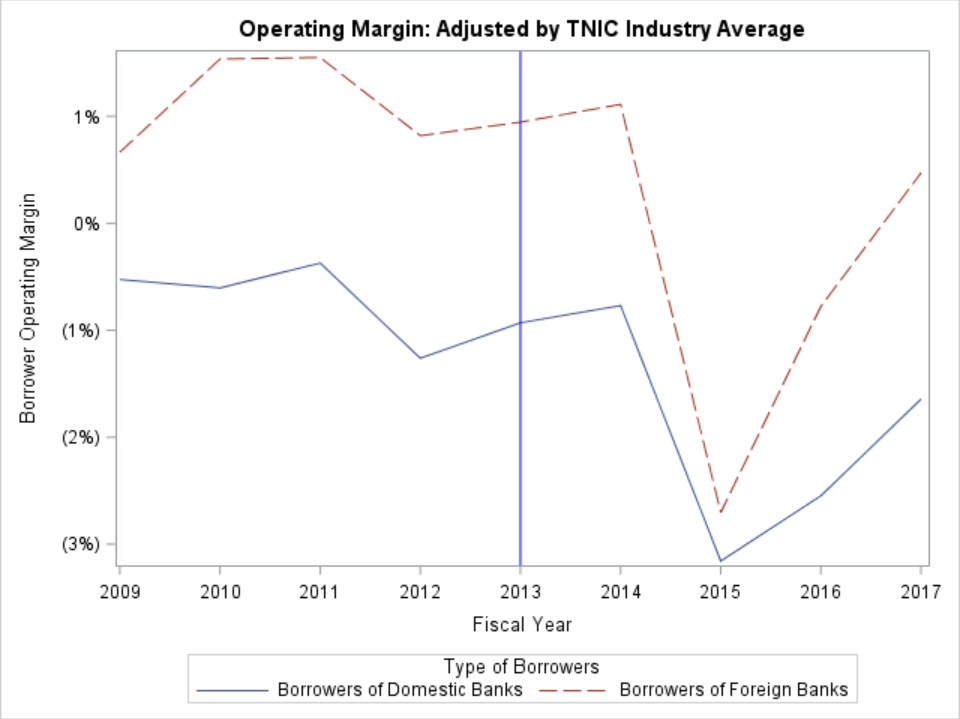


Figure 2.8: Pre-period Borrower's Operating Margin



shown in Figure 2.8, operating margins of two types of borrowers experience a similar change as gross margin but the difference-in-difference -0.004 (t-stat=-1.11) is statistically indifferent from zero. To understand why affected borrowers do not suffer much in profitability, I further examine their SG&A expenses, scaled by sales and adjusted by TNIC industry. Figure 2.9 shows the average SG&A expenses between 2009 and 2017. There is no significant difference in terms of SG&A expense during the pre-period (dif=0.002 and t-stat=1.10). However, in the post-period starting from 2014, there is a sharp drop by borrowers of FBOs (dif=-0.005 and t-stat=-2.21). The difference-in-difference is -0.007 with a t-stat of -2.38, meaning that compared with borrowers of domestics BHCs, FBO borrowers cut SG&A expenses sharply upon the passage of new regulatory rules to stay profitable. As another dimension of investment, Capex is also widely studied in the literature of financial constraints. Availability and cost of financing have the potential to cause firms to adjust their investment strategy. For example, firms may have to abandon some projects due to short of financing or because higher costs of capital make them become negative NPV projects. So I examine the trend in Capex as well and Figure 2.10 shows the results. The difference-in-difference is -0.02 (t-stat=-2.37). The observation that adjustment in Capex comes a little bit later than SG&A expenses reflects the reality that Capex is costlier and less flexible to adjust than SG&A expenses. Another possibility is that firms don't adjust Capex immediately because they are not sure how long the bank regulation induced financial constraints will last.

Taking the results from SG&A expenses, Capex, sales, gross margin and operating profitability together, I conclude that during the post-period FBO borrowers manage to maintain revenue growth but struggle with lower gross margin, implying that they cut prices. Meanwhile, they cut SG&A expenses and adjust Capex to keep their profitability. This finding suggests that the cost disadvantage caused by bank regulation and the threat of predation does affect borrowers' product market strategies[17]. Whether the adjustment in strategies will make affected borrowers survive predation is the question I am going to address in the next section.

[17]Results are quantitatively similar if I focus only on borrowers in highly competitive markets or use raw changes without industry adjustment.

Figure 2.9: Pre-period Borrower's SG&A Expenses

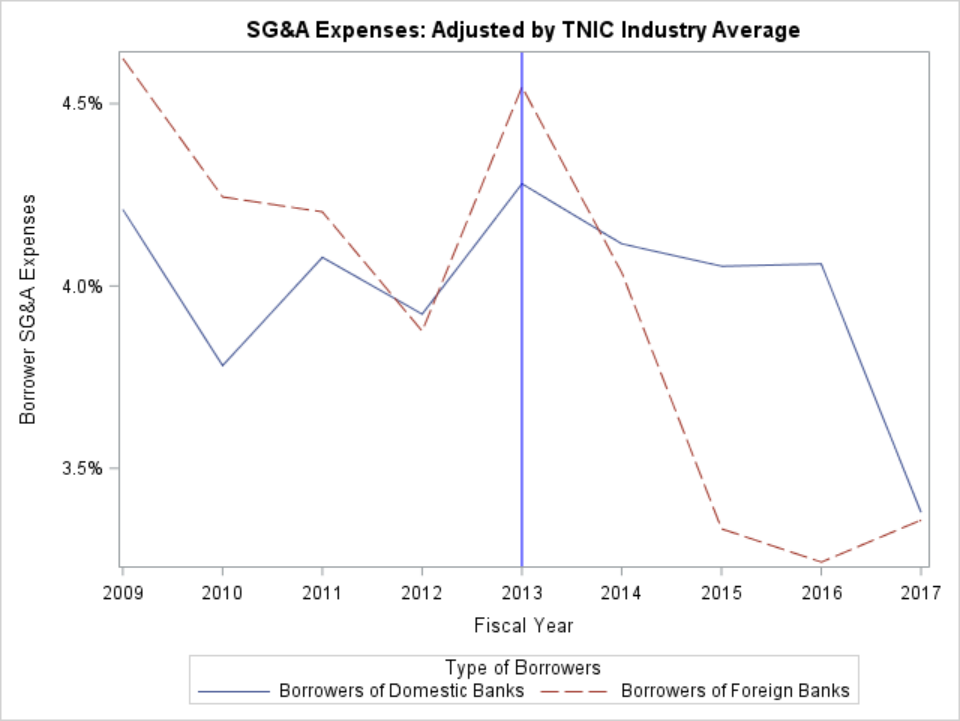
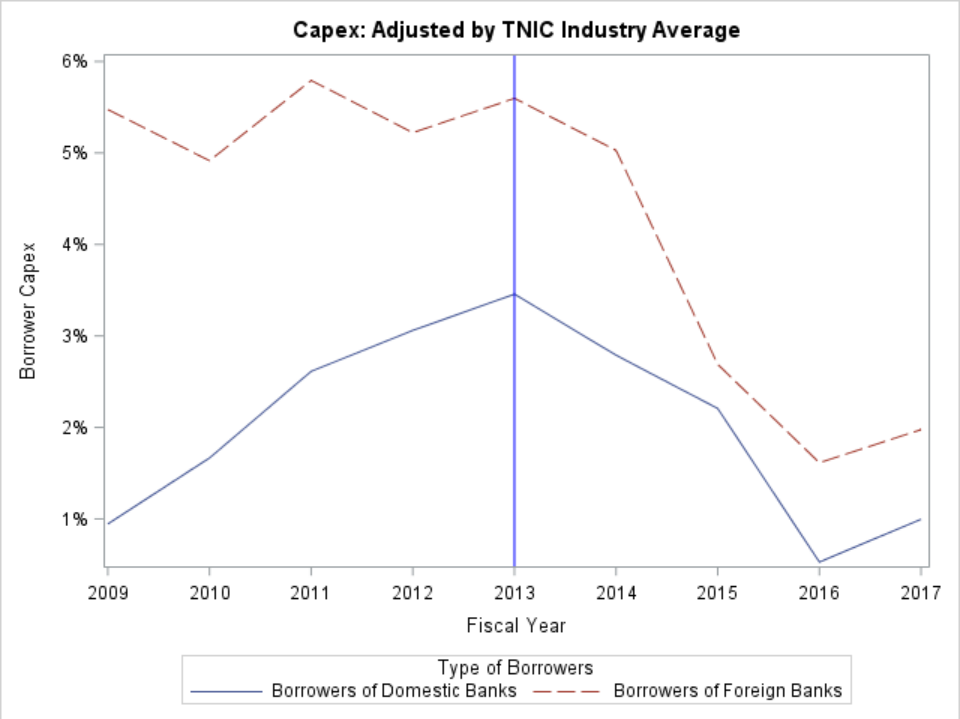


Figure 2.10: Pre-period Borrower's Capex



2.6.2 Predation Behavior by Rivals

Companies do not operate in isolation but as participants of the product market who strategically interact with others. Shocks to some firms can induce predation behaviors that change the industrial structure of the product market. The concept of rational predation is formulated by Bolton and Scharfstein (1990). Hubbard (1998) also discusses the implication of capital-market imperfections for product market behaviors and the probability of business survival. Firms operating in highly competitive markets are more likely to be targeted. This section examines how borrowers' survival/delisting probability relates to the competitiveness of their product market. As before I focus on the sub-group of borrowers who borrow during the pre-period but need to negotiate new loans in the post-period, requiring borrowers to survive at least till the event date[18]. I examine the delisting probability of this group of borrowers by running a linear probability model in the following form:

$$\begin{aligned} \text{Delist}_i =& \beta_0 + \beta_1 \text{FBO}_j + \beta_2 \text{Loan Variables}_{i,j,t} \\ &+ \beta_3 \text{Borrower Variables}_{i,t} + \beta_4 \text{Macro Variables}_t + \lambda_t + \varepsilon_i \end{aligned} \tag{2.4}$$

Delist_i is a dummy variable that equals one if a borrower delists after 90 days post the event date, regardless of the reason of delisting[19]. Column (1) and (2) of Table 2.9 show the results from regression using borrowers in high and low competition market separately. Column (1) shows that among firms facing more product market threats, borrowers of FBOs are more likely to delist in the post period. Compared with borrowers of domestic BHCs, the probability is 11.3% higher (t-stat=3.75). There is no such effect for borrowers in a low competition environment. To examine the cause of delisting, I take a closer look into the delisting codes and categorize delisting into two groups: delisting due to M&A or distressed delisting. I classify a delisting as M&A related if the CRSP delist code is in the 200s. Following the definition of performance-related delisting

[18]This is exactly the reason why a difference-in-differences approach is not working because this screening criteria guarantee the probability of delisting during the pre-period is always 0.

[19]I use 90 days post event as the cutoff because some delisting cases might already materialize before the event so the observed delisting right after the event date is not caused by the event of interest.

in Shumway (1997) and recent update on CRSP classification, distressed delistings are those with delist code of 400s (liquidations), 500, and 520 to 591 with the exception of 573(going private) and 575 (company request, offer rescinded, issue withdrawn by underwriter). Column (3) and (4) of Table 2.9 show the results from regression in equation 2.4 with Delist_i being replaced by a Delist_MA_i dummy. Column (5) and (6) of Table 2.9 are for distressed delisting probability. These results indicate that the overall delisting effect is driven by firms operating in highly competitive markets being acquired. I then manually check the identity of all acquirers and find in almost all the cases, acquirers are either rivals or private equity firms. When an acquirer is a private equity, shortly after the first acquisition, the target firm is subsequently sold by the private equity to a firm from the same industry as the target firm. There is no significant difference in the probability of distressed delisting, regardless of the competition environment. The 11% difference in the likelihood of being acquired is economically large, considering that a typical Dealscan borrower during the same period has a 24% probability to delist due to M&A related reasons and an average Compustat firm has a 4.4% probability of being acquired.

A major concern over the delisting analysis is that FBOs may on average lend to firms with a higher likelihood of being acquired. Table 2.5 shows the difference in borrower and loan characteristics of the facilities initiated during the pre-period across FBOs and non-FBOs. There are indeed significant differences in some dimensions. For example, loans from FBOs are on average of longer maturity and of larger size. Borrowers of FBOs are larger in size, lower in market-to-book, higher in leverage, lower in profitability. To address the concern that the observed delisting effect is a reflection of matching between borrowers and lenders, I repeat the delisting analysis using a propensity score matching approach. Using all the variables used in table 2.5 to match the probability of obtaining loans from FBOs, I estimate the average treatment effect on the delisting probability during the post period and results are displayed in table 2.10. No matter which period I use as the pre-period, the results are always quantitatively similar to that from regression specifications.FBO borrowers are more likely to delist during the post-period due to M&A related reasons and the effect is concentrated among borrowers facing higher product market fluidity.

Table 2.9: Delisting Results: Linear Probability Model

This table presents the results from a linear probability model, as specified in equation 2.4, that predicts the delisting probability of borrowers who borrow from either FBOs and non-FBOs during the pre-period and are still traded in the exchange covered by CRSP by the event date. Also the facility end date is beyond the event date. Definitions of control variables are in the Appendix. Standard errors are two-way clustered by lenders and borrowers. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	Delist	Delist	Delist_MA	Delist_MA	Delist_Dis	Delist_Dis
Fluidity	High	Low	High	Low	High	Low
FBO	0.113***	0.017	0.107***	0.021	0.025	-0.000
	(3.75)	(0.53)	(3.59)	(0.66)	(1.01)	(-0.01)
Log of Loan Size	0.002	-0.021**	-0.001	-0.016*	-0.003	-0.010
	(0.22)	(-2.49)	(-0.09)	(-1.69)	(-0.44)	(-1.40)
Log of Loan Maturity	-0.034	-0.067	-0.006	-0.060	-0.049*	-0.009
	(-0.68)	(-1.66)	(-0.11)	(-1.44)	(-1.86)	(-0.87)
Log of Borrower Assets	-0.047***	-0.029**	-0.036**	-0.026*	-0.022***	-0.005
	(-2.85)	(-2.26)	(-2.28)	(-1.87)	(-3.07)	(-0.68)
Market to Book	0.014	0.020**	0.007	0.020**	0.012**	0.001
	(1.61)	(2.60)	(0.74)	(2.33)	(2.27)	(0.38)
Leverage	-0.002	0.000	-0.002*	-0.001	0.000	0.002**
	(-1.44)	(0.08)	(-1.72)	(-0.43)	(0.46)	(2.26)
Profitability	0.001	-0.004	0.005*	-0.003	-0.007**	-0.002*
	(0.15)	(-1.40)	(1.69)	(-0.93)	(-2.63)	(-1.73)
Tangibility	-0.000	0.001	-0.001	0.001	0.000	0.000
	(-0.26)	(1.27)	(-0.55)	(1.23)	(0.31)	(0.06)
Cash Flow Volatility	-0.000	0.000	-0.000	0.000	0.000	0.000*
	(-0.39)	(0.45)	(-0.83)	(0.02)	(1.66)	(1.81)
Modified Z-score	-0.061*	-0.032	-0.040	-0.040*	-0.033*	0.012
	(-2.03)	(-1.61)	(-1.51)	(-1.98)	(-1.75)	(1.40)
KZ-index	0.001	0.001	0.000	0.001	0.000	0.000
	(0.35)	(1.41)	(0.01)	(1.48)	(0.43)	(0.15)
Distance to default	-0.012**	-0.006	-0.011**	-0.004	-0.000	-0.003
	(-2.14)	(-1.35)	(-2.51)	(-0.93)	(-0.15)	(-0.82)
Credit Spread	-0.000	0.000	0.000	-0.000	-0.000	0.000
	(-0.24)	(0.07)	(0.09)	(-0.04)	(-0.42)	(0.58)
Term Spread	-0.000	0.000	-0.000	0.000	0.000	-0.000
	(-0.12)	(0.91)	(-0.38)	(1.03)	(1.24)	(-0.84)
Federal fund rate	-0.006	-0.017	-0.013	-0.022	0.001	0.006
	(-0.25)	(-1.04)	(-0.49)	(-1.08)	(0.10)	(0.73)
Other Appropriate Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year/LoanType/Industry FE	Yes	Yes	Yes	Yes	Yes	Yes
N	1399	2105	1312	2052	1100	1676
Adj R_square	0.175	0.124	0.148	0.107	0.325	0.170

Table 2.10: Delisting Results: Propensity Score Matching

This table presents delisting probability based on the propensity score matching method using different pre-period samples. Coefficients are estimated by Stata "teffects psmatch" command. Matching variables are the same as used in delisting regression. Definitions are in the appendix. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1) Delist	(2) Delist_MA	(3) Delist_Distress	(4) Delist_MA (High Fluidity)
Pre-period: 01 Jul, 2009 - 18 Feb, 2014	0.072***	0.073***	(0.003)	0.089***
	(3.31)	(3.44)	(-0.31)	(2.64)
Pre-period: 01 Jul, 2012 - 18 Feb, 2014	0.085***	0.090***	(0.010)	0.135***
	(3.03)	(3.26)	(-0.94)	(2.75)
Pre-period: 01 Jul, 2009 - 18 Feb, 2014 (exlude EU Debt Crisis)	0.118***	0.113***	0.001	0.103***
	(4.67)	(4.57)	(0.09)	(2.94)

To further understand whether the M&A results are driven by loan price effects or quantity effects, I create a dummy to indicate whether a pre-period borrower also borrow during the post-period and then run separate regressions for borrowers with and without loans during the post-period. Results are in Table 2.11. As shown in column (1) and (3), FBO borrowers who have no loan in the post period do not face a higher probability of being acquired compared with borrowers of domestic BHCs who also have no loans in the post-period. But the likelihood is significantly higher for FBO borrowers who borrow during both period, as in column (2) and (4). This analysis provides preliminary evidence that the M&A results are not driven by firms' ability to access the bank loans. Instead, it is more likely to be a loan price effect.

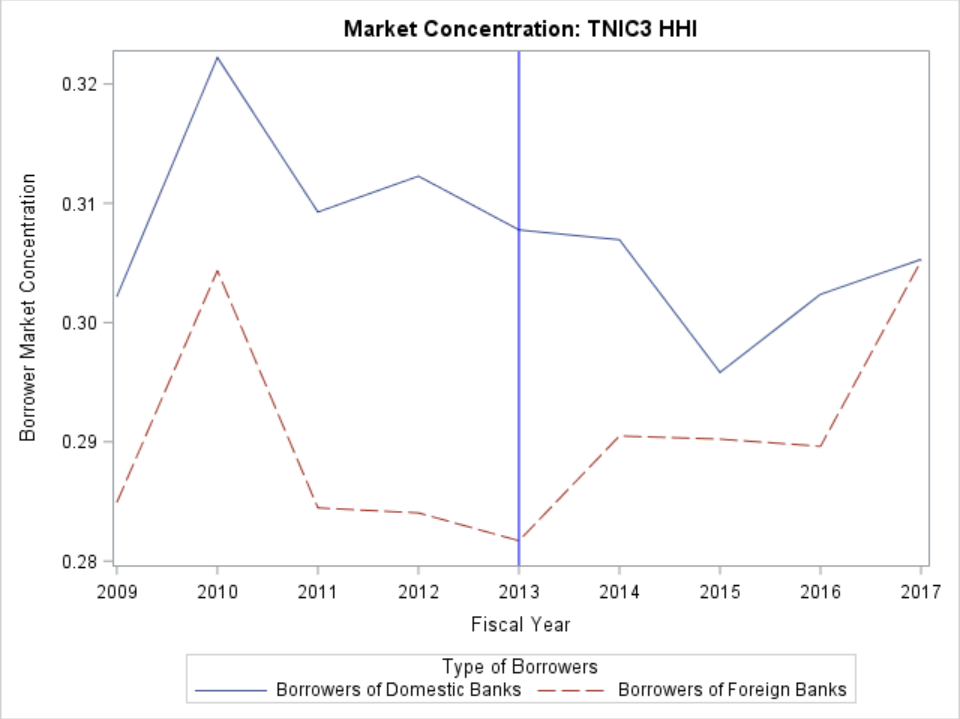
A natural consequence of M&A activities is the change in industry concentration and competition. Figure 2.11 plots the industry Herfindahl–Hirschman Index (HHI)of the two types of borrowers over time. Industries are defined by TNIC developed by Hoberg and Phillips (2010, 2016) and HHI data is from Hoberg-Phillips Data Library. As shown in Figure 2.11, borrowers of FBOs experience an increase in market concentration while at the same time borrower of non-

Table 2.11: M&A : Sub-sample based on Loan Availability

This table presents the M&A results using sub-sample based on whether pre-period borrowers have loan in the post-period. The linear probability model, as specified in equation 2.4, predicts the delisting probability of borrowers who borrow from either FBOs and non-FBOs during the pre-period and are still traded in the exchange covered by CRSP by the event date. Also the facility end date is beyond the event date. Definitions of control variables are in the Appendix. Standard errors are two-way clustered by lenders and borrowers. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1)	(2)	(3)	(4)
Pre-period	01 Jul, 2009 - 18 Feb, 2014	01 Jul, 2009 - 18 Feb, 2014	Two-Year Before Event	Two-Year Before Event
	No Loan in Post-period	Has Loan in Post-period	No Loan in Post-period	Has Loan in Post-period
FBO	0.019	0.114***	0.001	0.130***
	(0.30)	(4.31)	(0.01)	(3.46)
Log of Loan Size	0.029	-0.004	0.081*	-0.013
	(0.58)	(-0.33)	(1.86)	(-0.80)
Log of Loan Maturity	-0.052	0.034	-0.086	0.025
	(-0.90)	(0.68)	(-0.78)	(0.55)
Log of Borrower Assets	-0.086**	-0.013	-0.049	-0.019
	(-2.14)	(-0.77)	(-1.24)	(-1.12)
Market to Book	0.039**	-0.005	0.093***	0.000
	(2.29)	(-0.71)	(3.83)	(0.01)
Leverage	-0.001	-0.003**	-0.004	-0.005***
	(-0.19)	(-2.61)	(-1.11)	(-4.07)
Profitability	-0.007	0.005**	-0.007	0.010***
	(-1.17)	(2.09)	(-0.72)	(2.98)
Tangibility	0.004*	-0.002*	0.006	-0.002**
	(1.78)	(-2.04)	(1.60)	(-2.06)
Cash Flow Volatility	0.000	-0.000	-0.000	0.000
	(0.73)	(-1.34)	(-1.39)	(0.26)
Modified Z-score	0.110**	-0.060**	0.050	-0.095**
	(2.72)	(-2.15)	(0.69)	(-2.33)
KZ-index	0.002	0.001	-0.001	0.002
	(0.51)	(0.45)	(-0.30)	(1.41)
Distance to default	-0.013	-0.010*	-0.019*	-0.014**
	(-1.33)	(-1.85)	(-2.08)	(-2.08)
Credit Spread	0.002	-0.001	-0.001	0.000
	(0.95)	(-1.03)	(-0.41)	(0.40)
Term Spread	-0.002**	-0.000	-0.004	0.000
	(-2.36)	(-0.88)	(-1.38)	(0.26)
Federal fund rate	-0.033	0.003	-0.083	0.097**
	(-1.39)	(0.11)	(-1.01)	(2.55)
Other Appropriate Dummies	Yes	Yes	Yes	Yes
Year/LoanType/Industry FE	Yes	Yes	Yes	Yes
N	274	1088	152	617
Adj R_square	0.466	0.223	0.470	0.257

Figure 2.11: Pre-period Borrower's TNIC Industry HHI



This plot shows industry HHI of borrowers of FBOs and non-FBOS, separately. Borrowers are those who borrow from either FBO or non-FBO (but not both) during July 2009 to February 18, 2014. Industry are defined by TNIC following Hoberg and Phillips (2010).

FBOs face a decrease. This finding echoes concern over product market competition trend, which has important policy implications for anti-trust enforcement.

2.7 Conclusion

Using the Federal Reserves' passage of new rule on FBOs regulation as an exogenous shock that significantly increases the compliance burden of FBOs, I study the effect of bank regulation on the real economy. Using syndicated loan data, I find FBOs cut lending and pass the compliance cost to borrowers in the form of higher loan spreads. The price effect concentrates among borrowers who operate in highly competitive markets. Borrowers of FBOs, facing a financing cost disadvantage and threats of predation by rivals, adjust their product market strategies. They cut prices to maintain market share and also cut SG&A expenses and Capex to maintain profitability. However, the adjustment in investment and pricing strategies does not protect them from predation, and they are still facing a higher probability of being acquired by rivals or private equity firms. These find-

ings suggest that bank regulation had unintended consequences on industry structure through the product market competition channel.

3. HOW DOES LENDER HEALTH AFFECT COVENANT VIOLATING BORROWERS?

3.1 Introduction

Financial covenants are an important tool that banks use to monitor corporate borrowers. Violations of such covenants, usually caused by an involuntary deterioration in borrower operating performance,[1] give banks control rights, which they sometimes use to force borrowers to take actions to protect against further declines in credit quality or to increase compensation for risk (e.g., Baird and Rasmussen (2006)). Prior studies (e.g., Chava and Roberts (2008) and Nini, Smith, and Sufi (2009)) show that violations of financial covenants often lead to a sharp drop in investment, consistent with creditors influencing investment policy even before borrowers miss a payment. In addition, there is a large literature that examines how bank reactions to financial covenant violations are related to borrower characteristics.[2] As Smith (1993) points out in his seminal review article, however, it is also important to consider whether bank financial health causally impacts resolution of such violations. Our paper focuses on the *causal* effect of lender health on the resolution of loan covenant violations.

A priori, whether bank health affects the resolution of financial covenant violations is unclear. Banks could use such violations as an opportunity to extract extra income or reduce risk, motives that are plausibly stronger for banks in poorer health. However, this comes at the cost of pushing borrowers closer to default, potentially hurting the future profitability of banks, a cost that is particularly significant for banks in poor health. Thus, the causal impact of bank health on the resolution of covenant violations is an open empirical question, which we explore.

A particular challenge in identifying Smith (1993)'s hypothesized lender health effect is that banks in poorer health are also more likely to simultaneously have borrowers in poorer health,

[1] In contrast, note the voluntary nature of violations of *negative* or *affirmative* covenants, which prohibit or require certain borrower actions, such as issuance of additional debt or the purchase an insurance policy. Henceforth, unless otherwise specified, all mentions of "covenants" refer to *financial* covenants, which require borrowers to maintain certain financial ratios above contractually pre-specified minimums (or less commonly, below maximums in the cases of some ratios such as leverage ratios).

[2] See Smith (1993) for a review of this literature.

relative to that of healthier banks. An association between lender health and the stringency with which covenant violations are resolved could thus be driven by either borrower or lender health effects. For example, for reasons discussed below, it is likely that banks finding themselves in the worst health during the Global Financial Crisis of 2007–2009 had made the riskiest loans before the crisis, so their borrowers also saw a greater in-crisis decline in health relative to others. Hence one cannot rule out the hypothesis that lenders most adversely impacted by the crisis, relative to others, became more stringent not because of the greater crisis-induced deterioration in their own health, but rather, because of that of their borrowers. Clean empirical identification of a causal lender health effect is thus an important aspect of our research question.

To achieve clean identification, we exploit the emerging market crises of the late 1990s to run a quasi-natural experiment. This shock adversely affected a subset of U.S. banks, but, unlike the Global Financial Crisis, it did not directly affect U.S. domestic borrowers. We find this exogenous adverse shock causes a significant increase in the odds that affected lenders resolve covenant violations stringently, relative to unaffected lenders. Furthermore, we find an exogenous shock to lender health has real effects: it increases the probability that covenant violations lead to borrower distressed delisting. We also find evidence of an effect on borrower investment, but this effect is not unique to covenant-violating firms. Near-violators also reduce their investment. We infer that investment effects apply more generally to unhealthy borrowers of unhealthy lenders, not just covenant-violating borrowers.

Borrowers violate financial covenants when they experience profitability, cash flow, or liquidity declines large enough to push certain ratios (e.g., operating profit-to-interest expense) cross contractually pre-specified threshold. Lenders can respond in a number of ways. They can give a blanket waiver, allowing the borrower to continue with little impact from the violation. In some cases, the lender may even agree to loosen the violated covenant, giving borrowers increased flexibility. Alternatively, lenders can choose to be stringent and impose costs on the borrower. Such costs include waiver fees, an increased interest rate, a reduction in credit availability, requiring additional collateral, and, at times, even forcing a borrower into bankruptcy by demanding immediate

repayment of the loan.

Though a large prior literature finds that borrower characteristics are important to the decision on how lenders react to a covenant violation, it is plausible that lender characteristics are also important. For example, banks closer to financial distress or regulatory violations have an incentive to reduce the risk of their loan portfolio.[3] One way to reduce such risk would be to take a harder line with financial covenant violators by, for example, reducing credit available, shortening the maturity, or imposing new covenants that increase the bank's control rights. In addition, the increased risk to the bank signaled by a covenant violation is magnified for banks closer to distress, so it is also plausible that banks closer to distress will demand more compensation from covenant violators in the form of fees or interest rate hikes.[4] On the other hand, it is possible that a deterioration in financial health could make lenders more reluctant to take actions that result in lost business or that push their borrowers closer to distress. Hence it is possible that deterioration in bank health could make banks more lenient with covenant violators. Therefore, the manner in which bank financial health impacts covenant violators is an empirical question. The primary challenge in identifying a bank health treatment effect is in disentangling it from the borrower health effect. The correlation between borrower and lender health naturally arises in two contexts. First, particularly poor borrower health may be what causes poor lender health in the first place. Second, many global shocks simultaneously affect both borrowers and lenders. To the extent that the worst performing lenders tended to make investments (including bank loans) that performed the worst with such shocks, they would be harsh with covenant-violating borrowers due to a borrower health effect, even absent a lender health effect. Thus, an ideal identification of a bank health effect requires a shock to bank health that does not simultaneously affect borrower health.

To cleanly identify a bank health treatment effect, we follow Chava and Purnanandam (2011)

[3]Berger and Bouwman (2013) find that the amount of capital a bank has affects its survival probability and market share, particularly during banking crises. It seems plausible that lender conditions including capital ratios and metrics of risk and opacity may affect bank decisions as they seek to avoid default. Smith (1993) discusses, in hypothetical terms, that banks closer to regulatory violations are likely to take this into account when dealing with covenant violating borrowers.

[4]Bird et al. (2017) find that banks close to missing earnings targets are more likely to enforce material covenant violations in order to extract extra income.

and Lo (2014) and exploit the emerging market sovereign debt and currency crises of 1998–1999, which affected Russia, Brazil and eventually the rest of Latin America, as a quasi-natural experiment.[5] The crises were significant and unexpected, but only a subset of U.S. banks were exposed to them. In addition, direct exposure of U.S. borrowers to these crises was minimal. Hence, for U.S. borrower-lender pairs, the crises constitute a significant negative exogenous shock to lender financial health with no direct effect on the borrower. Using calendar-time factor portfolio stock return regressions, we verify that U.S.-listed covenant violators indebted to exposed U.S.-chartered banks are themselves no more exposed to the crisis countries than are the covenant violators indebted to unexposed banks. The similar exposure of all borrowers in our sample to crisis countries provides validity for the difference-in-differences analysis that we run using this quasi-natural experiment.[6] We also further confirm the validity of our identification by conducting pre-period trend analysis and placebo tests.

Empirically, we find that the probability of banks acting stringently with covenant violators increases after the crisis by 24 percentage points more for exposed banks relative to unexposed banks. This effect is economically large relative to a base probability of 45%. Stringent actions are equally likely to be risk-reducing (reducing credit availability, shortening maturity, etc.) and income-increasing (raising the interest rate, charging fees, etc). Furthermore, we find the effect of crisis exposure on lender stringency with covenant violators is strongest for lenders whose pre-crisis capital ratios are close to regulatory minimums. Thus, adverse shocks to lender health have stronger effects when they push lenders closer to their regulatory constraints. Our results, therefore, in addition to establishing a causal channel of lender health on covenant violation resolutions, also establish that banks' capital ratios influence their real decisions. This finding complements that of Berger and Bouwman (2013), who find that capital ratios impact bank performance.

Next, we examine real effects on borrowers that violate financial covenants. The average covenant violator indebted to exposed banks sees its probability of a future distressed delisting

[5] See also Schnabl (2012).

[6] In contrast, had we found that borrowers from exposed banks were more exposed to crisis countries, we would be unable to disentangle the lender health channel from the borrower health channel due to a violation of the parallel trends assumption inherent to the difference-in-differences framework.

increase by 18 percentage points post crisis relative to those indebted to unexposed banks. Further results suggest that harsher covenant violation resolution is the channel through which this effect operates. We find no detectable treatment effect on distressed delisting of non-violating borrowers in similarly poor financial health compared to violators, thereby ruling out harsher lender treatment of all struggling borrowers, independent of covenant violations, as the channel though which this effect operates.

Among violators that survive, we do not find a significant average treatment effect on real outcomes, except among violators indebted to banks with low capital ratios, for whom we find a significant effect on investment. However, we cannot attribute this investment treatment effect solely to a direct covenant violation resolution channel as we find a similar effect on non-violating borrowers in similarly poor financial health. Rather, our findings suggest that both harsh violation resolutions directly, as well as more aggressive borrower action to avoid future violations due to an increased anticipated lender harshness, are responsible for the lender health treatment effect on investment.

The study closest to ours is Chodorow-Reich and Falato (2019). They find that lenders more adversely impacted by the Global Financial Crisis of 2008–2009, due to their greater exposure to subprime residential mortgages, Lehman Brothers, or risky trading securities, become more likely to reduce the size of loan commitments in response to a covenant violation than do less impacted lenders. Interpreting these results as supportive of a causal lender health effect requires assuming that the crisis had the same direct impact on covenant violators indebted to banks with greater exposure to these risks as it had on violators indebted to banks with less exposure. Alternatively, if there was a different direct crisis impact on the two groups of covenant violators, the required identifying assumption is that the small number of observable borrower characteristics in the paper's specifications perfectly capture it.

While the validity of the identifying assumption in Chodorow-Reich and Falato (2019) is difficult to directly refute (just as it is impossible to prove), there are important reasons to doubt it. To believe it, one must also believe that taking on greater exposure to subprime, Lehman, and trading

securities were isolated instances of increased bank risk-taking, uncorrelated with risk-taking in other business lines, such as corporate lending. However, evidence in prior research suggests that the greater risk-taking by the banks most exposed to the crisis was a result of firm-wide practices. Ellul and Yerramilli (2013) find that banks with weaker firm-wide risk management controls before the crisis had significantly worse outcomes during the crisis, including more non-performing loans, and Ellul (2015) argues that such weak risk controls are likely the product of a pervasive "risk culture of a financial institution as set by the senior management." Fahlenbrach, Prilmeier, and Stulz (2012) find that banks' overall risk culture determined their exposure to and performance in the crisis. Finally, Bhattacharyya and Purnanandam (2011) and Kolasinski and Yang (2018) find that incentives for executives to excessively focus on short-term performance before the crisis can explain much of the variation in bank exposure to the crisis. To the extent that there is some correlation in the level of risk-taking and prudence by banks across business lines, such as through poor governance, compensation-related incentives, risk management practices, or general bank-level culture, the same banks that took on more subprime, Lehman Brothers, and trading securities exposure were likely to have made riskier and less prudent business loans before the crisis than banks that were less exposed. Given the large, far-reaching global impact of the crisis, such a difference in borrower risk, in turn, would cause covenant violators indebted to banks more exposed to the aforementioned risks to themselves be more severely impacted by the crisis than covenant violators indebted to less exposed banks, for reasons unrelated to their lenders' health. Furthermore, though the borrower control variables employed by Chodorow-Reich and Falato might capture a few of the relevant differences between the two groups of covenant violators, they almost surely do not capture them all.[7] As a result, much of the effect that they find is likely attributable to borrower health effects.[8]

[7] As noted by Denis and Wang (2014), some of the most important factors considered by lenders when deciding whether to loosen a covenant are the borrower's future prospects, which are generally not observable to the econometrician.

[8] Their finding that, upon a covenant violation, exposed lenders reduce their syndicate share relative to less exposed lenders in the same syndicate is more cleanly identified, but it provides no indication of how much of their main result is due to lender or borrower health effects. Furthermore, a change in syndicate composition does not imply an adverse effect on the borrower.

In contrast, our setting, the emerging market debt crises of the late 1990s, allows us to more cleanly disentangle borrower and lender health effects because this crisis, unlike the Global Financial Crisis, had a minimal direct effect on borrowers in the U.S. economy. Furthermore, tests described below suggest that borrowers indebted to affected banks did not have greater direct exposure to any of the crisis-hit emerging markets than borrowers indebted to unaffected banks. Thus, for both theoretical and empirical reasons, the parallel trends assumption for borrower health is likely to be satisfied in our setting. In addition to cleaner identification, our study differs from Chodorow-Reich and Falato (2019) in that we estimate bank health treatment effects during a fairly typical expansion period. Their sample period coincides with the worst economic downturn of the post-war era. Our results suggest that, during an expansion period, real effects of bank health on borrowers are largely driven by banks that were already close to their regulatory capital minimums before being hit by the adverse shock. That is, it requires a bank to be in particular trouble for there to be significant real effects. During the Global Financial Crisis, most banks were in some level of significant trouble, making such a nuance difficult to identify in their setting. Furthermore, our hand-collected data allows us to study different types of stringent lender actions (risk-reduction vs. income extraction), whereas Chodorow-Reich and Falato almost exclusively focus on credit reduction. Finally, we study a real outcome of borrowers that seems particularly important, distressed delistings, and also design tests to show that the effect of lender health on borrower delistings flows through covenant violations, in contrast to other real effects of lender health on borrowers, which affect even non-violators. Thus, even if one ignores the endogeneity concerns with the Chodorow-Reich and Falato results, an important contribution of our study is to provide a more nuanced view of the scope of the lender health effect on troubled borrowers.

Our ability to identify a causal effect of bank health on the stringency with which covenant violations are resolved has important policy implications. In recent years, academics and policymakers have debated the need for additional regulations on banks, weighing the costs and benefits.[9] Reliance on spurious (and general) correlations between bank health and stringent treatment of

[9]See, for example, Acharya, Berger, and Roman (2018), Bao, O'Hara, and Zhou (2018), Bessembinder et al. (2018), and Cortes et al. (2018).

covenant violators could lead policymakers to incorrectly infer that a deterioration in bank health can impact the real economy through the covenant violation resolution channel. This, in turn, could lead to an overemphasis of the benefits of banking regulation. In contrast, by cleanly identifying a causal effect of bank health on covenant violation resolutions, our study provides reliable evidence that can inform policymakers of the extent that bank health matters in the commercial loan market.

3.2 Literature Review

An early literature provided small sample evidence of the effects of covenant violations. Using a sample of 91 firms between 1983 and 1987, Beneish and Press (1993) were the first to study the costs of covenant violations, finding increased interest costs between 0.84 and 1.63 percent of the market value of equity. Chen and Wei (1993) use a sample of 120 violations and find that waivers are more likely for firms further from default. Defond and Jiambalvo (1993) and Sweeney (1994) examine accounting choices, finding abnormal accruals prior to covenant violations, suggesting that managers take actions to try to avoid violations. Beneish and Press (1995) find a negative announcement effect to covenant violations. Overall, the early literature on covenant violations used limited data and found that managers use accruals to try to avoid covenant violations and, once violations occur, there are significant costs to borrowers.

More recently, the literature has used larger samples and focused mostly on the effects on borrowers of covenant violations. Dichev and Skinner (2002) find support for the hypothesis that managers make accounting choices to reduce the likelihood of covenant violations. Chava and Roberts (2008) use Dealscan data and find lower investment for firms that violate financial covenants. Roberts and Sufi (2009) find a decline in net debt issuance after covenant violations, and Nini, Smith, and Sufi (2012) find that firms make significant changes to investment and financial policies around covenant violations. Both Roberts and Sufi and Nini, Smith, and Sufi hand-collect large amounts of data in their papers. Finally, Jha (2013) uses the data from Nini, Smith, and Sufi to study accruals management before, during, and after covenant violations.

The most significant gap in the literature, which we aim to fill, is in the *causal impact* of the health of lenders on their choice of covenant cures. The existing literature has very naturally

shown that borrower health affects lenders' decisions on whether to take action against a covenant violating firm. However, it is also natural to expect that lender decisions could also vary with their own credit conditions. Identifying the extent to which such a channel operates has important implications for the design of financial regulations such as bank stress tests and counter-cyclical capital requirements.

3.3 Hypotheses

Bank loan contracts typically do not give lenders control rights except when the borrower renegotiates debt, needs to rollover debt, defaults on debt, or violates a covenant. Our focus is on financial covenants, which give lenders contingent control rights. Evidence in Nini, Smith, and Sufi (2012) shows that after covenant violations, borrowers make significant cuts to assets, PP&E, and acquisitions. They interpret this finding as evidence that creditors play an active role in the corporate governance of a firm as the firm's performance declines. Our focus is on the lender's choice of how to cure covenant violations. Lenders have the option to impose stricter lending terms or to simply waive violations. Though the existing literature has largely focused on borrower conditions, our main contribution is to examine how lender conditions are related to covenant cures, in a causal way.

It is plausible that the financial condition of lenders impacts how lenient they are with borrowers that violate covenants. Managers of a bank that is closer to financial distress have an incentive to reduce the risk of the bank's loan portfolio in order to increase its probability of survival, as well as reduce its probability of running afoul of regulators. Thus we might expect to see banks closer to distress extract concessions such as a reduction in loan amount outstanding, a shortening of maturity, and greater limitations on dividends and capital expenditures. Alternatively, such banks could plausibly also impose higher compensation for risk, in the form of higher interest rates or waiver fees, in response to a covenant violation. When a bank is closer to financial distress, undiversified, risk-averse managers will tend to demand a higher return in exchange for the same level of risk. Hence banks closer to distress will be more likely to drive a hard bargain and extract a greater rate increase or waiver fee from a violator. Hence we present our first empirical hypothesis

as follows:

H1: Banks are more likely to be stringent with covenant violators when their own financial health is poorer.

As noted above, banks who just experienced large losses are plausibly more desperate to avoid losing business. Hence the opposite of Hypothesis 1 is also plausible, *a priori*. The effect of lender health on the way lenders treat covenant violators, therefore, remains an open empirical question, which we resolve. Furthermore, given the importance of regulatory capital ratios, we might also expect that financial health effects are amplified when the ratio is low.

The bond market constitutes an alternative source of financing for borrowers. We hypothesize that the effect of lender health on the odds a bank treats a covenant violator stringently is stronger when the borrower is unable to access the bond market. Using the existence of a bond rating or outstanding bonds as a proxy for borrower access to the bond market, we predict:

H2: The effect of lender health on lender stringency with covenant violators is stronger when borrowers do not have a bond rating or bonds outstanding.

Prior research finds that covenant violators suffer real adverse consequences when their lenders act stringently (e.g., Chava and Roberts (2008) and Nini, Smith, and Sufi (2009)). Thus:

H3: Covenant violators are more likely to suffer adverse real effects when their lenders are in poor financial health.

Adverse real effects we consider include reduction in book assets, capital expenditures, and debt, as well as increased probability of distressed delisting.

It is possible that treated lenders act more harshly with all borrowers that are in poor financial health. Since covenant violators, on average, are in poorer financial health than non-violators, it is possible that any real effects we find for covenant violators are due to such treatment of all struggling borrowers, independent of whether they violate covenants. To disentangle harsher lender treatment of all struggling borrowers from a covenant violation channel, we test:

H4: Lender health deterioration causes similar adverse real effects in non-covenant-violating borrowers in a poor state of financial health similar to that of covenant violators.

Even if we confirm H4, there are two possible channels through which there could be a lender health treatment effect on real outcomes for troubled, but non-covenant-violating borrowers. First, it is possible that poor financial health causes lenders to act more harshly with all troubled borrowers, regardless of violation status. While covenant violations convey direct control to lenders, even prior to violations, lenders can negatively impact borrowers through other harsh actions, such as by refusing to roll-over debt, provide additional loans, or renegotiate negative covenants. Alternatively, we might see real effects even on non-violating but struggling borrowers if a deterioration in their lender's health makes them more likely to anticipate harsher resolution to future covenant violations. Hence, even non-violating treated firms in poorer health take more aggressive action, such as cutting investment, to avoid future violations. While empirically disentangling these two channels is difficult, finding different types of real effects for non-violating borrowers would provide suggestive evidence. If an overall increase in lender stringency with all troubled borrowers, regardless of violation status, is the channel through which the lender health treatment effect operates, we would expect to see treatment effects on all real outcomes for non-violating borrowers in poor health. If the channel is more aggressive action to avoid future covenant violations by treated non-violators, we would expect to see real effects on non-violators only for directly borrower-controlled outcomes, such as investment, that impact the probability of future violations.

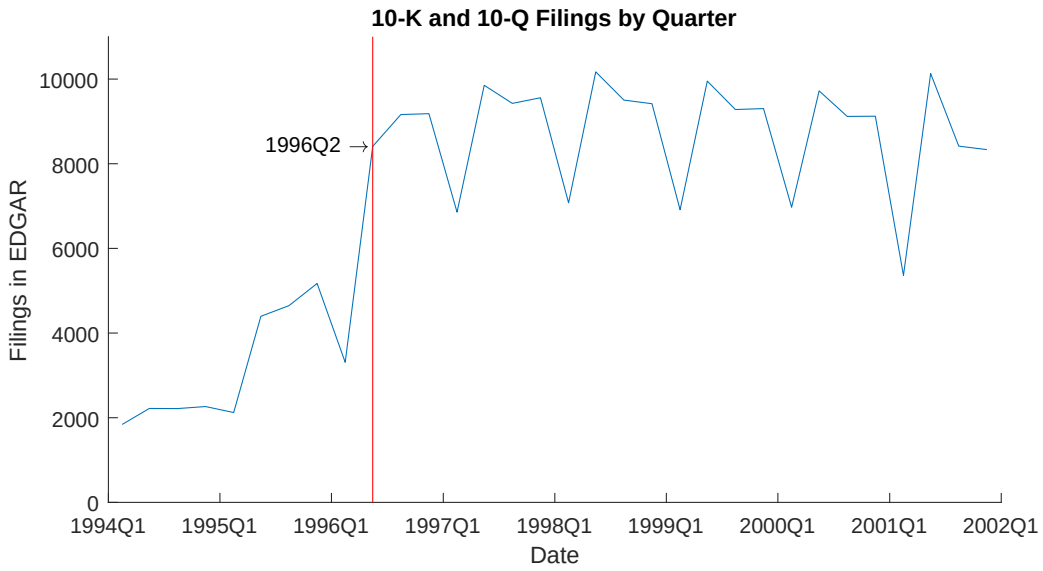
3.4 Data

Our data collection starts with the sample of covenant violations compiled by Nini, Smith, and Sufi (2012) and posted on Amir Sufi's website. We use violations from Q3 1996 to Q2 1997 and Q4 1998 to Q4 1999. We begin our sample in Q3 1996 because we require electronic SEC filings to identify covenant cures, and the SEC only made electronic filing mandatory for fiscal periods ending after May 6, 1996.[10] In Figure 3.1, we show that Q2 1996 was the first quarter in which there was a spike in 10-Q and 10-K filings. Because we use SEC filings to identify new covenant

[10] See SEC release 34-36997.

violations (as discussed further below), the first quarter in which we can reliably identify violations that were not previously discussed is Q3 1996.

Figure 3.1: EDGAR Filings Over Time



Plot of the total number of 10-K and 10-Q filings in EDGAR for each quarter from the first quarter in 1994 to the last quarter of 2001.

We note further that the Asian Crisis began at the end of Q2 1997, whereas the Russian default occurred just in the middle of Q3 1998. Hence we exclude the Q3 1997 through Q3 1998 to ensure the Asian crisis does not contaminate our results (we discuss our reasons for excluding the Asian crisis below). Since our analysis requires us to identify the lender, we further limit the sample to covenant violators whose loans are in the Dealscan database, which covers the universe of syndicated loans.

We use the Schwert (2018) linktable to map each lender designated by Dealscan as the lead arranger to its respective bank holding company in Compustat. We further require banks to have non-missing data on total book assets and tier 1 capital as of the end of 1995 in Compustat. Borrower characteristics prior to violations are obtained from Compustat and CRSP. The majority of the 32 banks in our sample are included in Kho, Lee, and Stulz (2000), who manually examine

banks' material exposure to a set of crises in the end of 1990s, including those in South Korea and Southeast Asia (collectively "Asian crisis"), as well as Russia, Brazil, Latin America and the Long-term Capital Management Crisis. We use their table to determine the exposure of the banks in our sample. For those banks in our sample but not on the Kho, Lee, and Stulz (2000) list, we follow their approach and use bank public filings in 1998 and 1999 to determine exposure to the crises. We exclude from our sample all covenant violations if the lender is exposed to the Asian crisis but not the Russian crisis in order to avoid contamination.

Our main outcome variable, a dummy for "stringent action," comes from directly reading the 10-K or 10-Q in which the violation is first disclosed. We check subsequent filings if the current filing does not disclose the final outcome of borrower/lender negotiations. Many violations in our sample are cured with a waiver, but in many cases, the waiver comes with conditions, which are sometimes lenient and sometimes stringent. We first code the actions into one of the following six categories: (1) Preemptive modification; (2) Waiver with amendments: lenient actions; (3) Waiver without amendments; (4) Waiver with amendments: unspecified; (5) Waiver with amendments: stringent actions; (6) No waiver. Appendix C provides details and examples of each category. The stringent dummy is set to 1 if the actions fall into category (5) or (6). In this step we also delete violations under non-bank-loan contracts or violations by unconsolidated subsidiaries.[11] We confirm lead arranger identity in this step and observations where lead arrangers cannot be identified are also deleted. In some cases, violations are disclosed in multiple filings so we exclude later duplicated cases if the description of the violation is exactly the same as that in the first filing. Stringent actions that we code as risk-reducing include those where the maturity is shortened, the loan amount is reduced, collateral is added, and other similar actions. We code stringent actions as income-extracting if they require paying fees to the lender or raising the interest rate. In some cases, both risk-reducing and income-extracting actions are imposed by the lender. After applying the above sample selection criteria, the final dataset contains 799 covenant violations by 456 unique

[11]Not only are non-bank loan covenant violations unrelated to bank health, but public debt covenants are fundamentally different from bank debt covenants. For further discussion, see Chava and Roberts (2008), Maxwell and Shenkman (2010), and Bao and Hou (2017).

borrowers who have loans outstanding from 32 unique bank holding companies between 1996 and 1999, inclusive.

In our tests for whether covenant violations have adverse real effects, we consider whether the firm suffers a distressed delisting. We classify delisting codes in the 400s (liquidations) as distressed delistings. To classify codes in the 500s, we start with performance-related delistings in Shumway (1997), but update the codes based on more recent classifications. In particular, we classify delisting code 500 and all codes from 520 to 591, with the exception of 573 (company request, gone private), 575 (company request, offer rescinded), and 588 (conversion of closed-end investment company to open-ended), as distressed delistings. The codes classified as distressed are indicative of poor financial conditions and include insufficient capital, price fell below acceptable level, insufficient float, delinquency in filing, non-payment of fees, and does not meet exchange's financial guidelines. We create a dummy variable that takes a value of one if the firm delisted for one of these adverse reasons within two years of the covenant violation, and takes a value of zero otherwise.

We use data from Compustat and CRSP to construct borrower control variables following Chava, Huang, and Johnson (2017). We include firm size, Market-to-Book, leverage, profitability, tangibility, modified Z-score, KZ index, distance-to-default, cash flow volatility, a dummy for the firm having lending relationship, and two measures of bank dependence: a dummy for the absence of a bond issuer rating and a dummy for whether the firm has bonds outstanding. Details of definitions are listed in Appendix B. All values of these variables are taken from the fiscal year just prior to the covenant violation. For some tests, we also use the lender tier 1 capital ratio as December 31, 1995, prior to the start of any of the crises in question. Since the capital ratio is likely to more strongly influence bank behavior when the lender is closer to the regulatory minimum, we use the following monotonic transformation in our regressions: we subtract 6%, the minimum well-capitalized threshold for US banks under Federal Deposit Insurance Corporation Improvement Act (FDICIA) as of 1991,[12] from the tier-1 ratio and take the natural logarithm.

[12] See van Roy (2008) for more details.

Descriptive statistics are in Table 3.1. Stringency is relatively common but is not the most common outcome, taking place 45% of the time. When banks act stringently, risk reduction is more common than income extraction. (The two dummy variable means sum to more than the mean for the stringent dummy because, in some instances, banks engage in both kinds of actions.) A majority of the covenant violations in our sample are associated with lenders exposed to the crises, as the mean of the Exposure Dummy is 0.691. However, we still have a significant number violations where the lenders are not exposed. About 75% of violators in our sample are bank dependent, as most do not have a credit rating and most do not have bonds outstanding. Pre-crisis, the banks in our sample were well-capitalized, with mean and median (untransformed) tier 1 capital ratios of over 8%, well above both the minimum required ratio of 4% under Basel I and the minimum ratio to be well-capitalized under FDICIA of 6%. In fact, even the worst-capitalized bank in our sample is above both minimums as of the end of 1995, at 6.51%.

Turning to the summary statistics of covenant violator characteristics, we note they are unremarkable in their size and market-to-book ratios. While the mean market-to-book is high, taking the value of 2.636, the median is only 1.252, roughly in line with the broader Compustat universe. The mean and median leverage are reasonable, at roughly 40%, in line with what is typical for public firms with bank debt outstanding. Likewise, our covenant violators are similarly unremarkable in their asset tangibility, the mean taking the value of approximately 0.3 in our sample. The summary statistics on Z-score, KZ index and distance-to-default all indicate the firms in our sample are less financially healthy than the average public firm, not surprising given that we are focused on firms that violate financial covenants on their loans.

3.5 Research Design

3.5.1 Natural experiment

Our primary goal is to determine whether lender health causally affects the resolution of covenant violations. Just showing that lender health is correlated with certain lender actions does not demonstrate that the effect is causal, due to the likely correlation between lender health and the

Table 3.1: Summary Statistics

This table reports summary statistics for the sample of covenant violations merged with borrower and lender characteristics. Borrower characteristics are measured at annual frequency. Definitions of variables are in Appendix B.

	Count	Mean	SD	Min	P25	P50	P75	Max
Covenant Cure Outcomes:								
Stringent Resolution	799	0.453	0.498	0	0	0	1	1
Risk Reduction	799	0.367	0.482	0	0	0	1	1
Income Extraction	799	0.223	0.416	0	0	0	0	1
Both Actions	799	0.136	0.343	0	0	0	0	1
Post Crisis Dummy	799	0.706	0.456	0	0	1	1	1
Exposure Dummy	799	0.691	0.462	0	0	1	1	1
Indicator for Lack of Bond Rating	799	0.748	0.434	0	0	1	1	1
Indicator for Bond Outstanding	799	0.249	0.433	0	0	0	0	1
Lender Capital Ratio_Tier1	799	8.086	1.250	6.510	7.240	8.110	8.220	14.380
Borrower Characteristics:								
Size	799	5.190	1.468	1.466	4.222	5.129	6.053	10.266
Market to Book	799	2.636	11.869	-47.819	0.710	1.252	2.255	284.658
Leverage	799	0.425	0.330	0	0.244	0.395	0.541	3.491
Profitability	793	0.044	0.170	-1.251	0.012	0.072	0.118	0.733
Tangibility	799	0.303	0.223	0	0.125	0.241	0.448	0.942
Modified Z-score	767	1.046	2.503	-26.407	0.521	1.384	2.053	8.430
KZ index	758	-1.194	25.052	-506.091	0.321	1.461	2.397	37.466
Distance to Default	776	1.462	2.582	-4.985	-0.109	1.111	2.559	21.529
Cash Flow Volatility	785	2.637	12.32	0.006	0.107	0.241	0.813	204.19
Dummy for Lending Relationship	799	0.318	0.466	0	0	0	1	1
Log Growth Rate of Asset, Debt, Leverage and CapEx:								
Asset Growth	571	-0.124	0.540	-2.752	-0.332	-0.123	0.096	3.320
Debt Growth	565	-0.034	0.349	-1.400	-0.151	-0.030	0.079	3.044
Leverage Growth	565	0.001	0.255	-1.110	-0.103	0.013	0.115	1.170
CapEx Growth	550	-0.017	0.138	-0.559	-.0.046	-0.013	0.003	1.836

ex-ante choice of riskier loans. Poor lender health could, in fact, be caused by deterioration in the credit quality of these riskier loans. In order to establish causality, we need an exogenous shock that impacts the health of some lenders, but not others, while at the same time having the same direct effect (or ideally, no effect) on firms borrowing from banks in both groups. To this end, we follow Chava and Purnanandam (2011) and Lo (2014) in exploiting the Russian Default of 1998, and the subsequent crises it triggered (i.e., the Brazilian, Latin American and that of Long-Term Capital Management), as an exogenous shock to bank health. While a bank's exposure to these crises is not exogenous, Chava and Purnanandam argue that the crises-triggering decision of the Russian government to default had little direct effect on U.S. borrowers since it had no measurable macroeconomic impact on the U.S. (unlike the Global Financial Crisis of 2007-2009). Hence exposure to Russian default (and the follow-on crises) likely caused changes in lender health in 1998 but not borrower health (except through the channel of lender health). As discussed below, we also empirically verify that covenant violators borrowing from exposed banks are themselves no more exposed to the countries experiencing the crises in the late 1990s than violators borrowing from unexposed banks. Specifically, we find covenant violator stock return comovement with the stock indices of the relevant countries is statistically the same for both groups. We can thus identify the causal impact of changes in lender health on changes in the manner in which lenders respond to covenant violations using a difference-in-differences analysis that uses covenant violation data from before the Russian default to just after.

It is useful to contrast bank exposure to the Russian and related crises with exposure to the Global Financial Crisis of 2007-2009. Because we can empirically rule out the proposition that the former crises had a direct (and differential) impact on borrower health, we can rule out the proposition that lenders will become stricter with borrowers during these crises due to borrowers becoming relatively less creditworthy. Even if lenders exposed to the Russian and related crises systematically made riskier loans, the crises did not make these loans any riskier. Hence a difference-in-differences test controls for these level differences, as long as the parallel trends assumption holds. The Global Financial Crisis, in contrast, presents a clear case where many borrowers were

directly impacted and parallel trends are likely violated. Recent papers (e.g., Bhattacharyya and Purnanandam (2011) and Kolasinski and Yang (2018)) find that banks that performed worse during the Global Financial Crisis systematically made riskier investments before the crisis. Fahlenbrach, Prilmeier, and Stulz (2012) and Ellul (2015) discuss the impact of bank risk-taking culture, suggesting that there is a bank-level risk culture set by senior management. Given the large scale of the Global Financial Crisis, nearly all borrowers were also impacted, and it is very likely that the riskiest borrowers were impacted the most. Hence, the least healthy banks in the financial crisis were also the most likely to have made loans to borrowers with the greatest deterioration in credit quality during the Global Financial Crisis. Therefore, even without a lender health channel, one would find that lender health was negatively correlated with the strictness of covenant violation resolutions in the Global Financial Crisis due to the borrower health channel. It is thus impossible to disentangle a lender health effect from a borrower health effect during the Global Financial Crisis.

As Lo (2014) points out, there were other crises occurring around the same time as the Russian default that impacted US banks. Before the Russia crisis, there was the Asian crisis, that began in Southeast Asia in July of 1997, soon spreading to South Korea and continuing into 1998. We do not consider this crisis in our main analysis because its effect on U.S. banks was relatively small,[13] but we are careful to ensure it does not contaminate our results. We thus define our pre-crisis period to include the second half of 1996 and the first half of 1997, ending it just before the Asian crisis. To further prevent the Asian crisis from contaminating our results, we exclude from the analysis any covenant violations by firms indebted to banks exposed to the Asian crisis, but not the Russian crisis. Since the Russian default occurred mid-way through the third quarter of 1998, and the LTCM crisis occurred in September (all LTCM-exposed banks were also Russia-exposed), we begin our post-crisis period in the fourth quarter of 1998. Finally, in January of 1999 there occurred the Brazilian currency crisis, which soon thereafter spread to the rest of Latin America. However,

[13] Lo (2014) finds that the spread between C&I loan rates and the Fed Funds rate were essentially unchanged from the start of the Asian crisis until the start of the Russian crisis. With the start of the Russian crisis, this spread increased dramatically.

the set of banks exposed to this crisis is largely the same as that exposed to the Russia crisis, so we are not concerned about this crisis contaminating our results. Nevertheless, we exclude from our analysis covenant violations by firms whose lenders were exposed to Brazil or Latin America but not to Russia. We thus are able to treat the Russian, LTCM, Brazilian and Latin American crises of 1998-1999 as one crisis, which we together call "the emerging market crises."

Our baseline difference-in-differences regressions, using data from Q3 1996-Q2 1997 and Q4 1998-Q4 1999, are of the form,

$$\begin{aligned}\text{Stringent Action}_i = \alpha_j + \beta_1 \text{Exposure}_j \times \text{Post} + \beta_2 \text{Post} \\ + \beta_3 \text{Borrower Variables}_i + \beta_4 \text{Controls} + \varepsilon_i\end{aligned} \tag{3.1}$$

where i indexes the covenant violation and j indexes the bank. *Exposure* is a dummy variable indicating at least one lead arranger of the loan whose covenant is being violated was exposed to the crises.[14] *Post* takes the value of 1 if the covenant violation occurs in or after Q4 1998, and is zero otherwise. *Borrower Variables* and *Controls* are computed as of the end of the fiscal year prior to violation. We omit bank-specific variables from *Controls* because there is no time series variation in our specification, thereby ensuring all lender-specific variables are captured by bank fixed effects. We also do not include *Exposure* as a separate independent variable because it is perfectly collinear with the bank fixed effects. A positive coefficient, β_1, on the interaction between *Crisis Exposure* and *Post* would indicate that exogenous shocks to bank health cause banks to become more stringent in their responses to covenant violations, supporting Hypothesis 1.

3.5.2 Identification concerns

Our identification strategy is a standard difference-in-differences regression, where the key assumption is parallel trends.[15] The interaction specification with bank fixed effects compares the

[14] Out of the borrower-lender mapping of all violations in our final sample, 97.5% of the cases have a single lead arranger. The remaining 2.5% have two lead arrangers and there are no cases with more than two lead arrangers.

[15] See Cameron and Trivedi (2005) and Angrist and Pischke (2009).

effect of the crisis on changes in exposed bank behavior relative to the effect on changes in non-exposed bank behavior. Because it focuses on the differential impact of the crises on changes (between the pre- and post-period) rather than levels, and because the crises did not directly impact the two groups of borrowers, the method remains valid even if exposed and unexposed banks and their borrowers differ in fundamental ways (e.g., exposed banks' borrowers are riskier). The method is also robust to the occurrence of other contemporaneous systematic shocks impacting either banks and borrowers, so long as these shocks have the same effect on crises-exposed and non-exposed banks and their borrowers. The particular identifying assumption that our application of difference-in-differences requires is that nothing other than bank losses due to the Russia default and the crises it triggered impacted crises-exposed banks and their borrowers differently than unexposed banks and their borrowers. Thus, it is important to rule out two possible ways in which these identifying assumptions might be violated: (1) other confounding events or (2) differential borrower exposure to the exogenous shock we exploit in our experiment. We presently explain why we do not think either violation occurs.

First, there might be some other confounding event contemporaneous to the crises that impacted the exposed banks or their borrowers differently than it did non-exposed banks or their borrowers. To our knowledge, however, there is no such event. While multiple crises occurred around the same time, the Russian crisis was the largest one and the crises that followed it were triggered by it. In order to ensure the crises that came before the Russian default (Southeast Asia and South Korea) do not contaminate our inferences, we exclude covenant violations from our sample where the lender was exposed to one of the these crises but not the Russian crisis. We also exclude from the sample all violations that overlapped with the Asian and Korean crises but not the Russian crisis (the other crises all follow the Russian crisis). Furthermore, all of the crises are just as unlikely to affect borrowers in ways other than through its effect on their lenders. Thus we should be able to construct our control sample and define our pre-and-post periods in such a way that other crises will not confound our inferences.

Second, one could conjecture that covenant violators indebted to banks exposed to Russia,

Brazil, and Latin America were impacted differently, through channels other than through their lenders' health, than were violators indebted to non-exposed banks. Hence exposed banks, relative to unexposed banks, might have more greatly increased their propensity to act stringently against covenant violators not because of the banks' losses from the crises, but because the crises directly impacted their covenant-violating borrowers' health more than that of the non-exposed banks. We address the possibility of systematic differences in covenant violator direct exposure to Russia or the other crises in our treatment and control groups in three ways: (1) a standard pre-event parallel trends analysis, (2) by directly comparing stock market exposure of covenant violators to crisis countries, and (3) by running placebo tests. In this section, we discuss the first two approaches, and we discuss the third approach below, following the presentation of our main results.

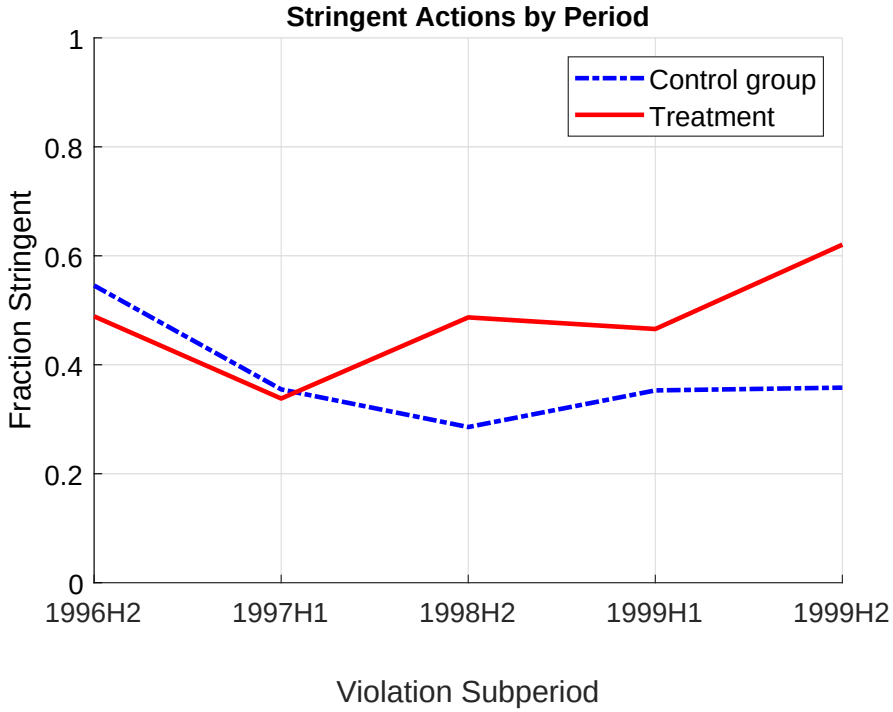
We start with a pre-period parallel trends analysis. In Table 3.2, we compute, separately, the frequency with which covenant violations are resolved stringently by exposed and unexposed banks for, separately, the two six month periods in our pre-period (Q3-Q4 1996 and Q1-Q2 1997). We then compare the changes in frequency of stringent resolution across the two periods for exposed and unexposed banks. As can be seen in Table 3.2, the two changes are statistically indistinguishable from each other, consistent with the parallel trends assumption. Figure 3.2 illustrates the same point in graphical form.

Table 3.2: Pre-event Parallel Trends Analysis

This table reports results of pre-event parallel trend analysis. Specifically, it reports differences in means between treatment and control groups, for change in the portion of stringent resolution from 1996H2 to 1997H1. Treatment group consists of covenant violators indebted to exposed banks, and control group consists of violators indebted to unexposed banks. 1996H2 refers to the last two quarters of 1996 and 1997H1 refers to the first two quarters of 1997.

No. of violations		
	1996H2	1997H1
Unexposed	33	31
Exposed	90	71
% stringent resolution		
	1996H2	1997H1
Unexposed	54.5%	35.5%
Exposed	48.9%	33.8%
	diff-in-diffs	4.0%
	t-stat	0.270

Figure 3.2: Parallel Trends



Proportion of covenant violations resolved in a stringent manner for loans affected by the Emerging Market Debt Crises (Treatment) and banks not affected (Control). 1996H2 and 1997H1 are the pre-treatment period. The treatment period is 1998H2–1999H2.

A key concern in studying the lender health channel is that the results are confounded by the well-documented borrower health channel. Parallel trends could be violated if borrowers from exposed banks were themselves more exposed to Russia and the other crisis countries than borrowers from unexposed banks. This could then create a difference in the stringency of covenant violation resolutions due to differences in borrower health. To rule out this possibility, we construct an equity portfolio that is long the covenant violators indebted to the exposed banks and short violators indebted to the unexposed banks for the period of January 1994 to December 2001. We then regress the time series of monthly returns on this portfolio on the Carhart (1997) four-factor model

plus index returns for crisis countries and regions,

$$R_{\text{exposed},t} - R_{\text{unexposed},t} = \alpha + \beta(R_{m,t} - r_{f,t}) + sSMB_t + hHML_t + uUMD_t \qquad (3.2)$$
$$+ \gamma R_{\text{crisis country index},t} + \varepsilon_t.$$

We use the Russia, Korea, and Brazil national indices, as well as the Southeast Asian and Latin American indices for those broader crisis regions. We obtain all indices from MSCI. If covenant violators indebted to banks exposed to the crises had greater exposure to crisis countries and regions, we would expect that this would be reflected in relatively stronger comovement with crisis country and region stock returns, i.e., $\gamma > 0$.

It is true that the lender channel, by itself, could, in principle, cause greater return comovement with crisis countries for borrowers indebted to exposed banks. The crises in question, however, were discrete events whose consequences for U.S. banks were revealed on only a handful trading days. Hence if the lender channel were the only source of borrower exposure to these countries, we would only expect comovement with crisis countries, for borrowers of exposed banks, on the handful of days where news of bank losses from the crises is revealed. We would not expect greater comovement for the vast majority of trading days over the last half of the 1990s, on which no such news is revealed. Hence the lender channel is unlikely to affect borrower partial correlations in lower frequency (monthly) returns with crisis countries over this sample period. On the other hand, direct borrower business exposure to crisis countries would result in significantly greater comovement.[16] Hence a comparison of return comovement with crisis countries provides direct evidence on the parallel trends assumption.

In Table 3.3, we report the results from our long-short portfolio regressions. In the first five columns of the table, we report regressions that include each country or region's index return (Southeast Asia, Korea, Russia, Brazil, and Latin America) one at a time with the Carhart factors.

[16]The potential exposure of covenant violators to Russia would likely be different than the exposure of banks. Banks made bets on Russian sovereign debt, which had a few discrete and large declines in value. Covenant violators, on the other hand, were unlikely to have made such bets. Exposures, if they existed, would be likely to be long-term business exposures.

Of the five country and region indices we consider, none are statistically significant. The economic magnitudes of the coefficients on the country and region indices returns are small, with the largest coefficient being 0.039. When we include all country and index returns together, we continue to find little evidence that borrowers from exposed banks are more exposed to the crises than borrowers from unexposed banks as all of the coefficients on country and region index returns continue to remain both economically and statistically insignificant. Thus, violator exposure to Russia and other crisis countries is unlikely to be contaminating our analysis, suggesting that unlike work using the Global Financial Crisis, our estimates of the lender health treatment effect are unlikely contaminated by a borrower health effect.

3.6 Empirical Results

3.6.1 Covenant violation resolutions

To test Hypothesis 1, that banks are more likely to be stringent with violators when their own financial health is poorer, we run our baseline difference-in-differences specification (equation (3.1)) using a linear probability model where the dependent variable is a dummy for stringency. We code actions as stringent where lenders reduce risk or extract additional income from borrowers. The former typically involves actions such as reducing credit availability, requiring collateral, or the imposition of new financial covenants. The latter often involves charging fees or increasing the interest rate on loans. We compute standard errors by clustering by lender, thereby making them robust to heteroskedasticity and arbitrary residual correlation among all covenant violations associated with the same lender.[17] In column (1) of Table 3.4, we find that relative to non-exposed banks, banks exposed to the Emerging Market Crises become 23.6 percentage points more likely to act stringently with covenant violators in the post-period. This is a large economic effect compared to the 45% probability of stringent actions in the full sample.

In columns (2) to (4), we consider whether exposed banks are more likely to act stringently through forcing risk reduction, income extraction, or both. Ex-ante, both risk reduction and income extraction are possible. Banks that suffer a large loss due to the Emerging Market Debt Crises

[17] Standard errors are little changed if we two-way cluster by lender and borrower.

Table 3.3: Borrowers' Exposure to Crisis

This table presents results from a regression of covenant violators long-short portfolio stock returns on Fama-French four factors and emerging market index returns, as defined in equation (3.2). The portfolio is constructed as long violators indebted to exposed banks and short violators indebted to unexposed banks. The portfolio is equal weighted and returns are measured at a monthly frequency. Emerging market indices are from Datastream for the period 12/31/1993 to 12/31/2001. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Excess Return on the Market	0.063	0.084	0.115	0.133	0.201**	0.175
	(0.62)	(1.06)	(1.19)	(1.50)	(2.04)	(1.47)
Small-Minus-Big Return	0.136	0.145*	0.146*	0.152*	0.183**	0.192**
	(1.61)	(1.84)	(1.72)	(1.90)	(2.21)	(2.06)
High-Minus-Low Return	0.295**	0.328***	0.316***	0.299***	0.338***	0.396***
	(2.45)	(2.93)	(2.65)	(2.72)	(3.05)	(3.04)
Momentum Factor	-0.023	-0.015	-0.032	-0.036	-0.055	-0.025
	(-0.40)	(-0.27)	(-0.55)	(-0.66)	(-1.01)	(-0.41)
Southeast Asia MSCI Return	0.039					0.034
	(1.03)					(0.79)
Korea MSCI Return		0.030				0.032
		(1.32)				(1.26)
Russia MSCI Return			0.001			0.006
			(0.09)			(0.31)
Brazil MSCI Return				-0.021		-0.019
				(-0.76)		(-0.21)
Latin America MSCI Return					-0.054	-0.059
					(-1.24)	(-0.44)
Constant	-0.006	-0.005*	-0.007*	-0.005	-0.005	-0.006*
	(-1.55)	(-1.71)	(-1.83)	(-1.54)	(-1.50)	(-1.67)
N	84	96	84	96	94	82
Adjusted R^2	0.048	0.049	0.035	0.037	0.063	0.063

Table 3.4: Probability of Stringent Resolution

This table displays results of the baseline linear probability model defined in equation (3.1). In column (1) the dependent variable is one if a covenant violation has a stringent resolution and zero otherwise. In column (2), the dependent variable is set to one if the violation resolution is in the category of “Risk Reduction" and zero otherwise. In column (3), the dependent variable is set to one if the violation resolution is in the category of “Income Extraction" and zero otherwise. In column (4), the dependent variable is set to one if both actions are taken to resolve a violation and zero otherwise. Borrower characteristics are defined in Appendix B. Standard errors are clustered by lenders. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1) Stringent Resolution	(2) Risk Reduction	(3) Income Extraction	(4) Both Actions
Post * Exposure	0.236**	0.153	0.164*	0.081
	(2.28)	(1.65)	(1.92)	(1.20)
Post Crisis Dummy	-0.135	-0.093	-0.078	-0.036
	(-1.44)	(-1.08)	(-1.25)	(-0.67)
Indicator for Lack of Bond Rating	-0.045	-0.047	0.147**	0.145*
	(-0.62)	(-0.59)	(2.18)	(1.88)
Indicator for Bond Outstanding	0.003	0.035	0.060	0.092
	(0.09)	(1.06)	(0.75)	(1.51)
Size	-0.005	-0.006	0.031**	0.029***
	(-0.18)	(-0.31)	(2.67)	(3.00)
Market to Book	-0.001	-0.001	-0.000	0.000
	(-1.25)	(-0.66)	(-0.27)	(0.34)
Leverage	-0.147	-0.155	-0.038	-0.047
	(-1.02)	(-1.21)	(-0.37)	(-0.52)
Profitability	0.376**	0.342**	0.318***	0.285***
	(2.36)	(2.09)	(3.04)	(2.96)
Tangibility	-0.342**	-0.404***	-0.089	-0.151**
	(-2.59)	(-3.70)	(-1.11)	(-2.37)
Modified Z-score	-0.033	-0.039*	-0.011	-0.018**
	(-1.59)	(-1.97)	(-1.31)	(-2.07)
KZ index	0.001***	0.001***	0.001	0.001
	(3.14)	(3.03)	(1.69)	(1.68)
Distance to Default	-0.047***	-0.041***	-0.026***	-0.020***
	(-4.92)	(-4.54)	(-3.48)	(-2.97)
Cash Flow Volatility	0.001	-0.001	0.002	-0.000
	(0.55)	(-1.00)	(1.18)	(-0.04)
Dummy for Lending Relationship	0.044	0.023	0.069	0.048*
	(1.16)	(0.86)	(1.57)	(1.92)
Lender Fixed Effect	Yes	Yes	Yes	Yes
N	707	707	707	707
Adjusted R^2	0.093	0.099	0.056	0.041

could potentially choose to reduce risk in loans given to covenant violators as part of an effort to improve risk management and prevent future losses in their loan portfolios. Alternatively, banks that suffer large losses could also seek to extract income from covenant violators in an effort to partially offset losses. In column (2), we find an economically large 15.3 percentage point greater increase in the probability that banks exposed to the crisis take risk-reducing actions, relative to unexposed banks. In column (3), we find a similar economic effect on income extraction actions. While individually the risk reduction effect is marginally insignificant and the income extraction effect is significant at the 10% level, they are jointly significant at the 10% level. We also cannot rule out that the two effects are equal, which implies that bank health has similar effects on the odds of the bank imposing risk-reducing and income-extracting measures on covenant violators. Finally, in column (4), we find that there is an insignificant increase for exposed lenders, relative to unexposed lenders, simultaneously acting stringently, in imposing both a risk-reducing and an incoming extracting resolution, in the post period. Overall, our results are supportive of Hypothesis 1, that poorer bank health causes banks to be more stringent with covenant violating borrowers.

In order to provide a third and final test validating the parallel trends assumption (as discussed in Subsection 3.5.2), we run a placebo test for the specifications discussed above. That is, we re-run specifications identical to those above, except we limit the sample to covenant violations over the Q3 1996 through Q2 1997, and we designate the first two quarters of 1997 as the post-event period. Since no event expected to differentially impact exposed and unexposed banks actually occurred during this period, the parallel trends assumption implies that the coefficients on the terms interacting the post-event dummy with the exposure dummy should be statistically indistinguishable from zero. As can be seen from the results in Table 3.5, this is precisely what we find. The coefficient on the interaction term is negative and insignificant, in contrast to the positive and significant coefficient in Table 3.4.

Next, we test for heterogeneity in the manner in which banks exposed to the Emerging Market Debt Crises act more stringently. It is possible that banks with lower (log-transformed) tier 1 capital ratios could choose to become more stringent because the debt crisis pushed them closer to

Table 3.5: Probability of Stringent Resolution–Placebo Test

This table shows results from the placebo test using linear probability model defined in equation (3.1). Regression specifications are similar to Table 3.4, except the sample period. For the placebo test, 1996Q3 and 1996Q4 are defined as pre-period, and 1997Q1 and 1997Q2 as post-period. Both periods are prior to real treatment. Other variables are defined in Appendix B. Standard errors are clustered by lenders. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1) Stringent Resolution	(2) Risk Reduction	(3) Income Extraction	(4) Both Actions
Placebo Post * Exposure	-0.072	0.069	-0.118	0.023
	(-0.65)	(0.61)	(-1.44)	(0.63)
Placebo Post	-0.025	-0.146	0.062	-0.058**
	(-0.33)	(-1.58)	(1.51)	(-2.57)
Indicator for Lack of Bond Rating	-0.086	-0.117	0.179**	0.149***
	(-0.96)	(-1.01)	(2.32)	(3.14)
Indicator for Bond Outstanding	-0.104	-0.022	-0.036	0.046
	(-1.33)	(-0.28)	(-0.92)	(1.42)
Size	-0.004	0.006	0.003	0.013
	(-0.13)	(0.19)	(0.08)	(0.62)
Market to Book	-0.007	-0.014	0.002	-0.005*
	(-0.82)	(-1.60)	(0.47)	(-1.90)
Leverage	0.072	0.062	-0.003	-0.013
	(0.36)	(0.43)	(-0.02)	(-0.15)
Profitability	0.208	0.073	0.199	0.064
	(0.68)	(0.31)	(1.22)	(0.56)
Tangibility	-0.336	-0.301	-0.222	-0.188
	(-1.23)	(-1.26)	(-1.29)	(-1.28)
Modified Z-score	-0.038*	-0.046**	-0.004	-0.012
	(-1.72)	(-2.29)	(-0.23)	(-0.97)
KZ index	0.001**	0.001**	0.000**	0.000
	(2.41)	(2.31)	(2.09)	(1.44)
Distance to Default	-0.010	-0.003	-0.006	0.001
	(-0.53)	(-0.15)	(-0.35)	(0.06)
Cash Flow Volatility	0.017	0.005	0.016*	0.005
	(1.42)	(0.51)	(2.08)	(0.99)
Dummy for Lending Relationship	-0.058	-0.048	0.031	0.041
	(-0.65)	(-0.49)	(0.53)	(0.56)
Lender Fixed Effect	Yes	Yes	Yes	Yes
N	208	208	208	208
Adjusted R^2	0.070	0.061	0.110	0.078

their regulatory constraints.[18] Alternatively, one might expect that banks with higher tier 1 capital ratios have a more conservative culture and would act stringently in order to reduce their risk exposure.[19] Thus, the effect of a bank's capital ratio is an empirical question and we test it using a triple-interaction specification,

$$\begin{aligned}\text{Stringent Action}_i = \alpha_j + \beta_1 \text{Exposure}_j \times \text{Post} \times \text{Trans Lender Capital Ratio}_j \\ + \beta_2 \text{Exposure}_j \times \text{Post} + \gamma \text{Controls}_i + \varepsilon_i,\end{aligned} \tag{3.3}$$

where the controls include the appropriate level and double interaction terms. Standard errors are computed as before. In column (1) of Table 3.6, we find that the triple interaction term, β_1 is negative and significant, consistent with changes in bank health having a stronger effect when banks are closer to their capital constraints (Tier 1 capital is lower).

Next, we test Hypothesis 2, the proposition that dependence of the covenant violating borrowers on bank debt enhances the lender health treatment effect. Covenant violators tend to be of low credit quality, and many low credit quality firms have significant heterogeneity in their debt structures (Rauh and Sufi (2010)). It is possible that borrowers that have the option to issue public debt could opt to borrow from the public market, instead of the bank, should the latter attempt to impose harsh terms. Knowing this, banks that are exposed may strategically choose to deal stringently with borrowers who are more dependent on bank loans. To test Hypothesis 2, we construct two measures of bank dependence. The first is a dummy variable that is equal to one if a borrower does not have an S&P bond issuer rating. Obtaining a credit rating is costly and unnecessary to obtaining a bank loan, so it follows that firms unable to access bond markets are unlikely to have a credit rating.[20] Our second measure is a dummy for whether the borrower actually has bonds outstanding. Our results, in columns (2) and (3), suggest that there is an insignificant difference

[18] Recall from Section 3.4 that we calculate the log transformed capital ratio as log(Capital Ratio - 6) because FDICIA defines 6% tier 1 capital as the threshold for a well-capitalized bank during our sample period.

[19] For example, Kolasinski and Yang (2018) argue that corporate culture may help to explain the short-termist actions that CEOs took with respect to subprime mortgages in the Global Financial Crisis.

[20] See Faulkender and Petersen (2006).

Table 3.6: Probability of Stringent Resolution–Triple Interaction

This table shows results of linear probability model as defined in equation (3.3), where the dependent variable is one if a covenant violation has a stringent resolution, and zero otherwise. The regression specifications are similar to column (1) of Table 3.4, but with triple interaction terms. Borrower characteristics included are the same as in Table 3.4 and definitions are in Appendix B. The applicable double interaction terms are also included in the regression. Standard errors are clustered by lenders. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1)	(2)	(3)
Post * Exposure * Trans Lender Cap Ratio	-0.869***		
	(-3.81)		
Post * Exposure * No Bond Rating		-0.048	
		(-0.35)	
Post * Exposure * Bond Outstanding			-0.058
			(-0.33)
Post * Exposure	0.916***	0.284	0.220*
	(4.90)	(1.68)	(1.97)
Post Crisis Dummy	-0.803***	-0.142	-0.169
	(-4.45)	(-1.51)	(-1.55)
Indicator for Lack of Bond Rating	-0.041	-0.222*	-0.026
	(-0.57)	(-1.80)	(-0.35)
Indicator for Bond Outstanding	-0.002	-0.011	-0.074
	(-0.06)	(-0.37)	(-0.83)
Other Double Interactions	Yes	Yes	Yes
Borrower Characteristics	Yes	Yes	Yes
Lender Fixed Effect	Yes	Yes	Yes
N	707	707	707
Adjusted R^2	0.102	0.095	0.098

in the change of how exposed banks resolve covenant violations for borrowers with and without access to public debt. Thus, we fail to find evidence in support of Hypothesis 2.

Overall, our results show that there is a causal lender health channel in the resolution of financial covenant violations. Lenders exposed to unanticipated emerging market crises losses become relatively more stringent with their violators than do unexposed lenders. The treatment effect, however, is mitigated for lenders with high pre-crisis capital ratios. We infer that the closer a lender had been to its regulatory constraints before the crisis, the larger the effect of crises-linked losses on the lender's stringency. However, we do not find evidence that the lender health effect is particularly strong when borrowers do not have access to public debt.

3.6.2 Real effects

3.6.2.1 Delistings

To study real effects, we begin by examining whether covenant violators become relatively more likely to suffer a distressed delisting within two years of violation if their lenders are crisis-exposed. We use OLS to estimate a linear probability model,

$$\begin{aligned} \text{Delist}_i = \alpha_j + \beta_1 \text{Exposure}_j \times \text{Post} + \beta_2 \text{Post} \\ + \beta_3 \text{Borrower Variables}_i + \beta_4 \text{Controls} + \varepsilon_i, \end{aligned} \tag{3.4}$$

where *Delist* is a dummy variable taking the value of one if the CRSP database indicates the firm suffered a distressed delisting within two years of the covenant violation, and is zero otherwise. All other variables are defined as before. Also as before, we include lender fixed effects and cluster our standard errors by lender.[21] A positive coefficient on the interaction term would imply that lender health causes covenant violations to become more likely to result in a distressed delisting. The results are in column (1) of Table 3.7. Consistent with Hypothesis 3, the coefficient on the interaction term is positive and statistically significant. The point estimate of 0.179 implies that having a lender exposed to the crisis increases a covenant violator's probability of distressed

[21] Two-way clustering by lender and borrower gives similar standard errors.

delisting within two years of the violation by 17.9 percentage points, an economically large effect. For comparison, the unconditional distressed delisting probability for all violators in our sample is 28.5%, whereas it is 3.2% for the average firm in the average year in the CRSP universe.

We next consider specifications with triple interaction terms analogous to those reported in Table 3.6. These specifications test whether bank capital ratios or borrower bank dependence augment the treatment effect of lender health on covenant violator distressed delisting probability. The results are in columns (2) through (4). We find a significant triple interaction term with the lender capital ratio. The results suggest that a higher lender capital ratio mitigates the lender health channel on delistings. In particular, at the 25th percentile of lender capital ratios, the treatment effect is 0.412 (*t*-stat = 2.74), while at the 75th percentile, the effect is 0.188 (*t*-stat = 2.35).[22] In columns (3) and (4), the two regressions that assess whether there is heterogeneity in the effect due to bank dependence, neither of the triple interaction terms is significant. This suggests that there is no statistically significant difference in the treatment effect between bank dependent and non-bank dependent covenant violators.

Our results, thus far, attribute poor lender health to causing greater rates of delistings for covenant-violating borrowers. The results, however, do not preclude the possibility that there is a similar effect for other borrowers from the same lender in poor health. That is, there may be a lender health effect that does not flow directly through a covenant violation channel. Here, we turn to testing whether the covenant violation itself is a cause for delisting (Hypothesis 4). An ideal test would be a formal regression discontinuity design (RDD), where we would compare the treatment effect of lender health for borrowers who barely violated covenants with borrowers who were just able to avoid violating covenants. The difficulty in implementing such a method is that it requires clear knowledge of where the covenant thresholds are. Unfortunately, Dealscan only provides thresholds at the initiation of a loan, whereas recent literature (Denis and Wang (2014) and Roberts (2015)) has shown that covenants are frequently amended. While collecting precise covenant thresholds over time is possible, it is very costly as they are often reported not in the

[22]The calculations are $-0.386 \times \ln(X - 6) + 0.495$, where X is the capital ratio, in percent.

Table 3.7: Probability of Distressed Delisting

This table shows the delisting results as discussed in Section 3.6.2.1. Column (1) corresponds to the linear probability model defined in equation (3.4), where the dependent variable equals one if a borrower suffers a distressed delisting within two years of covenant violation, and is zero otherwise. Column (2)-(4) each includes a triple interaction term and applicable double interaction terms. Borrower characteristics included are the same as in Table 3.4 and definitions are in Appendix B. Standard errors are clustered by lenders. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1)	(2)	(3)	(4)
Post * Exposure * Trans Lender Cap Ratio		-0.386*		
		(-1.71)		
Post * Exposure * No Bond Rating			0.159	
			(1.56)	
Post * Exposure * Bond Outstanding				-0.041
				(-0.23)
Post * Exposure	0.179**	0.495**	0.079	0.199*
	(1.98)	(2.53)	(0.58)	(1.95)
Post Crisis Dummy	-0.099	-0.435**	-0.110	-0.090
	(-1.38)	(-2.56)	(-1.46)	(-1.04)
Indicator for Lack of Bond Rating	0.012	0.014	-0.197*	0.016
	(0.15)	(0.17)	(-2.04)	(0.19)
Indicator for Bond Outstanding	0.084	0.082	0.076	0.262***
	(1.16)	(1.15)	(1.01)	(3.25)
Other Double Interactions	No	Yes	Yes	Yes
Borrower Characteristics	Yes	Yes	Yes	Yes
Lender Fixed Effect	Yes	Yes	Yes	Yes
N	707	707	707	707
Adjusted R^2	0.134	0.135	0.143	0.135

main text of SEC filings, but instead require reading exhibits containing original loan contracts and amendments. For example, Denis and Wang (2014) manually collect data on debt covenant renegotiations for 1,000 randomly selected loans. The manual collection in our case would be significantly more labor intensive, as we would not only have to collect the history of renegotiations for the violators in our sample (to determine whether they were only “barely" violators), but also for a substantial sample of non-violators (as we do not perfectly know, ex-ante, which non-violators are close to violation).

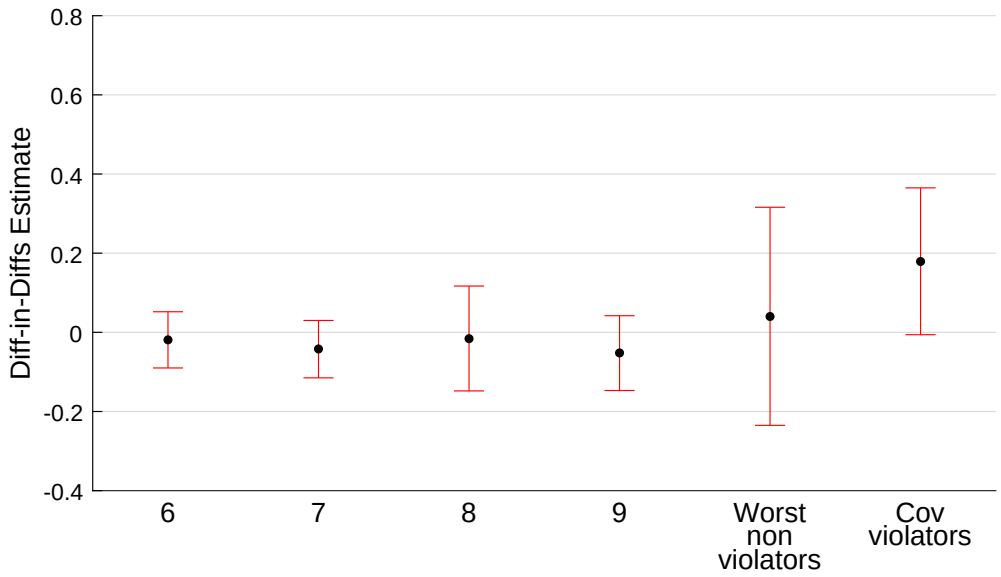
To provide evidence on Hypothesis 4, we instead design a test based on sorts of firm financial ratios commonly referenced by financial covenants to get a sense of whether the delisting effects (Table 3.7) are specific to covenant violators or are also observed in firms that are more likely to be near violators of financial covenants. For all firms indebted to a bank in our sample, for each firm-quarter, we compute their financial ratios that are used in the ten most common financial covenants in Dealscan. We follow Demerjian and Owens's 2016 standard definitions to calculate these ratios.[23] For each ratio, in each quarter, we sort observations for firms that have a covenant that references that ratio, into deciles.[24] Then for each firm-quarter observation, we impute to it its worst ratio decile across the 10 covenant ratios, provided the firm in that quarter has a covenant that references that ratio. That is, if a firm in a quarter has three financial covenants in place and the worst decile is for Debt/EBITDA (one of the three), then the firm's overall decile is the decile for Debt/EBITDA. We choose the worst decile because the worst financial ratio has the highest probability of triggering a violation. Next, we re-run the difference-in-differences regressions for non-covenant-violators in each of the five worst deciles and plot the difference-in-differences estimates in Figure 3.3.[25] For comparison purposes, we also plot the previously-reported estimates for violators. Our results suggest that the effect of lender health on non-covenant violators is

[23]The ten most common financial covenants are: Max. Debt to EBITDA; Max. Debt to Tangible Net Worth; Max. Leverage ratio; Max. Senior Debt to EBITDA; Min. Current Ratio; Min. Debt Service Coverage; Min. Fixed Charge Coverage; Min. Interest Coverage; Net Worth; Tangible Net Worth. We scale Net Worth and Tangible Net Worth by Book Assets to make them comparable across firms.

[24]Our results are qualitatively similar if we sort firms into 20 groups (ventiles) instead of deciles.

[25]As one might expect, firms in the better deciles have a paucity of delistings, making the difference-in-differences regressions largely uninformative for such deciles.

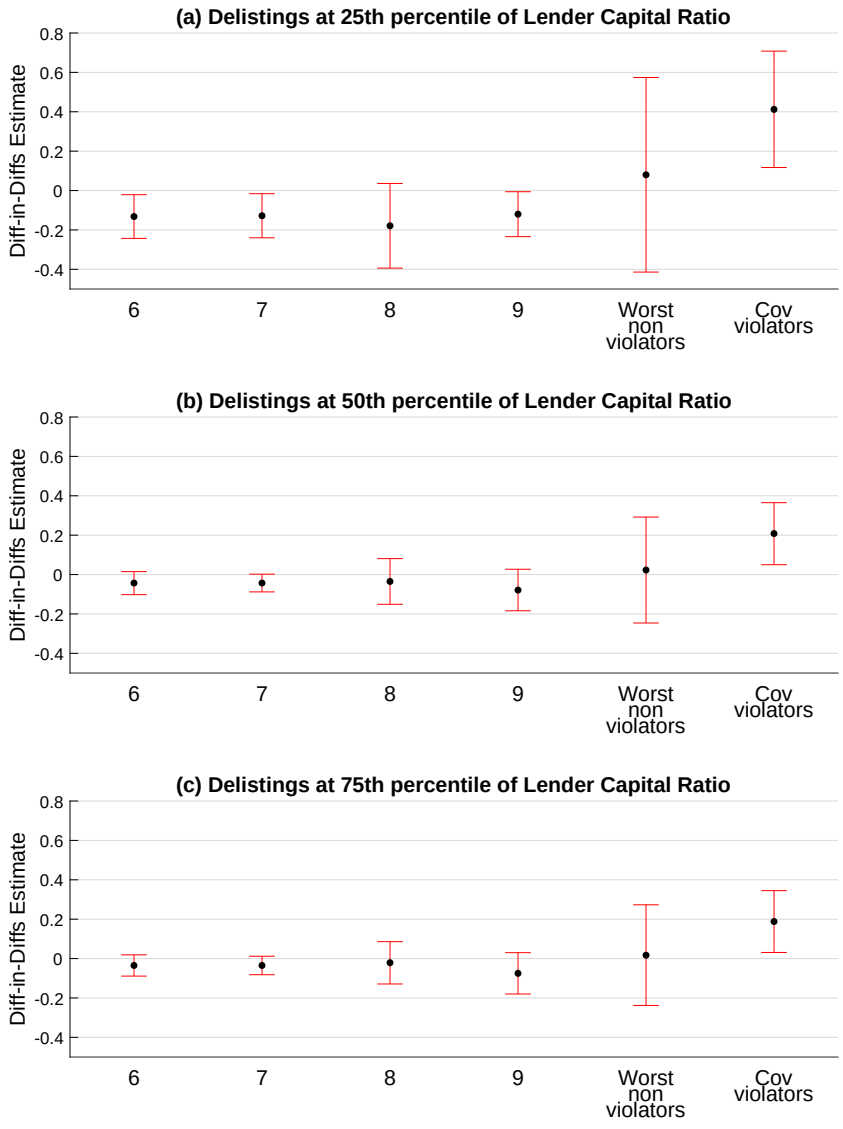
Figure 3.3: Delistings



Difference-in-differences estimates of the effect of lender health on delistings by group. Non-violators are first sorted into 10 groups based on estimates of how far they are to violating a covenant. Equation (3.4) is run group-by-group. The figure presents the difference-in-differences estimates for the five groups closest to violation and for covenant violators. The black dots are point estimates and the red bars, 95% confidence intervals. The five groups furthest from covenant violations are not displayed as they do not have a meaningful number of delistings.

statistically and economically insignificant, even for the worst non-covenant violators. In Figure 3.4, we consider the effect at different levels of lender capital ratios. We conclude that harsher covenant resolutions are the indeed channel through which the lender health treatment effect on delistings operates, as there is no detectable treatment effect for any group of non-violators, even those that are relatively close to violating covenants.

Figure 3.4: Delistings: by percentile of Lender Capital Ratio



Difference-in-differences estimates of the effect of lender health on delistings, interacted with log transformed lender capital ratios, by group. Non-violators are first sorted into 10 groups based on estimates of how far they are to violating a covenant. Equation (3.4), with a triple interaction term, is run group-by-group. The figure presents the difference-in-differences estimates at different levels of lender capital ratios for the five groups closest to violation and for covenant violators. The black dots are point estimates and the red bars, 95% confidence intervals. The five groups furthest from covenant violations are not displayed as they do not have a meaningful number of delistings.

3.6.2.2 Investment and financing decisions

Next, we consider the effects of lender health on the investment and financing decisions of covenant violators who survive. We run triple interaction specifications of the following form,

$$\log\left(1+\frac{Y_{i,t+1}-Y_{i,t-1}}{\text{Assets}_{i,t-1}}\right)=\alpha_j+\beta_1\text{Exposure}_j\times\text{Post}\times\text{Trans Lender Capital Ratio}_j \tag{3.5}$$
$$+\beta_2\text{Exposure}_j\times\text{Post}+\beta_3\text{Borrower Variables}_i+\beta_4\text{Controls}+\varepsilon_i$$

where $Y_{i,t+k}$ is the outcome variable for borrower i, k fiscal years after (or before) the covenant violation. As outcome variables, we use total borrower assets, debt, and capital expenditures.[26] Other variables are defined as before. As before, we estimate the equation using OLS and cluster standard errors by lenders. We also include lender fixed effects and all of the applicable double interaction and level terms. Our choice to take the log growth rate stems from the large positive skewness of growth rates, though tests using the growth rates directly give similar conclusions. We run separate specifications for each outcome variable. Our sample is limited to firms for which we can observe future assets, debt, and capital expenditures. Hence, our sample size shrinks from the previous tables because, as shown in Table 3.7, a number of firms delist. Our results, reported in Table 3.8, should be interpreted as the real effects on covenant violating firms conditional on survival.

[26] We also consider changes in leverage.

Table 3.8: Real Effects

This table reports regressions of real outcomes on borrower and lender characteristics, as defined in Equation (3.5). Columns (1), (5), and (9) present the log growth rate of assets, $\log\left(\frac{Assets_{t+1}}{Assets_{t-1}}\right)$. Columns (2), (6), and (10) present the log growth rate of debt as a fraction of assets, $\log\left(1+\frac{Debt_{t+1}-Debt_{t-1}}{Assets_{t-1}}\right)$. Columns (3), (7), and (11) present the log growth rate of leverage, $\log\left(1+\frac{Debt_{t+1}}{Assets_{t+1}}-\frac{Debt_{t-1}}{Assets_{t-1}}\right)$. Columns (4), (8), and (12) present the log growth rate of capital expenditures as a fraction of assets, $\log\left(1+\frac{CAPX_{t+1}-CAPX_{t-1}}{Assets_{t-1}}\right)$. Borrower characteristics included are the same as in Table 3.4 and definitions are in Appendix B. The applicable double interaction terms are also included. Standard errors are clustered by lenders. *t*-statistics are in parentheses. *,**, and *** indicate the corresponding *p*-values are less than 0.1, 0.05 and 0.01, respectively.

	(1) Asset growth	(2) Debt growth	(3) Leverage growth	(4) CapEx growth	(5) Asset growth	(6) Debt growth	(7) Leverage growth	(8) CapEx growth	(9) Asset growth	(10) Debt growth	(11) Leverage growth	(12) CapEx growth
Post * Exposure * Trans Lender Cap Ratio	1.349***	1.165***	0.256	0.893***								
	(9.86)	(7.41)	(1.51)	(7.47)								
Post * Exposure * No Bond Rating					-0.074	0.640***	0.659***	-0.072				
					(-0.32)	(3.52)	(6.03)	(-0.59)				
Post * Exposure * Bond Outstanding									-0.098	-0.351	-0.236	-0.003
									(-0.37)	(-1.69)	(-1.51)	(-0.04)
Post * Exposure	-1.088***	-0.836***	-0.129	-0.580***	0.045	-0.536***	-0.567***	0.219***	0.078	0.144	0.085	0.151
	(-9.73)	(-4.95)	(-0.68)	(-4.63)	(0.30)	(-2.93)	(-5.42)	(5.61)	(0.36)	(0.86)	(1.24)	(1.05)
Post Crisis Dummy	0.985***	0.669***	0.041	0.560***	-0.036	0.475**	0.496***	-0.221***	-0.083	-0.246	-0.153**	-0.157
	(8.31)	(4.13)	(0.22)	(4.51)	(-0.18)	(2.57)	(5.89)	(-5.68)	(-0.43)	(-1.64)	(-2.44)	(-1.12)
Indicator for Lack of Bond Rating	-0.065	0.066	0.134***	-0.007	0.149	0.812***	0.806***	-0.044	-0.070	0.078	0.141***	-0.004
	(-0.66)	(1.39)	(3.26)	(-0.83)	(0.55)	(3.90)	(9.30)	(-0.35)	(-0.64)	(1.68)	(3.65)	(-0.36)
Indicator for Bond Outstanding	-0.051	0.075	0.053*	-0.011	-0.053	0.072	0.058**	-0.017	0.057	-0.227	-0.243	-0.060
	(-0.63)	(1.40)	(1.79)	(-1.11)	(-0.68)	(1.42)	(2.13)	(-1.40)	(0.21)	(-0.87)	(-1.46)	(-0.60)
Other Double Interactions	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender Fixed Effect	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender	Lender
N	503	497	497	486	503	497	497	486	503	497	497	486
Adjusted R^2	0.190	0.309	0.085	0.231	0.164	0.275	0.093	0.057	0.171	0.270	0.090	0.057

As shown in columns (1), (2), and (4), lender health deterioration causes reductions in assets, debt, and the rate of capital expenditures at low levels of pre-treatment lender capital ratios. But, as shown by the positive triple interaction terms, these effects are mitigated as the pre-treatment lender capital ratio increases. At the 25th percentile of lender capital ratio, the parameters in the first column of Table 8 imply a point estimate of the treatment effect to be -0.798 (t-stat = -8.04),[27] which is large compared to the unconditional standard deviation of log asset growth of 0.54 among all covenant violators. If we exponentiate this point estimate and subtract one, we obtain -0.55 (t-stat = -12.30, using the delta method). This value implies that the negative shock to lender health we study, when the lender's capital ratio was previously 7.24%, exacerbates the negative effect of a covenant violation on borrower asset growth by 55 percentage points. The size of the treatment effects on log debt and CAPX growth is of a comparable magnitude. Column (3) shows that there is little effect on leverage, regardless of the level of lender capital ratios. This is consistent with proportional declines in assets and debt. Turning to the bank dependence regressions, inconsistent with Hypothesis 2, we fail to find evidence that bank dependence exacerbates any investment or financing treatment effects. In fact, for one of our bank dependence proxies, the lack of a credit rating, we actually find it mitigates the treatment effect on debt and leverage. This result is not robust, however, as we fail to find similar effects for our other proxy. Overall, our results are suggestive of real effects of lender health on borrower investment and financing decisions only if lenders are already close to their regulatory minimum capital ratios before the negative shock to their health.

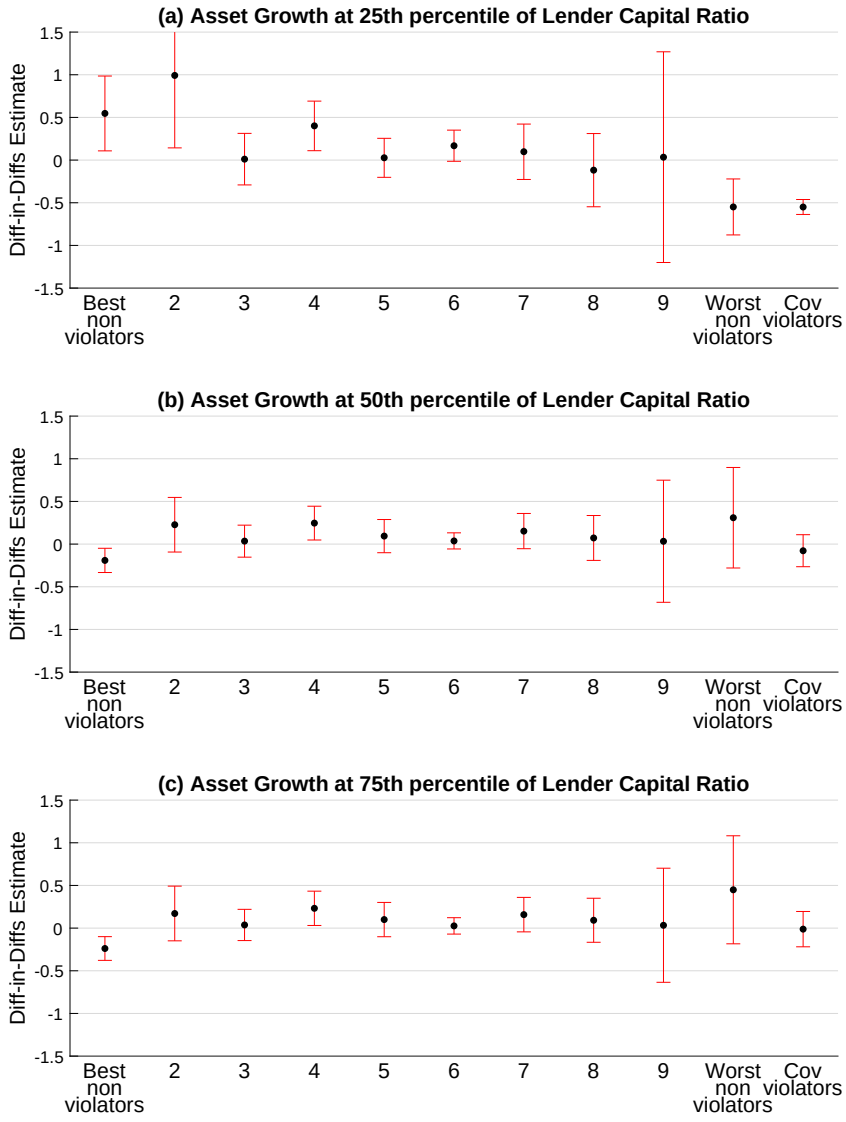
Finally, we examine whether the treatment effects on assets and capital expenditures of surviving borrowers that we find are unique to covenant violators, or whether we can find similar effects on non-violating borrowers in comparably poor financial health (Hypothesis 4). To that end, similar to our tests on the delisting outcome, we group all non-violating borrowers into 10 subsamples based on the decile of their worst financial ratio that is subject to a covenant. We then re-estimate equation (5) for each subsample. In Figures 3.5 and 3.6, we plot average treatment

[27] The calculation is $-0.798 = -1.088 + 1.349 \times \log(7.24 - 6)$.

effects for each non-violating borrower group, as well as for violators, for three different levels of the pre-treatment lender capital ratio, the 25th, 50th, and 75th percentiles. The treatment effects are statistically indistinguishable from zero for all groups, including that of violators, when the pre-crisis lender capital ratio is at the 50th or 75th percentile. When the lender capital ratio is at the 25th percentile, however, average treatment effects are statistically significant and economically large for both covenant violators and also for the worst non-violator group. If anything, the point estimate of the effect sizes is slightly larger for the worst non-violator group than it is for the violators, though the difference is small. Thus, we find that the effect of lender health on investment for struggling borrowers who do not violate covenants is just as large as it is on borrowers who violate.

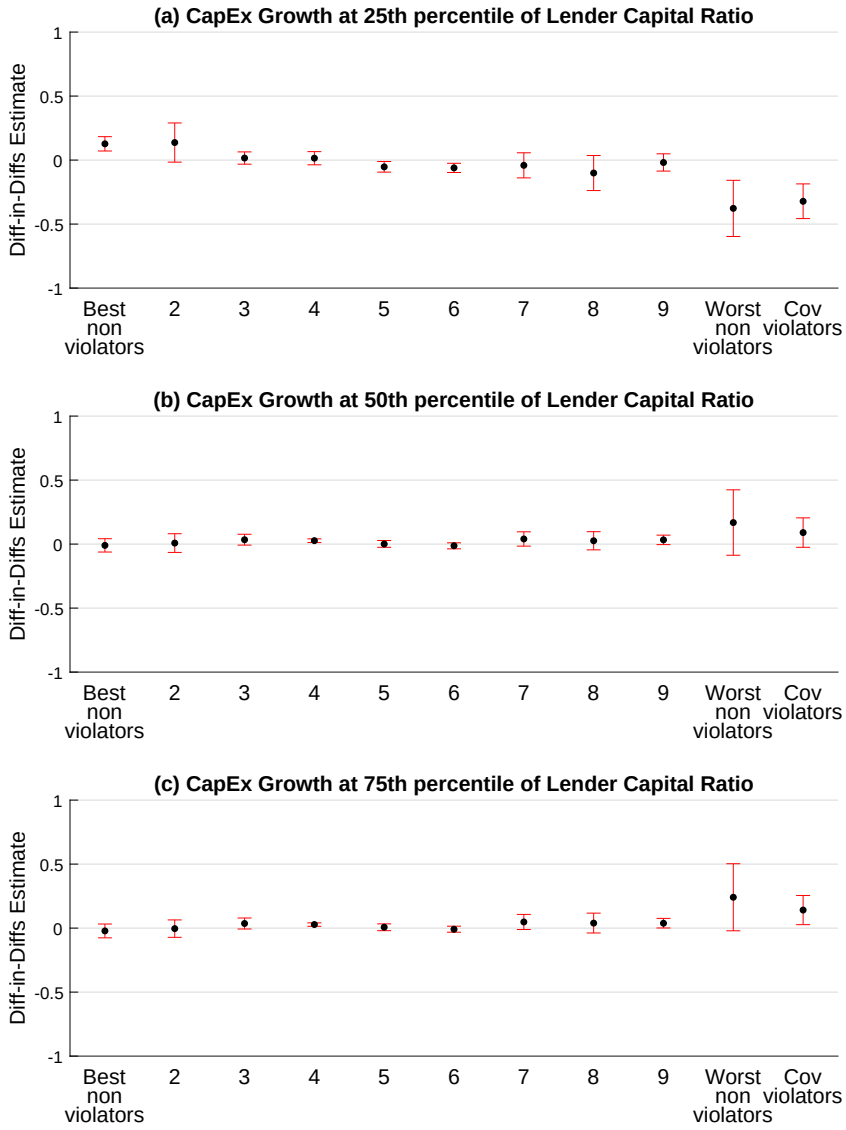
The last finding is consistent with the hypothesis that the lender health effect on violator investment, reported in Table 3.8, is due to treated lenders becoming harsher with all their struggling borrowers, regardless of violation status. It is also, however, consistent with the hypothesis that non-violating borrowers, who are close to violation, take more aggressive action to avoid future violations, in anticipation of their lenders becoming harsher on violators as a result of a shock to lender health. Notice, however, that we only find treatment effects for non-violator real outcomes that are under the borrower's control (investment). In Subsection 3.6.2.1 we found no detectable effect on the adverse outcome for troubled non-violators that are not directly under the borrower's control (distressed delisting). Therefore, taken as a whole, our results suggest the real treatment effects on non-violator investment that we find are more likely due to treatment causing troubled non-violators to take greater care to avoid future violations, rather than due to treated lenders becoming harsher with non-violating but troubled borrowers.

Figure 3.5: Asset Growth: by percentile of Lender Capital Ratio



Difference-in-differences estimates of the effect of lender health on asset growth by group. Non-violators are first sorted into 10 groups based on estimates of how far they are to violating a covenant. Equation (3.5) is run group-by-group. The figure presents the transformed difference-in-differences estimates at different levels of lender capital ratios, $\exp(\beta_1 \times \text{Transformed Lender Capital Ratio} + \beta_2) - 1$. Standard errors are calculated based on the delta method. The black dots are point estimates and the red bars, 95% confidence intervals.

Figure 3.6: Capital Expenditure Growth: by percentile of Lender Capital Ratio



Difference-in-differences estimates of the effect of lender health on capital expenditure growth by group. Non-violators are first sorted into 10 groups based on estimates of how far they are to violating a covenant. Equation (3.5) is run group-by-group. The figure presents the transformed difference-in-differences estimates at different levels of lender capital ratios, $\exp(\beta_1 \times \text{Transformed Lender Capital Ratio} + \beta_2) - 1$. Standard errors are calculated based on the delta method. The black dots are point estimates and the red bars, 95% confidence intervals.

3.7 Conclusion

The extent of the impact of adverse shocks to lender health on the borrowers is a matter of first order importance to academics and policymakers, particularly when debating the potential benefits of banking regulation. We study the impact of lender health on the resolution of financial covenant violations. Though the effect of borrower health on the resolution of covenant violations has been extensively studied, the causal effect of lender health has largely been ignored. Using the Emerging Market Crises of the late 1990s as an exogenous shock to bank health that had little to no impact on borrower health, a difference-in-differences framework, and hand-collected data on the resolutions of financial covenant violations, we find evidence that adverse shocks to a lender's financial health causes the lender to deal with covenant violations more harshly. Furthermore, we find that this increased harshness makes covenant violators significantly more likely to enter financial distress. The effect is mitigated when lenders are far from their minimum regulatory capital ratios before the negative shock to their health.

Our ability to establish a causal effect of lender health on the stringency with which covenant violations are resolved and the transmission to the firms is important. Simple correlations between bank health and covenant violation resolutions do not, unfortunately, provide strong evidence of a lender health effect. In particular, in periods where shocks to bank health are correlated with shocks to the health of their borrowers (e.g., the Global Financial Crisis), a correlation between bank health and the stringency of covenant violation resolutions is likely to largely reflect the strong influence of the well-established borrower health channel. Regulations based on such analysis will likely overemphasize the importance of regulating banks. Our study is the first to credibly estimate a causal effect of bank health on covenant violation cures, disentangled from the borrower health effect.

4. SUMMARY

Both essays included in this dissertation use clean identification (Diff-in-Diff) to answer research questions with important policy implications. They both link the banking sector with the real economy, but the channels are different.

In the first essay, I exploit a new bank regulation rule as a shock to banks' compliance costs. This supply side shock allows me to establish a causal effect of bank regulation on borrowing cost of firms and examine how increased borrowing cost affects firms' interactions with competitors in the product market and ultimately how the industry structure is affected. Findings suggest that bank regulation could have unintended consequences on industry structure through the product market competition channel. Among the several strands of literature I bridge, rational predation among product market participants is the most promising one. The essay, as of this stage, shows some sign of pricing war among firms but can not distinguish who initiate it first. I hope a future version of this paper could speak to the pricing war dynamics.

The second essay uses the Emerging Market Debt Crises as a shock to banks health to investigate how shocks to bank health is transmitted into the real economy through the covenant channel. This is the first paper to use covenant violations in non-US-crisis period to show the covenant channel matters even in tranquil times, echoing finding by Chodorow-Reich and Falato (2019). The quantitative significant of the covenant channel paves the way for future research, as loan covenants became less strict post the financial crisis. What causes this new trend of covenant stringency and what is the implications for the interaction between banking and the real economy? These questions deserve my continuous research efforts in this area.

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APPENDIX A

VARIABLE DEFINITIONS FOR SECTION 2

Below are definitions for variables used in section 2:

- *FBO Dummy* equals one if a lender parent company (BHC) is registered outside the U.S.;
- *Post Dummy* equals one if the facility start date is on or after February 18, 2014;
- *Size* is measured by the log of borrower's total assets;
- *Market to Book* is calculated as (Market Cap + Book Value of Debt)/AT;
- *Leverage* is defined as (DLC+DLTT)/AT, where DLC is the current liabilities, DLTT is the long term debt and AT is total assets;
- *Profitability* is the ratio of EBITDA/AT. OIBDP is used if EBITDA is missing. (SALE-COGS-XSGA) is used when both EBITDA and OIBDP are missing;
- *Tangibility* is the ratio of PPENT/AT;
- *Modified Z-score* is (1.2*WCAP+1.4*Retained Earning+3.3*EBIT +0.999*SALE)/AT. Missing WCAP is replaced with (ACT-LCT) and missing EBIT is replaced with (SALE-COGS-XSGA-DP);
- *Cash Flow Volatility* is the volatility of annual operating cash flow in the previous five years and the operating cash flow is calculated as OCF/(DLC+DLTT);
- *Credit Spread* is the difference between two Moody's Seasoned Corporate Bond Yields in percentage points: Aaa Bond Yield minus Baa Bond Yield;
- *Term Spread* is the difference between two Treasury Constant Maturity Rates in percentage points: 5 Year Rate-3 Month Rate;
- *Federal Fund Rate* is the Federal fund rate in basis points;
- *Indicator for S&P Rating* equals one if the borrower has an S&P long-term bond issuer rating at the time of loan origination;
- *Indicator for Bonds Outstanding* equals one if the borrower has bonds outstanding at the time of loan origination;
- *Dummy for Lending Relationship* equals one if the borrower has at least one loan from the same lender during the past five years prior to the loan origination;
- *Delist dummy* equals one if a borrower's associated delist code is not missing and is not equal to 100;
- *Delist_MA dummy* equals one if a borrower delists due to M&A related reasons, defined as CRSP delisting codes in the 200s (7 such codes in my sample: 200, 231-234, 241-244, 251, 261-262);

- *Delist_dis dummy* equals one if a borrower delists due to distress, defined as CRSP delisting codes in the 400s, 500, an 520 to 591 (17 such codes in this group: 450, 470, 490, 500, 520, 550-552, 560, 561, 570, 574, 580, 582, 584, 585,587; 573 is excluded);
- *Capex* is CAPX/SALE;
- *SG&A expenses* is XGA/SALE;
- *Sales Growth* is $(SALE_t - SALE_{t-1})/SALE_{t-1}$;
- *Gross Margin* is (SALE-COGS)/SALE;
- *Operating Margin* is OIBDP/SALE;

APPENDIX B

VARIABLE DEFINITIONS FOR SECTION 3

Below are definitions for variables used in section 3:

- *Stringent Resolution* equals one if covenant cure actions fall into category 5 or 6 as described in Appendix A;
- *Risk Reduction* equals one if the lender takes at least one covenant cure action, other than increasing the interest rate or charging a fee, that imposes additional costly constraints upon borrower;
- *Income Extraction* equals one if covenant cure actions include increasing the interest rate or charging a fee;
- *Both Actions* equals one if both *Risk Reduction* and *Income Extraction* equal one;
- *Post Crisis Dummy* equals one if violations occur between 1998Q4 and 1999Q4;
- *Exposure Dummy* equals one if lender was exposed to emerging market crises;
- *Indicator for Lack of Bond Rating* equals one if the borrower does not have an S&P long-term bond issuer rating at the time of violation;
- *Indicator for Bonds Outstanding* equals one if the borrower has bonds outstanding at the time of violation;
- *Lender Capital Ratio_Tier1* is lender's tier1 risk-adjusted capital ratio measured as of December 31, 1995. Regressions in the paper use a log-transformed version, defined below;
- *Log Transformed Lender Capital Ratio* is the log of (lender's tier1 risk-adjusted capital ratio - 6);
- *Size* is measured by the log of borrower's total assets;
- *Market to Book* is borrower's (Market Cap + Book Value of Debt)/AT;
- *Leverage* is defined as (DLC+DLTT)/AT, where DLC is the current liabilities, DLTT is long term debt and AT is total assets;
- *Profitability* is the ratio of EBITDA/AT. OIBDP is used if EBITDA is missing. (SALE-COGS-XSGA) is used when both EBITDA and OIBDP are missing;
- *Tangibility* is the ratio of PPENT/AT;
- *Modified Z-score* is (1.2*WCAP+1.4*Retained Earning+3.3*EBIT +0.999*SALE)/AT. Missing WCAP is replaced with (ACT-LCT) and missing EBIT is replaced with (SALE-COGS-XSGA-DP);
- *KZ index* is calculated as defined in Lamont, Polk, and Saa-Requejo (2001);
- *Distance to Default* is calculated according to the Naive approach in Bharath and Shumway (2008);
- *Cash Flow Volatility* is the volatility of annual operating cash flow in previous five years and operating cash flow is calculated as OCF/(DLC+DLTT);

- *Dummy for Lending Relationship* equals one if the borrower has at least one loan from the same lender during past five years;
- *Delisting* equals one if a borrower delists due to distress, defined as CRSP delisting codes in the 400s, 500, an 520 to 591 (excluding 573, 575, and 588);
- *Asset Growth* is defined as the natural logarithm of $(\frac{AT_{t+1}}{AT_{t-1}})$, where t is the year of violation;
- *Debt Growth* is defined as the natural logarithm of $1 + \frac{Debt_{t+1} - Debt_{t-1}}{AT_{t-1}}$, where t is the year of violation and $Debt$ is sum(DLC, $DLTT$) ;
- *Leverage Growth* is defined as the natural logarithm of $1 + \frac{Debt_{t+1}}{AT_{t+1}} - \frac{Debt_{t-1}}{AT_{t-1}}$, where t is the year of violation and $Debt$ is sum(DLC, $DLTT$) ;
- *CapEx Growth* is defined as the natural logarithm of $1 + \frac{Capex_{t+1} - Capex_{t-1}}{AT_{t-1}}$, where t is the year of violation.

APPENDIX C

EXAMPLES OF CURES FOR COVENANT VIOLATION

Below, we describe our manual classification of covenant cures from reading 10-K and 10-Q filings.

1. **Preemptive modification**
   Loan terms are modified before violation actually occurs. This is often done to avoid potential future violations.

   Example: METAL MANAGEMENT INC, 10Q, filed on 19990629
   http://www.sec.gov/Archives/edgar/data/795665/0000950124-99-003962.txt
   Text from SEC filing:
   Since September 30, 1998, due to the decline in our results of operations, we have entered into three amendments to our Senior Credit Facility to avoid the occurrence of events of default under that facility.

2. **Waiver with amendments: lenient actions**
   This category includes but is not limited to: loosing or removing financial covenants, increase in credit availability, extension in maturity, etc.

   Example: SMURFIT STONE CONTAINER CORP, 10K, filed on 19990331
   http://www.sec.gov/Archives/edgar/data/919226/0000950117-99-000654.txt
   Text from SEC filing:
   Also in connection with the Merger, Stone amended and restated the Stone Credit Agreement (the "Stone Credit Agreement") to (i) extend the maturity date on the Stone Tranche B $190 million Term Loan from October 1, 1999 to April 1, 2000, (ii) extend the maturity date of the revolving credit facility from May 15, 1999 to April 1, 2000 or, in the event the Tranche B Term Loan is repaid on or before April 1, 2000, December 31, 2000, (iii) permit the use of the net proceeds from the sale of the newsprint and related assets at the Stone Snowflake, AZ facility to repay a portion of Stone's 11.875% Senior Notes due December 1, 1998, (iv) permit the Merger; and (v) ease certain financial covenants.

3. **Waiver without amendments**
   This category refers to cases in which violation is cured with a simple waiver without amendments.

   Example: LAMONTS APPAREL INC, 10Q, filed on 19961217
   https://www.sec.gov/Archives/edgar/data/785962/0000912057-96-029518.txt
   Text from SEC filing:
   Although the Company failed to comply with certain covenants related to inventory levels

for the months ending July 6, 1996 and August 3, 1996, the Company requested and received a waiver relating to such breaches.

4. **Waiver with amendments: unspecified**
   Violation is cured through amendments but the details are not disclosed.

   Example: METROMEDIA INTERNATIONAL GROUP INC, 10K, filed on 19970331
   http://www.sec.gov/Archives/edgar/data/39547/0001005477-97-000970.txt
   Text from SEC filing:
   At December 31, 1996 Snapper was not in compliance with certain financial covenants under the Snapper Revolver. Subsequent to December 31, 1996, Snapper and AmSouth amended the Snapper Credit Agreement. As part of the amendment to the Snapper Credit Agreement, AmSouth waived the covenant defaults as of December 31, 1996. Furthermore, the amendment replaces certain existing financial covenants with covenants regarding minimum quarterly cash flow and equity requirements, as defined. In addition, Snapper and AmSouth have agreed to the major terms and conditions of a $10.0 million credit facility. The closing of the credit facility remains subject to the execution of definitive loan documentation.

5. **Waiver with amendments: stringent actions**
   This category includes but is not limited to: increase in interest rate, waiver fee or amendment fee, warrants issuance to lenders, asset sale or equity offering to reduce borrowing, reduction in availability, loan collateralized/guaranteed, imposition of new financial covenants.

   Example: DECISIONONE HOLDINGS CORP, 10Q, filed on 19990216
   http://www.sec.gov/Archives/edgar/data/1007588/0000893220-99-000183.txt
   Text from SEC filing:
   As of December 31, 1998, the Company obtained a waiver of certain financial covenants in the amended New Credit Facility until January 28, 1999 and on January 27, 1999, the Company entered into a Waiver of Financial Covenants (the "Waiver Agreement") pursuant to which the Company's senior lenders agreed to waive financial covenants in the amended New Credit Facility through July 29, 1999. These waivers cover the quarters ending December 31, 1998, March 31, 1999 and June 30, 1999. Without these waivers, the Company would not be in compliance with certain financial covenants under the amended New Credit Facility. The Waiver Agreement requires the Company, among other things, to meet certain additional financial covenants as of March 31, 1999, limits the additional borrowings available to the Company under its revolver to $10 million, and generally, requires that interest payments on loans must be made monthly. In connection with the Waiver Agreement, the Company incurred waiver fees of approximately $4.3 million, which will be amortized as interest expense from January 1999 to July 1999.

6. **No Waiver**
   Violation is not waived. The outcome includes but is not limited to: lenders declare the borrower to be in default, loan assigned to third party or to shareholders, lenders require

immediate payment, borrower terminates contracts, or borrower files for bankruptcy.

Example: ALLIANCE ENTERTAINMENT CORP, 10Q, filed on 19970815
http://www.sec.gov/Archives/edgar/data/885066/0000885066-97-000012.txt
Text from SEC filing:
On July 14, 1997, as a result of the defaults under the Pre-petition Credit Agreement, the pending payment default on the Company's Senior Subordinated Notes and an overall inability to operate the Company's business under the existing liquidity restraints, the Company and fourteen of its wholly-owned subsidiaries filed voluntarily under Chapter 11 of the Bankruptcy Code in order to facilitate the reorganization of the Company's core businesses and the restructuring of the Company's long-term debt, revolving credit and trade obligations.