MONASH University

Essays on Changes in the Syndicated Loan Market

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Abstract

This thesis studies issues and trends in the syndicated loan market, a lending arrangement where multiple lenders collaborate to fund a single borrower.

Chapter 2 studies the transformative dynamics of bank-firm matching in the U.S. syndicated loan market after the GFC. Contrary to the pre-GFC era, where bank-dependent predominantly borrowed from well-capitalised banks, this matching ceased to exist after 2010. The analysis reveals that the prolonged low-interest rate environment and the expansion of the bond market collectively contribute to the change. Moreover, unlike in previous economic downturns, the disappearing matching does not adversely impact unrated firms' access to bank credit during COVID-19.

In Chapter 3, I provide evidence that the proportion of loans with multiple lead arrangers in the syndicated loan market has increased substantially in the past two decades. Using loan-level data, I find that these co-lead arrangements facilitate ex-ante screening by incorporating more privately available information on borrowers' riskiness into loan spreads. The empirical analysis also indicates that co-lead arrangements improve ex-post monitoring by decreasing the frequency of covenant violations. However, this effect is contingent on the lead arrangers being highly reputable.

Chapter 4 explores the spillover effects of capital regulation tightening on cross-border lending, specifically focusing on the dynamics of loan spreads. Employing a difference-in-difference regression, I document a reduction of 61.8 basis points in the spread of loans arranged by stress-test banks compared to loans from other banks following the implementation of the first stress test in the U.S. Expanding the analysis to non-U.S. countries, I observe a consistent spillover effect.

Declaration

This thesis is an original work of my research and contains no material which has been accepted for the award of any other degree or diploma at any university or equivalent institution and that, to the best of my knowledge and belief, this thesis contains no material previously published or written by another person, or any use of generative artificial intelligence technologies, except where due reference is made in the text of the thesis.

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Chapter 1

Introduction

Banks play a pivotal role in directing funds from agents with a surplus of savings to those in a deficit. An examination of bank lending offers valuable insights into economic growth. The banking landscape has undergone notable changes, particularly after the global financial crisis (GFC), driven by, for instance, the implementation of new banking regulations, the adoption of unconventional monetary policies, and the emergence of non-bank institutions. This thesis conducts an empirical study on emerging issues within the corporate lending market. Investigating how these changes impact the lending market and the resulting tensions between banks and firms is a key focus of this thesis. It aims to contribute to the literature by documenting trends of a diminishing bank-firm relationship and an increasing prevalence of co-lead arrangements in the U.S. domestic lending market and assessing their implications. The focus of this study is specifically on syndicated loans, a lending arrangement wherein multiple lenders collectively provide credit to a single borrower. The syndicated loan has come to facilitate risk-sharing and address capital constraints. Over time, this market represents a very significant segment of corporate lending and serves as a vital source of funding for firms. While the syndicated loan market holds considerable importance in corporate lending, it is crucial to recognize its uniqueness compared to traditional one-to-one loans. Therefore, this thesis also endeavours to delve into the distinctive role that syndicated loans play as the lending market evolves and undergoes dynamic changes.

Chapter 2 Shifting Paradigms in Bank-Firm Relationships: Post-GFC Dynamics in the Syndicated Loan Market

Chapter 2 studies the transformative dynamics of bank-firm matching in the U.S. syndicated loan market after the GFC. Contrary to the pre-GFC era, where bank-dependent, typically unrated firms predominantly borrowed from well-capitalised banks, there is a paradigm shift in these relationships post-2010. Specifically, the traditional matching between unrated firms and well-capitalised banks has ceased to exist. The analysis reveals that the prolonged low-interest rate environment and the expansion of the bond market collectively contribute to the change. On the demand side, better access to the bond market reduces incentives for many unrated firms to seek out well-capitalised banks. This study documents that only those unrated firms finding it challenging to transition from loans to bonds continue to match with well-capitalised banks post-GFC, while those borrowing from under-capitalised banks prepare for the transition by obtaining credit ratings. On the supply side, under-capitalised banks displayed an increased propensity to chase yield by lending to unrated firms, thus crowding the matching between unrated firms and well-capitalised banks. Moreover, the results also show that, unlike in previous economic downturns, matching with well-capitalised banks does not influence unrated firms' access to bank credit during the COVID-19 period. This finding is attributed to the stricter capital requirements implemented post-GFC, enhancing the ability of low-capital banks to provide lending in the COVID-19 shock.

Chapter 3 The Rise of Co-lead Arrangements in Loan Syndication

In Chapter 3, I provide evidence that in the syndicated loan market, the proportion of loans with multiple lead arrangers has increased substantially in the past two decades.

Does the co-lead arrangement improve lead arrangers' ability to produce private information? Using loan-level data, I find that the co-lead arrangement facilitates ex-ante screening by incorporating more privately available information on the borrowers into loan pricing. Specifically, a full-rating downgrade results in a 17.74 basis points increase in loan spreads in a co-lead loan, which doubles the magnitude of the widened spreads in a single-lead arranger loan. I also examine the role of co-lead arrangements in enhancing ex-post monitoring. I find supporting evidence, but it is conditional on lead arrangers having a high reputation. Specifically, it shows that when lead arrangers are, on average, large in size or have large market shares, the frequency of covenant violations during the loan life is lower in co-lead loans than in single-lead arranger loans. In addition, co-lead loans with highly reputable lead arrangers also experience milder covenant violations by reducing the difference between the covenant threshold and the corresponding financial variable, given a covenant is breached. Moreover, I address endogeneity issues by using a propensity score matching method and a two-stage instrumental variable approach, with the instrumental variable being a borrower's past relationship with lenders; I find similar results in both methods.

Chapter 4 International Spillovers from Changes in Capital Regulation: Evidence from the Syndicated Loan Market

Chapter 4 explores the spillover effects of capital regulation tightening on cross-border lending, specifically focusing on the dynamics of loan spreads in the syndicated lending market. Employing a difference-in-difference regression on U.S. stress test implementation, the study reveals a 61.8 basis points reduction in loan spreads from stress-test banks compared to non-stress-test banks after 2009. I propose a novel mechanism to rationalise this spillover effect. This mechanism emphasises a unique feature of loan syndication where the lead arranger might not allocate sufficient monitoring efforts as desired by the participants. Given this moral hazard problem, participants will accept lower loan spreads only when the lead arranger's share in the loan is sufficiently high. By acknowledging that cross-border lending facilitates regulatory arbitrage, lead arrangers are willing to retain more shares after capital regulation tightening. This is supported by an 8% increase in lead arranger shares following stress test implementation. Expanding the analysis to non-U.S. countries, I observe a consistent spillover effect. I also find that the effect diminishes when the borrower's country has also enforced stricter capital regulation or the regulation is implemented on a consolidated basis; in both cases, the cross-border loans are not attractive because regulatory arbitrage cannot be achieved by lending through foreign subsidiaries.

Authorship

This thesis consists of three research papers written during the author's candidature. A working paper based on Chapter 2, with Han Zhou as the lead author and Silvio Contessi, Hassan Naqvi, and Eden Zhang from Monash University as co-authors, has been prepared for future submission to a scholarly journal.

Chapter 2

Shifting Paradigms in Bank-Firm Relationships: Post-GFC Dynamics in the Syndicated Loan Market

2.1 Introduction

Due to the prolonged low-interest rates period and stricter capital requirements, banks' operating environment changed markedly after the Global Financial Crisis (GFC). Have these changes impacted the way in which banks and firms match in lending markets? This paper studies the post-GFC evolution of endogenous two-sided bank-firm matching and identifies its causes and consequences. Schwert (2018) provided the first robust evidence that bank-dependent firms, proxied by unrated firms, tend to borrow from well-capitalised banks while less dependent firms match with poorly capitalised banks. In this paper, we show that this capital-based matching is time-varying and explore how it evolved since the GFC. We find that the capital-based matching *disappears* after the GFC.[1] Figure 2.1 describes the presence

[1] Two examples can illustrate this stylised fact. An unrated firm, Saga Communications, obtained three loans from Fleetboston Financial Corp and three loans from Bank of New York Mellon during 2001-2006; both banks' capital ratios are in the top terciles of banks by capital ratios. After the

of the capital-based matching by plotting, in a four-quarter rolling window, the relationship between banks' capital ratios and firms' unrated status.[2] It shows a positive relationship fluctuating in a cyclical pattern between 1987 and 2009 until it becomes essentially null during 2010-2019. This indicates that there is no longer a match between bank-dependent firms and well-capitalised banks after the GFC, namely the "disappearing matching".

Insert Figure 2.1 here.

To perform a formal test, we use two empirical models that are popular in the literature for studying bank-firm matching. We first collect U.S. loan-level data from Dealscan, merging with banks' and firms' financial information from Compustat.[3] Then, we construct a matching sample with all possible combinations of lenders and borrowers in each quarter. We identify observed matches where the banks and firms actually make loan contracts and hypothetical matches consisting of the rest. Using the matching sample, we run a linear regression of the observed match dummy on the interaction between the bank capital ratio and the firm unrated dummy; thus, it estimates the probability of observing capital-based matching. We find that the coefficient of the interaction term changes from significantly positive in the pre-GFC sample to insignificant in the post-GFC sample. Hence, the matching between unrated firms and well-capitalized banks is no longer more likely to be observed in the data after the crisis. We confirm the result by estimating a semiparametric matching model. It measures the value of bank-firm matches, assuming that the observed matches are more valuable than the hypothetical matches. We find the same change in the coefficient of interest, indicating that the matching between unrated firms and well-capitalised banks does not add value to a bank-firm matching after the GFC. Both results confirm the phenomenon of the disappearing matching.

GFC, however, the firm turned to JP Morgan Chase&Co. and Bank of America Corp whose capital ratios are in the bottom tercile. Similarly, another unrated firm, Scansource Inc, switched from a well-capitalised bank (Trust Financial Corp) to an under-capitalised bank (JP Morgan Chase&Co.) after the GFC.

[2]For each of the rolling four-quarters, we use loan-level data and run a pooled regression of bank capital ratio on a dummy variable indicating whether the borrower is unrated or not with various controls. This regression is used as the main regression model in Schwert (2018). Then, we plot the coefficients of the unrated dummy over time.

[3]We focus on the matches of lead arrangers.

Next, we put forth an explanation linked to the prolonged low-interest rate environment and the resultant growth in the bond market. Initially, by borrowing from a more solvent bank, bank-dependent firms can sidestep the termination of lending relationships when a crisis hits and impairs banks' lending capacity. The firms can also benefit from a lower cost of information delusion because banks with larger shareholder equity are encouraged to dedicate more efforts to monitoring. Both features suggest that unrated firms prefer to borrow from well-capitalised banks, as observed in pre-GFC. However, we argue that the bond market expansion after the GFC enhances access to a nonbank funding source, diminishing the advantages of borrowing from well-capitalised banks. The growth in the bond market has been documented in prior studies such as Çelik et al. (2020); Berg et al. (2021). We find a substantial increase in both the number of issues and the total issuance amount in the U.S. public bond market. Additionally, there is a noticeable decline in average bond yields relative to loan rates in the post-GFC period. The favourable conditions in the bond market facilitate a smoother and less costly entry for bank-dependent firms, serving as a buffer against the potential termination of lending relationships. Consequently, bank-dependent firms exhibit less concern about bank capitalization.

As a prediction from the story, only the unrated firms facing difficulties in switching from loans to bonds will still match with well-capitalised banks after the GFC. We use total assets, the difference between estimated bond issuing costs and average loan rates, and loan outstanding as a share of total debt to proxy for switching difficulties. We then group unrated firms into two according to each variable. We estimate the models in the post-GFC sample, and the data aligns with the prediction. We find a higher probability of observing matching between well-capitalised banks and unrated firms that are small in size, face higher costs of accessing the bond market, and predominantly rely on loans. Conversely, the other unrated firms are as likely as rated firms to borrow from well-capitalised banks. We further document that, in the post-GFC period, unrated firms that borrow from low-capital banks are more likely to acquire a credit rating after the loan origination, compared to those borrowing from well-capitalised banks. Therefore, if the unrated firms choose not to rely on well-capitalised banks, they would consider the bond market as a "spare tyre" and prepare for the transition.

What is the consequence of the disappearing matching? We compare unrated firms' loan access during the COVID-19 shock in two scenarios: an observed scenario when there is no capital-based matching and a counterfactual scenario when capital-based matching is in place instead. The loan access is measured by the change in loan provision of an unrated firm's bank. In the observed scenario, an unrated firm borrows from its original bank in its most recent loan before the shock. In the counterfactual, we let unrated firms match with well-capitalised banks. Surprisingly, we find no statistical difference in loan access between the observed and the counterfactual scenarios during the COVID-19 shock. In contrast, during the GFC, unrated firms experienced a substantial reduction in access to lending if they did not borrow from well-capitalised banks. This implies that while the presence of capital-based matching pre-GFC played a crucial role in credit allocation, the absence of such matching post-GFC does not lead to a significant credit disadvantage for unrated firms during the COVID-19 shock. We attribute this finding to the implementation of stricter capital requirements after GFC, such as Basel III and stress test programs. These regulatory changes have enhanced the ability of low-capital banks to provide lending during the most recent shock.

We then provide some further discussions. First, because the explanation linked to the bond market expansion stands from a borrower's point of view, we explore a supply-side factor to explain the disappearing matching. From the perspective of well-capitalised banks, they have the advantage of lending to bank-dependent firms because they are better at handling risks and arranging syndicated loans. Nonetheless, we posit that the incentives for low-capital banks to form matches with bank-dependent firms increased after the GFC, crowding out the capital-based matching. The stricter capital requirements pressure under-capitalised banks to adjust (up) their capital. At the same time, when interest rates are close to the zero lower bound, the rigidity of deposit rates impairs banks' interest margins. Thus, low-capital banks are incentivised to pursue short-term earnings from unrated firms to extract information rents and risk premiums. If this "chase for yield" argument is the driving factor, we expect to observe risky banks are also likely to match with unrated firms aiming to meet target returns under the low-interest-rate period. The empirical results support this conjecture, and we use the non-performing asset ratio, loan loss provisions over gross loans, and liquidity ratio to measure bank riskiness. Moreover, Heider et al. (2019) suggests that when banks rely less on deposits, their interest margins are unaffected by the low interest rate; thus, these banks have minimal incentive to chase for yield. Hence, conducting an analysis on a subset of banks with low deposit ratios can effectively mitigate the impact of the supply-side factor. However, even within this subset, the absence of capital-based matching persists. This underscores the significance of the demand-side factor in explaining the disappearing matching. Secondly, we find that capital-based matching also disappears at the intensive margin. Prior to the GFC, loans initiated from a well-capitalized bank to an unrated firm have a larger loan amount compared to other loans; however, this distinction is no longer evident in the post-GFC sample. Lastly, we confirm the results from the linear regression model using logistic regression.

Literature Review and Contributions A one-sided lending relationship is widely documented in the prior literature; that is, financially opaque firms rely more on banks than transparent firms. The relationship is ascribed to banks' advantages in processing borrowers' information and mitigating information asymmetry (Berlin and Loeys, 1988; Rajan, 1992; Schenone, 2010; Schwert, 2018). For example, in Diamond (1984)'s classic model, banks enjoy economies of scale in monitoring their lending. Also, Fama (1985) argues that the history of deposit accounts allows banks to obtain more low-cost information about their borrowers' repaying ability. This study enriches the literature by providing a nuanced exploration of the decisions made by bank-dependent firms regarding their preferences, or lack thereof, for well-capitalized banks over low-capital banks.

The foundation for this study is Schwert (2018), which for the first time documents the two-sided matching between bank-dependent firms and well-capitalized banks. We extend this research by examining the time variation in the capital-based matching. Schwert (2018) suggests that such matching enhances credit allocation efficiency. Intuitively, one would expect market efficiency to improve over time, resulting in stronger matching. However, we present a novel finding that this matching does not persist throughout the entire ten years following 2010. Additionally, we offer explanations linked to post-GFC policy and regulatory implementations. Furthermore, while Schwert (2018) propose a concept, we provide empirical evidence demonstrates that both lender-side and borrower-side factors contribute to the strength of capital-based matching.

There are other recent papers studying two-sided bank-firm matching in loan contracts. For example, using Portuguese loan-level data, Farinha et al. (2022) confirm the capital-based matching. The prior literature also documents another matching in the loan market, which is between small firms and small banks (see Berger et al., 2005; Levine et al., 2020). Using survey data and focusing on the other side of the size distribution, Berger et al. (2017) find no disappearance of the capital-based matching between small banks and small firms over time. Instead, the size-based matching is robust to different macroeconomics and financial conditions that may characterise different phases of the business cycle. This paper finds that capital-specific matching is more vulnerable and more fragile over time. Using variations in the cultural contexts, Accetturo et al. (2023) finds that bank firm match depends on measures of cultural distance. However, they show that it is the demand-side incentive that mainly drives the capital-based matching in this context, while we find that the supply-side factor of reaching for yield can also affect the strength of the matching between bank-dependent firms and well-capitalised banks.

This study also relates to the literature exploring interactions between the loan and bond markets. First, Datta et al. (1999) and Ma et al. (2019) find that the holding of loans imposes a certification effect on bond prices, while Booth (1992) documents the impact of bonds on loan pricing. Second, Chemmanur and Fulghieri (1994), Bolton and Freixas (2000), De Fiore and Uhlig (2011), and Crouzet (2018) literature discuss capital structures by comparing firms' choices between loans with bonds in firms' debt choices. Lastly, Goel and Zemel (2018) and Darmouni and Siani

(2020) study the tendency to switch from loans to bonds during crises. Also, Becker and Benmelech (2021) and Berg et al. (2021) document that bonds are the preferred sources of funding, compared to bank loans, to finance the liquidity shock during the COVID period. The evidence is consistent with our bond market explanation for the disappearing matching.

Last but not least, this study relates to the literature studying the dynamics in bank lending during the post-GFC period. Similar to this study, a wide range of papers examine the impact of the prolonged low-interest rate environment on bank lending. Papers such as Claessens et al. (2018), Molyneux et al. (2020), and Lopez et al. (2020) find that, due to the rigidity of deposit rates raising banks' funding costs, the low-interest rates negatively affect banks' net interest incomes and profitability. Borio and Gambacorta (2017) and Heider et al. (2019) provide evidence that banks slow down their lending activities in such a low-rate environment as a consequence of the reduced interest margins. Another line of research studies the impact of the implementation of bank regulations after the crisis. Bordo and Duca (2018) and Hogan and Burns (2019) argue that the Dodd-Frank Act increased the regulatory burden on banks. The additional constraints can discourage banks from lending to small and riskier businesses because risky loans lower the chance of passing the stress tests under the act. Moreover, Acharya et al. (2018) and Cortés et al. (2020) find the negative impact of the U.S. stress tests on syndicated loans and on small business lending, respectively. The papers listed above conclude that the post-GFC changes in monetary policies and capital regulations alter banks' lending, while we show that these changes also disturb firms' borrowing choices of banks. Also, the regulatory changes in capital regulations ultimately naturalise the negative impact of the disappearing matching.

The remainder of this paper is organised as follows. Section 2.2 elaborates on data sources, the sample setting, and the testing methodologies. Section 2.3 discusses the rationales behind the matching between bank-dependent firms and well-capitalised banks and proposes an explanation of "bond market expansion" for the disappearing matching after the GFC. This section also introduces a testing implication. Section 2.4 presents the empirical results. Following that, Section 2.5 tests the impact of the disappearing matching in the COVID-19 shock by constructing counterfactual scenarios. Then, Section 2.6 proposes a supply-side explanation of "chase for yield". We illustrate the story, specify a testing implication, and present the results. In Section 2.7, we further discuss our results and extend the results with robustness tests. Finally, Section 2.8 concludes.

2.2 Data and Methodology

This section documents the data sources and methodology to test the disappearing matching. Section 2.2.1 describes the sample and data sources. Section 2.2.2 introduces two alternative methodologies to test the existence of capital-based matching before the GFC and its disappearance during the post-GFC sample. Section 2.2.3 presents the summary statistics for key variables.

2.2.1 Data Sources and Sample Description

In this paper, the loan information comes from Thomson Reuters Loan Pricing Corporation's DealScan (DealScan). This database contains detailed information describing a syndicated loan. However, it only provides limited financial data on borrowers and lenders. We collect relevant information from Compustat, which provides quarterly data for publicly held banks and firms, and use the linking tables provided by Chava and Roberts (2008) and Schwert (2018) to merge it with DealScan.[4] Because Chava and Roberts (2008)'s linking table only updates to 2017, we follow their method to manually merge the two databases for the period of 2017-2020 (Appendix A.1.2 provides more details of the method and process).[5] After the merge, the sample spans from quarter one of 1982 (1982Q1 for short) to 2019Q4. However, the data

[4]We thank Sudheer Chava, Michael Roberts and Michael Schwert for making these data available on WRDS. The lenders in Schwert (2018)'s linking table are those who acted as lead arrangers on at least 50 loans or at least $10 billion in volume in the set of loans in Chava and Roberts (2008)'s linking table.

[5]The authors updated the linking table at the end of 2017. However, Dealscan updates some of the 2017 loans in the following years. Thus, our manual merge starts in 2017.

availability of the early part of the original sample is problematic. In particular, the number of loans per quarter remained at a three-digital number until 1987, and the observations in 2020 are quite limited at the time of writing.[6] Thus, we restrict the final sample period from 1987Q1 to 2019Q4 (The main testing period in the paper is the post-GFC period, which is between 2010Q1 and 2019Q4).

A syndicated loan involves multiple lenders that jointly finance a borrower. It is the lead arranger's role to make the contract, communicate with the borrower, and sign the loan terms while participants only provide funds.[7]. Also, during the life of a loan, the lead arranger is charged with administrative and monitoring responsibilities. Following the convention in the literature (see Sufi, 2007; Schwert, 2018, among other), this study focuses on lead arrangers in syndicated loans to study bank-firm matching. We follow Bharath et al. (2011) to define the lead arranger of a loan and aggregate to a consolidated basis.[8] On average, each loan has 1.05 lead arrangers in the pre-2010 sample and 1.40 lead arrangers after the GFC.

This paper focuses on U.S. banks and firms. We exclude all loans to financial borrowers (SIC between 6000 and 6999) and exclude all observations with non-positive assets, equity, and share prices. These restrictions exclude about 20% of loans in the original DealScan-Compustat merged sample. The final sample contains 10,737 loans pre-GFC and 19,581 loans post-GFC. The quarterly average number of banks (firms) is 18.27 (220.37) from 2000 to 2007 and 12.08 (172.15) from 2010 to 2019, respectively.

[6] In DealScan, the number of loans per quarter reaches one thousand only after 1986. Also, in the original DealScan-Compustat merged sample, the issuance of loans (borrowers) per quarter reached three- (two-) digital numbers in the last quarter of 1987 (1986).

[7] In the syndicated loan process, banks bid for the lead arranger position by presenting potential price-quantity combinations they are willing to offer. After mandating the lead arranger, the bank engages with the borrower to negotiate preliminary loan terms and often determines the underwritten amount. Subsequently, the lead bank markets the loan to potential participants. Once the contract closes and the allocation is finalised, the lead arranger assumes responsibility for communicating with and monitoring the borrower

[8] The definition of lead arrangers follows Bharath et al. (2011), who also study the relationships between banks and firms. In particular, Dealscan specifies a field called "Lead Arranger Credit," which takes values of "Yes or "No" for each bank. Any bank assigned "Yes" in this field is recognised as a lead arranger. Papers like Sufi (2007) and Gopalan et al. (2011) also use the field as the sole criteria. Bharath et al. (2011) identify lead arrangers using another Dealscan variable named "lender role". They require a lead arranger to have one of the following "lender role": admin agent, arranger, lead bank, and sole lender. The definition used in this study has a correlation of at least 0.85, to identify the lead arranger, with each of the other definitions.

There was a slight drop in the numbers partially due to mergers and acquisitions after the GFC.

Two other data sources are used in this paper. One is the Mergent Fixed Income Securities (FISD), and the other is the Capital I.Q. Capital Structure. FISD is a database recording detailed terms - including bond types, offering yield, offering amount, etc. - of all U.S. public bond issues. We exclude all convertible bonds and all bonds to financial companies because these bonds differ significantly from the others in timing and purposes. Capital I.Q. provides data on the capital structure of firms worldwide. For example, this dataset reports the term loan outstanding of a firm in a given year. Using the Capital I.Q. data, we are able to measure how much a firm's total debt consists of bank loans. However, the data is only comprehensive from 2002 and on an annual basis. The debt types include loans (consisting of drawn credit lines and term loans), bonds (consisting of senior bonds and notes and subordinated bonds and notes), commercial paper, capital leases and other borrowings.

2.2.2 Empirical Models

To illustrate and emphasise the disappearance, we separately estimate the strength of the capital-based matching during the pre-and post-GFC periods. The pre-GFC period is from 1987Q1 to 2007Q2. As shown in Figure 2.1, the capital-based matching weakens during the banking crises. Since the crisis periods are not the focus of this paper, we exclude the 1990s banking crisis periods from our pre-GFC sample (i.e., from 1990Q1 to 1992Q4), but the results are unchanged if crisis periods are included. The post-GFC period is defined as 2010Q1-2019Q4. We use two empirical models. The first one is a regression model that estimates the impacts of banks' and firms' characteristics on the probability of matching. We also estimate a semiparametric matching model developed by Fox (2018). This model allows us to generate counterfactuals where firms match with different banks deviating from their actual loan contracts.

2.2.2.1 The Regression Model

We first introduce the regression model. Although the dependent variable is a dummy, we use a linear probability regression in the main tests because it is better for explaining the interaction terms. However, we confirm the robustness of the results using a logistic regression. To conduct the regression, we construct a sample that comprises all possible matches of the banks and firms in each quarter. One part of the matches are observed matches where the banks and firms actually contracted and have records in Dealscan, while the rest are hypothetical ones. Take 2013Q3 as an example. There are 13 banks and 174 firms. In total, we can construct 2262 (13×174) bank-firm combinations and 225 of them are observed matches. Note that the number of observed matches is slightly larger than the total number of firms. This is because there are 33 borrowers borrowing from more than one lead arranger. Also, 10 out of the 13 banks lend to more than one firm. Thus, this is a many-to-many matching game. The unique observation in the sample is a bank-firm-quarter triplet. We then create an Observed Match Dummy, which takes one if a bank-firm match a given quarter is recorded in Dealscan. We regress this dummy on a vector of bank and firm characteristics and their interaction terms. The regression is specified in the following equation.

$$
\begin{aligned}
\text{Observed Match Dummy}_{bft} = {} & \beta_1 \text{Bank Capitalisation}_{bt} \times \text{Unrated Dummy}_{ft} \\
& + \theta_1 \text{Bank Capitalisation}_{bt} + \theta_2 \text{Unrated Dummy}_{ft} \\
& + \theta_3 \text{Bank Size}_{bt} + \theta_4 \text{Firm Size}_{ft} \\
& + \theta_5 \text{Bank Size}_{bt} \times \text{Firm Size}_{ft} \\
& + \theta_6 \text{Relationship Lending Dummy}_{bft} + \theta_7 \text{Top Industry}_{bft} \\
& + \theta_8 \text{Bank-Firm Distance}_{bft} + \sum_{i=9}^{15} \theta_i \text{Other Firm Controls}_{ft} \\
& + d_b + d_f + d_t + u_{bft} \qquad (1)
\end{aligned}
$$

Equation 1 estimates the probability of observing an actual match. The subscripts b, f, and t refer to bank, firm, and quarter, respectively. Variables are defined in

Table A.1 in Appendix A.1.1.

A similar regression setting is widely used in the prior literature to study matching in loan contracts. For example, Farinha et al. (2022) use it to examine the matching between banks and firms, and Corwin and Schultz (2005); Sufi (2007); Gao and Jang (2021) test the matching among banks. Alternatively, as in Figure 2.1, we could use the loan-level sample (i.e., observed matches) and regress Bank Capitalisation on Unrated Dummy. Comparatively, there are two advantages of running Equation 1. First, including all possible bank-firm matches takes into consideration the out-of-sample information. Focusing only on loan-level data misses the information on the number of potential bank-firm matches outside the actual loan contracts. Second, using the loan-level data may double-count a given bank-firm matching because a borrower can have two loan facilities with the same bank in one deal. This will misleadingly reduce the standard errors (Sufi, 2007). In the current setting, the duplicated records in a given quarter in Dealscan are treated as only one match.

The variable of interest is the interaction term between Bank Capitalisation and Unrated Dummy. Following Schwert (2018), Bank Capitalisation is the market capital ratio, which is defined as market capitalisation (the product of the share price and common share outstanding) divided by quasi-market assets (a sum of market capitalisation and book liabilities).[9] Bank-dependent firms are defined as those that do not have a credit rating. This definition is widely used in prior literature (e.g., Kashyap et al., 1994; Faulkender and Petersen, 2006; Chava and Purnanandam, 2011). On the one hand, the unrated status means little access to the public bond market.[10] If a firm cannot borrow from the public, bank credit is a preferred private source

[9]The focus on the market measure is due to the fact that this measure of capital is forward-looking while the book value is backwards-looking. Also, the book capital ratio is vulnerable to earnings manipulation (Frank and Goyal, 2009). The bank capital ratio also has a regulatory definition (defined by the Basel Committee on Banking Supervision Basel Accords). The use of regulatory capital ratios is subject to criticism in the literature. For instance, Demirguc-Kunt et al. (2013) argue that the Basel-defined capital ratios are less informative to investors than the equity ratio, especially during a crisis. Also, Schwert (2018) finds that bank-dependent firms have no preference for high-regulatory-capital banks. To be consistent with the literature and the pre-GFC evidence, we use market capital ratios to explore the time variation of the capital-based matching, as well.

[10]According to Schwert (2018), on average, firms with access to the public bond market have 62.55% bond outstanding at the time of loan origination, while firms without access only have 10.26% bond outstanding.

of funding due to its flexibility (Darmouni and Siani, 2020) and the information it signals (Fama, 1985). On the other hand, a firm without a rating tends to be more informationally opaque, given that it has a limited tractable history of repaying debts and lacks a third-party assessment of its creditworthiness (Frank and Goyal, 2009; Sufi, 2007).[11] Thereby, unrated firms rely more on banks' advantages in information production and monitoring. In Equation 1, Unrated Dummy takes one if a firm does not have a long-term issuer rating from S&P in the loan origination month. Compustat no longer updates S&P longer-term issuer rating since 2017Q1. We then use the relevant information in the Capital IQ S&P Credit Ratings database to update the sample.[12]

The coefficient of the interaction variable. A positive β_1, means we have a higher probability of observing an actual match between an unrated firm and well-capitalised banks, relative to the other matches between unrated firms and poorly-capitalised banks, rated firms and well-capitalised banks, and rated firms and poorly-capitalised banks. We can also interpret β_1 from a borrower's perspective. θ_1 measures the increase in the probability of borrowing from banks with (a one-unit) higher capital ratios. Thus, a positive β_1 indicates that unrated firms have a higher probability of borrowing from well-capitalised banks, relative to rated firms. From either point of view, a positive (insignificant) β_1 indicates the existence (non-existence) of capital-based matching. Thus, we expect β_1 to be positive during the pre-GFC period and insignificant during the posit-GFC period, as evidence of the disappearing matching.

Equation 1 controls for several bank-firm-level factors affecting a bank-firm matching. The first one is the interaction between bank size and firm size. The literature evidenced that small banks have significant advantages in lending to small firms relative to large banks (see Berger et al. (2017), among others). Thus, the size-based interaction isolates the size effect and is expected to have a positive coefficient. Prior literature finds that a bank and a firm are more likely to match if they have less information proximity (Degryse and Van Cayseele, 2000; Boot, 2000; Bolton et al., 2016)

[11] According to this study's sample, an average unrated firm is 1.43 times smaller in book assets, has 1.33 times lower tangible asset ratio, and is less likely to be listed than a rated firm.

[12] The results are similar when we use Capital IQ data to measure the unrated dummy for the whole sample period.

and/or are closer in geographic distance Chen and Song (2013); Farinha et al. (2022). In order to control for the informational closeness, Equation 1 includes two dummies. Relationship Lending considers whether the bank-firm pair has a prior relationship. Prior literature uses different ways to define relationship intensity, and we follow Bharath et al. (2007) and Schenone (2010) to define a dummy, taking the value of one if the current firm and current bank have ever made a lending contract in 20 quarters before the loan. Another proxy for the informational closeness (Top Industry) measures a bank's information on the firm's industry; it takes one if a firm's industry is in the top three industries to which the bank makes loans in a given quarter. In order to capture the geographic closeness, Equation 1 includes the physical distance between banks and firms (Bank-Firm Distance). A bank is more likely to lend to a closer firm because this facilitates monitoring (Agarwal and Hauswald, 2010; Levine et al., 2020). All three variables are expected to have positive coefficients.

The borrower-specific variables contain Altman's z-score measuring default risks, asset tangibility, profitability, cash holdings, leverage, Tobin's Q, and years since IPO to control for credit demands.

Lastly, both bank-(d_b) and firm-(d_f) fixed effects are included to capture unobservable time-invariant factors from both the supply-side and demand-side. Quarter-fixed effects (d_t) controls for macroeconomic factors. The standard errors are clustered at the bank level to account for the within-bank correlation in the error terms (Petersen, 2008).

2.2.2.2 The Semiparametric Matching model

Next, we estimate a semiparametric model to verify the results of the linear probability regressions. The use of the semiparametric model allows us to test the impact of the disappearing matching by generating counterfactual scenarios. In addition, the semiparametric model has two other advantages over the regression model. First, since loan rates and other nonpecuniary terms represent the value transferred from borrowers to lenders, they can be important determinants of bank-firm matching. However, we cannot observe the relevant data for hypothetical bank-firm matches, so we are unable to include loan terms as controls in the regression. Rather, the estimation of the semiparametric model does not require loan-level information. Second, the model does not require a known function form of the error term's distribution (e.g., the logistic function in a logit regression). Instead, it assumes a rank-order property that holds for various distributions Fox (2007). This is where the "semi" comes from.

The semiparametric model provides an estimate of the value/utility of banks' and firms' characteristics to a match. Thus, in this study, we test whether the combination of high capital ratio and unrated status are no longer value-added after the GFC. The estimation of Fox (2018) model relies on pairwise stability. Simply, it means that it will be less desirable and less valuable for either the bank or the firm to switch its counterpart in an observed match.

Consider an observed bank-firm match between Bank_1 and Firm_1. Let's write the value of the match to the bank; it consists of transferred payments from the firm to the bank and an extra synergy effect specific to the bank. For example, The transferred value includes loan rate and other contracting terms such as covenants, and the synergy can be thought of as the reduction in screening costs by matching with a close borrower. We specify the payoff to the bank as follows.

$$V_b\left(\text{Bank}_1, \text{Firm}_1\right) + t\left(\text{Bank}_1, \text{Firm}_1\right)$$

Similarly, we can write the value of the match from a firm's point of view as a synergy specific to the firm minus the transferred payments.

$$V_f\left(\text{Bank}_1, \text{Firm}_1\right) - t\left(\text{Bank}_1, \text{Firm}_1\right)$$

The total value of the match can be expressed as:

$$V_{b_1,f_1} = V_b\left(\text{Bank}_1, \text{Firm}_1\right) + V_f\left(\text{Bank}_1, \text{Firm}_1\right)$$

The pairwise stability says that the value of the observed bank-firm match is larger than that of a hypothetical match where the bank lends to another firm. Let's introduce a second firm – Firm_2. Then, the payoff to Bank_1 under the observed match with Firm_1 should be larger than the payoff under the hypothetical match with Firm_2.

$$\begin{aligned} V_b\left(\text{Bank}_1, \text{Firm}_1\right) + t\left(\text{Bank}_1, \text{Firm}_1\right) &\geq V_b\left(\text{Bank}_1, \text{Firm}_2\right) + \\ &\left(V_f\left(\text{Bank}_1, \text{Firm}_2\right) + t\left(\text{Bank}_2, \text{Firm}_2\right) - V_f\left(\text{Bank}_2, \text{Firm}_2\right)\right), \end{aligned}$$

where $\text{V}_f(\text{Bank}_1, \text{Firm}_2)+\text{t}(\text{Bank}_2, \text{Firm}_2)-\text{V}_f(\text{Bank}_2, \text{Firm}_2)$ is the maximum transferred value Firm_2 is willing to pay Bank_1, which includes the synergy obtained from Firm_2's hypothetical match, the saving of transferred payments from its observed match with Bank_2, and the loss of the synergy from the observed match.

Estimating the parameters using one pairwise stability condition involves transferred payments. To remove the transferred payments, Fox (2018) introduces a second observed match between Bank_2 and Firm_2. Then, we compare the value of this observed match to the value of the hypothetical match between Bank_2 and Firm_1.

$$\begin{aligned} V_b\left(\text{Bank}_2, \text{Firm}_2\right) + t\left(\text{Bank}_2, \text{Firm}_2\right) &\geq V_b\left(\text{Bank}_2, \text{Firm}_1\right) + \\ &\left(V_f\left(\text{Bank}_2, \text{Firm}_1\right) + t\left(\text{Bank}_1, \text{Firm}_1\right) - V_f\left(\text{Bank}_1, \text{Firm}_1\right)\right) \end{aligned}$$

Summing the two pairwise conditions cancels out the transferred payments:

$$\begin{aligned} &V_b\left(\text{Bank}_1, \text{Firm}_1\right) + V_f\left(\text{Bank}_1, \text{Firm}_1\right) + V_b\left(\text{Bank}_2, \text{Firm}_2\right) + V_f\left(\text{Bank}_2, \text{Firm}_2\right) \\ &\geq V_b\left(\text{Bank}_1, \text{Firm}_2\right) + V_f\left(\text{Bank}_1, \text{Firm}_2\right) + V_b\left(\text{Bank}_2, \text{Firm}_1\right) + V_f\left(\text{Bank}_2, \text{Firm}_1\right) \end{aligned}$$

The left-hand side of the inequality is the total value of two actual matches, while the left-hand side is the total value of two hypothetical matches. Thus, the inequality can be re-written as $\text{V}_{b_1,f_1}+ \text{V}_{b_2,f_2}\geq\text{V}_{b_1,f_2}+ \text{V}_{b_2,f_1}$, where the total value of each match,

V_{bf}, is defined in the following linear function.

$$
\begin{aligned}
V_{bf} &= \boldsymbol{X}'_{bf}\boldsymbol{\beta} + \epsilon_{bf} \\
&= \beta_1 \text{Bank Capitalisation}_b \times \text{Unrated Dummy}_f + \beta_2 \text{Bank Size}_b \times \text{Firm Size}_f \\
&+ \beta_3 \text{Relationship Lending Dummy}_{bf} + \beta_4 \text{Top Industry}_{bf} \\
&+ \beta_5 \text{Bank-Firm Distance}_{bf} + \epsilon_{bf}
\end{aligned} \tag{2}
$$

In Equation 2, the first component is a vector of observable characteristics multiplying a vector of parameters, and the second component is an error term representing the unobservable factors determining the matching value. Note that Equation 2 does not include bank- or firm-specific variables because these variables are absorbed by each other on the two sides of the inequality.

Fox (2018) specifies a "Rank Order Property" that assumes the decision/probability of matching is only determined by the observable characteristics. Thus, we can rewrite the inequality without the error term. The rank order property assures consistency of the parameters being estimated from the following inequality.

$$\boldsymbol{X}'_{b_1,f_1}\boldsymbol{\beta} + \boldsymbol{X}'_{b_2,f_2}\boldsymbol{\beta} \geq \boldsymbol{X}'_{b_1,f_2}\boldsymbol{\beta} + \boldsymbol{X}'_{b_2,f_1}\boldsymbol{\beta}$$

We then use all sets of actual matches in a given quarter to construct inequalities for all sample quarters.[13] Then, to estimate $\boldsymbol{\beta}$, a matching maximum score objective function is defined as follows.

$$\mathrm{M}(\boldsymbol{\beta}) = \sum_{q=1}^{Q} \sum_{[(b_1,f_1),(b_2,f_2)]\epsilon\mu_a} \mathbf{1}\left\{\boldsymbol{X}'_{b_1,f_1}\boldsymbol{\beta} + \boldsymbol{X}'_{b_2,f_2}\boldsymbol{\beta} >= \boldsymbol{X}'_{b_1,f_2}\boldsymbol{\beta} + \boldsymbol{X}'_{b_2,f_1}\boldsymbol{\beta}\right\}, \tag{3}$$

where Q is the number of quarters in the sample period, μ_q is all actual bank-firm matches in a given quarter q, and $\mathbf{1}\{\cdot\}$ is an indicator function. Estimation of the parameters should satisfy the inequalities —i.e., the pairwise stability— as much as possible. Thus, the optimisation problem is to find a set of parameters maximising

[13]When estimating the semiparametric model, we follow Schwert (2018) who drops all loan facilities with multiple lead arrangers.

M($\boldsymbol{\beta}$). Because M($\boldsymbol{\beta}$) is a step function, we use a direct search method. Specifically, we use the differential evolution algorithm, which is preferable to prevent settling on local maxima. Also, we use the subsampling procedure developed by Politis et al. (1999) to construct confidence intervals.[14]

Following Schwert (2018), we subtract quarterly averages from all firm- and bank-specific variables and set the parameter of Relationship Lending Dummy (i.e., β_3) to be 1000 (when we set β_3 to be -1000, the number of inequality satisfied is much lower, indicating that the model fits poorly). Thus, the estimates of other parameters indicate the importance of the characteristics relative to lending relationships. Similar to the regression coefficient, we expect $\hat{\beta}_1$ to be positively significant in the pre-GFC sample, meaning that the matching between well-capitalised banks and unrated firms is value-added. We expect an insignificant $\hat{\beta}_1$ in the post-GFC sample, implying the irrelevance of bank solvency to unrated firms and of unrated status to well-capitalised banks.

2.2.3 Summary Statistics

Table 2.1 reports the summary statistics of key variables used in this study in both periods of 2001-2006 (Panel A) and 2010-2019 (Panel B). We do not use a full pre-GFC sample here in order to maintain the comparability between the pre-and post-GFC periods. On average, banks and firms have a probability of 11.70% to match in the post-GFC sample, compared to 5.74% in the pre-GFC sample. This is partially due to a smaller lead arranger pool in syndicated loans after the GFC. According to firm-specific characteristics, post-GFC borrowers are, on average, larger, hold more cash assets, and have a higher leverage ratio. Moreover, the proportion of relationship lending has doubled after the GFC, while the distance between banks and firms

[14]Following Schwert (2018), we draw (without replacement) 100 subsamples of one-quarter of the full set of inequalities. The sampling distribution is

$$\boldsymbol{\beta}_s = \left(\frac{n_s}{N}\right)^{\frac{1}{3}} \left(\widehat{\boldsymbol{\beta}}_s - \widehat{\boldsymbol{\beta}}\right) + \widehat{\boldsymbol{\beta}},$$

where n_s is the size of the subsample, N is the number of all inequalities, $\boldsymbol{\beta}_s$ is the subsample estimate, and $\boldsymbol{\beta}$ is the full sample estimate.

remains similar. Banks have become larger, which could be attributed to mergers and acquisitions during and after the GFC and their organic growth as the U.S. economy grew along its trend. Also, banks hold more liquidity, which could be explained by stricter regulatory requirements post-GFC.

Insert Table 2.1 here.

2.3 The Growth in the Bond Market to Explain the Disappearing Matching

In this section, we first discuss the explanation for the disappearing matching and then introduce its testing implication.

2.3.1 The Rationales behind the Capital-based Matching and Its Disappearance

The matching between bank-dependent firms and well-capitalised banks was first documented in Schwert (2018) and subsequently confirmed by Farinha et al. (2022) using Portuguese loan-level data. In this section, we propose an explanation for the disappearing matching observed in Figure 2.1. That is, to explain why bank-dependent firms no longer prefer well-capitalised banks after the GFC over poorly-capitalised banks.[15]

We begin our discussion by examining the rationales behind the preference of bank-dependent firms for well-capitalized banks, a phenomenon that was particularly evident prior to the GFC. As mentioned in the previous section, bank-dependent firms exhibit two distinctive characteristics. First, the firms have difficulties accessing public financing sources, especially the public bond market. Thus, they rely on bank credit as a private source of funding that is more flexible (Darmouni and Siani, 2020). Second, the firms tend to be informationally opaque. Thus, they depend on banks'

[15] Note that the story depicted in this section stands in the borrowers' perspective. We also discuss how banks initiate the capital-based matching and drive the post-GFC disappearance in Section 2.6

screening and monitoring to lower the information dilution cost and obtain certificates from this (Fama, 1985; Ma et al., 2019).

The advantages of bank financing to bank-dependent firms are arguably more pronounced when banks maintain higher capital ratios. Higher capital adequacy enables banks to extend more lending, particularly in times of crises (Cornett et al., 2011; Berger and Bouwman, 2013; Gambacorta and Shin, 2018; Roulet, 2018). This avoids the potential termination of lending relationships. Given the difficulty in transferring private information (Rajan, 1992), coupled with the challenges faced by bank-dependent firms in switching to alternative funding sources, they are more likely to borrow from well-capitalised banks. Further, a higher capital ratio implies a greater commitment of a bank's own money to its profits, which intensifies the bank's stake in loan performance (Goodhart, 2013; Levine et al., 2020). Consequently, a well-capitalised bank will exhibit greater incentives for monitoring (Sufi, 2007; Ivashina, 2009; Allen et al., 2011). As bank-dependent firms experience more severe information asymmetry than other firms, they prefer well-capitalised banks over low-capital banks.

These advantages explain the existence of the capital-based matching pre-GFC. However, we propose that well-capitalised banks are less beneficial to bank-dependent firms after the GFC because the firms have better access to a non-bank funding source, given the growth in the bond market.

In Figure A.1 in Appendix A.2, we use data from FISD to plot the total number of issues and the total amount of issuance in the U.S. public market for each year. The figure reveals a notable surge in both variables after the GFC, making the issues and issuance largely exceed the pre-GFC levels. Prior studies such as Çelik et al. (2020) and Berg et al. (2021) also document post-GFC increases in the overall U.S. bond issuance and the amount of bond outstanding relative to GDP. The growth of the bond market is partly driven by the post-GFC low-interest rate environment, where the Federal Reserve substantially reduced the policy rate and maintained it for a prolonged period. The long-term rates also hit a historically low level due to large-scale asset-purchasing programs (Bikker and Vervliet, 2018). As a result, the U.S. experienced decreasing corporate bond yields (see Figure A.2 for the plots of corporate bond yields of both Aaa-grade and Bbb-grade firms). In line with the market timing theory (Baker and Wurgler, 2002), companies are more willing to issue bonds to capitalise on the low yields. Furthermore, the decreased interest rates, which have negatively impacted the performance of mutual funds, making these funds inclined to invest in non-investment grade bonds[16] This feeds bond-issuing opportunities to opaque and risky firms. Then, how do interest rates of bonds compare to loans? In Figure A.3, we plot two quarterly time series of average bond yields relative to loan yields. One (the solid black line) is calculated for investment-grade firms, and the other (the dashed red line) is for speculative-grade firms. In the figure, the bond yields fall faster than the loan yields after the GFC. This leads to a market condition that is more favourable for bonds than for loans. There are several reasons why loan rates fall slowly. First, the GFC impacted banks' balance sheets, inflating their funding costs relative to the policy rates (Kapuściński and Stanisławska, 2018), while the low-interest rate environment also narrowed banks' interest margins (Borio and Gambacorta, 2017). Both factors indicate that banks may increase loan rates to maintain more stable profits. Second, the GFC could have reduced banks' risk appetite, making them lift lending standards and increasing loan rates (Illes and Lombardi, 2013; Hasan et al., 2014). The last possible reason is that banks may have intentionally lifted their loan rates to restore their banks' capital positions or to comply with a new capital regulation (Kapuściński and Stanisławska, 2018).

With the expanding bond market and diminishing bond yields, it is easier and less costly for bank-dependent firms to transit away from banks.(Petersen and Rajan, 1995; Boot, 2000) The firms can use the bond issuing opportunities to buffer against potential lending reduction in the loan market. Thus, the commitment to sustained lending from those solvent banks becomes less valuable. Moreover, because bond issuance relies more on public information, the intensive monitoring from well-capitalised banks becomes less attractive as well (Datta et al., 1999; Ma et al., 2019). However, there is a cost associated with borrowing from high-capital banks. According to either the tax destructibility of debts or the pecking order theory, equity

[16]Becker and Ivashina (2015) document that mutual funds held 16% of corporate bonds in 2010.

capital is an expensive funding source. Thus, banks will likely pass the expenses of maintaining higher capital ratios onto borrowers Bolton and Freixas (2000); Schwert (2018). Trading off the reduced benefits and the cost makes a large proportion of bank-dependent firms choose not to borrow from well-capital banks. This results in the disappearance of capital-based matching during the post-GFC period.

2.3.2 A Testable Implication

The" bond market expansion" explanation implies that, after the GFC, unrated firms that can easily enter the bond market do not match with well-capitalised banks, the same as rated firms. This is because the firms can obtain a credit rating and enjoy growing bond opportunities and favourable market conditions. In contrast, unrated firms that face substantial costs of switching from loans to bonds will still prefer well-capitalised banks. Thus, they have a higher probability of matching with well-capitalised banks than rated firms after the GFC.

To test, we split unrated firms into two groups based on their ability to switch from loans to bonds. The first one is firm size. Larger firms can issue bonds more easily either because they have more assets to use as collateral or because more information about them exists in markets (Frank and Goyal, 2009; Bharath et al., 2011). By contrast, small firms lacking public status confront larger barriers to entering the bond market (Ma et al., 2019). We group unrated firms based on total assets for each quarter, with the small unrated group falling in the smallest quintile, thus facing the largest entry frictions to the bond market.

Different from the first proxy, which assesses a firm's current status, we calculate the second proxy by estimating the switching cost that an unrated firm would expect to pay for the transition from loans to bonds. Specifically, we estimate the yield by matching the firm to bond-issuing firms with similar characteristics. Estimating yields instead of spreads takes into account the post-GFC low-interest rate environment. We first group firms in both Dealscan and FISD into terciles based on each of the three financial variables in a given quarter. We use firm size, return on assets, and Tobin's Q to highlight the impact of cash follow and growth opportunities on the funding constraint (Lian and Ma, 2021). Then, we match each Dealscan firm with the FISD firm(s) in the same tercile for each financial variable as well as in the same industry (according to the two-digit SIC code). We average bond yields of all matched FISD firm(s) to proxy for the bond yields of the estimated Dealscan firm. We also calculate the loan rate of a Dealscan firm by averaging the all-in-drawn spreads plus 12-month LIBOR across all of the firm's loans in the quarter. The difference between the estimated bond and average loan rate is our measure of switching costs. We define a costless switch (costly switch) if the bond yield is lower (higher) than the loan rate.[17] Thus, if unrated firms expect that the bond is cheaper than the loan, they will care less about bank capitalisation. In contrast, a costly-switch unrated firm will be reluctant to switch from loans to bonds. Consequently, even if there is a bond market expansion after the GFC, those firms continue to rely on well-capitalised banks. In Table 2.1, the mean switching cost reduces by 0.5% after the GFC, consistent with our argument that the bond market growth reduces overall switching costs.

Lastly, in contrast to the second proxy, the third one looks at the history of the firm's financing. Specifically, we check the structure of a firm's outstanding debts in the previous year. On average, a firm has a 41.44% loan outstanding over total debt before originating a new loan. Firms that historically stick to loans are less likely to change funding sources by issuing bonds or other forms of debts (Denis and Mihov, 2003). We define a firm that heavily relies on bank financing if all of its outstanding debts are a loan at the end of the previous year. Thus, those firms are extremely loan-dependent, and we expected that they are more likely to borrow from well-capitalised banks. In our post-GFC sample, 22% of the unrated firms fully relied on loans at the end of the previous year.

For each proxy, we create a dummy for each group. Then, we add the interactions between the bank capital ratio and each of the dummies to the regression model and the semiparametric model. Note that this practice is effectively to add a triple interaction for each of the three proxies but without including the individual variable. Thus, it allows us to compare the matching probability of each group of unrated firms

[17]The results are similar if we use the median to split unrated firms.

with rated firms instead of making a comparison within unrated firms. The regression model is specified as follows.

$$
\begin{aligned}
\text{Observed Match Dummy}_{bft} = {} & \beta_1 \text{Bank Capitalisation}_{bt} \times \text{Hard-to-switch Unrated Dummy}_{ft} \\
& + \beta_1 \text{Bank Capitalisation}_{bt} \times \text{Easy-to-switch Unrated Dummy}_{ft} \\
& + \theta_1 \text{Bank Capitalisation}_{bt} \\
& + \theta_2 \text{Hard-to-switch Unrated Dummy}_{ft} \\
& + \theta_2 \text{Easy-to-switch Unrated Dummy}_{ft} \\
& + \sum \theta_i \text{Control Variables} + d_b + d_f + d_t + u_{bft} \qquad (4)
\end{aligned}
$$

Except for the first two interaction terms, the variables are the same in Equation 4 and in Equation 1. Hard-to-switch Unrated Dummy takes one if an unrated firm falls into the bottom size quintile, faces a higher issuing cost of bonds than loans, or borrowed entirely from loans. Thus, β_1 measures the incremental probability for unrated firms that face high switching costs to match with well-capitalised banks relative to rated firms. We run the regression in the post-GFC sample and expect β_1 to be significantly positive. Also, we expect β_2 to be insignificant, indicating that there is no higher probability of observing a match between well-capitalised banks and unrated firms that face low switching costs.

Similarly, we adjust Equation 2 by replacing Bank Capitalisation$\times$Unrated Dummy with interactions between the bank capital ratio and each of the group dummies and estimate the maximum score function during 2010-2019.

2.4 Empirical Results

This section presents empirical results from the regression and semiparametric matching models.

2.4.1 The Disappearing Matching between Bank-dependent Firms and Well-capitalised Banks

Table 2.2 presents the results of testing the disappearing matching using the regression model (Columns 1-2) and the semiparametric model (Columns 3-4). We multiply OLS coefficients by 100, so the coefficients are interpreted as the impacts on the probability percentage of bank-firm matching. The first column reports the coefficients in a pre-GFC sample. The interaction between bank capitalisation and the unrated dummy has a significantly positive coefficient, confirming the observation of the capital-based matching in Schwert (2018). Specifically, in the pre-GFC period, we have a 0.32% (which equals one standard deviation of the unconditional matching probability) higher probability of observing a matching if the firm is an unrated firm and the bank has a one-standard-deviation higher capital ratio. Turning to the second regression in the post-GFC sample, the coefficient of the capital-rating interaction becomes insignificant.[18] Thus, during the post-GFC period, unrated firms are not more likely to match with high-capital banks than rated firms. Overall, the regression results are consistent with the finding in Figure 2.1, and thus, we confirm the disappearance of capital-based matching after the GFC.

Regarding the reported control variables, except for the size interaction, all other variables have the expected coefficients. Moreover, the statistical significance of these coefficients remains the same before and after the GFC. For example, both regressions show that a lender and a borrower are more likely to match if they have a tighter relationship and are closer in geographical distance. However, the magnitude of the distance coefficient is smaller in the pre-GFC sample than in the pre-GFC sample. Short distance facilitates the collection of soft information through, for instance, in-person visits (Agarwal and Hauswald, 2010; Gustafson et al., 2021). Liberti and Petersen (2019) point out the trend of "hardening" soft information; the rising pop-

[18]We have also used another regression specification by interacting Bank Capitalisation $\times$ Unrated Dummy with a dummy representing the post-GFC period, and run it in the full sample. The F-statistic of testing whether the sum of the coefficient of Bank Capitalisation $\times$ Unrated Dummy and the coefficient of the triple interaction term is 2.06, and the corresponding p-value is 0.157. This confirms that, during the post-GFC period, there is no capital-based matching.

ularity of "FinTech" and "Robo-advising" can be examples. Thus, less reliance on soft information can explain the smaller impact of distance in Column 2. In Section 2.7, we use logit regressions to check the robustness of the results and obtain similar coefficients, especially confirming the post-GFC disappearance.

The next two columns show estimates from the semiparametric model. The results also confirm the disappearing matching. In the pre-GFC sample, the interaction between bank capitalisation and the unrated dummy has a significant and positive coefficient.[19] However, the coefficient becomes insignificant in the post-GFC sample. As shown in the square bracket, the 95% confidence interval of the capital-unrated interaction contains zero, indicating that the combination between well-capitalised banks and unrated firms does not add value to a bank-firm matching after the GFC. It is important to note that the estimates of all other variables significantly impact the value of a bank-firm matching. Especially, the size interaction presents significant and positive coefficients, meaning that the match of small firms with small banks after the GFC continues to add value in these bilateral relations (Berger et al., 2017). Therefore, the disappearing matching is **a distinct finding** from the findings related to small firms and small banks highlighted in Berger et al. (2017) and other studies. The model fit is reported as the fraction of satisfied inequalities, which is over 90% in both samples. This indicates that the variables we include in the model explain well why a certain firm matches with a certain bank in terms of pairwise stability.

Insert Table 2.2 here.

2.4.2 Unrated Firms Facing High Switching Costs Are More Likely to Match with Well-capitalised Banks in the Post-GFC Period

We propose that the bond market expansion during the low-interest rate period drives the disappearing matching. We hypothesise that only the unrated firms facing the

[19]The number of inequalities in Column 3 is larger than that in Schwert (2018), whose sample period is 1987-2012. This could be because the definition of lead arrangers in our paper is different from Schwert (2018), resulting in larger numbers of banks and firms in our sample. During 1987-2012, we have, in total, 64 banks and 4993 firms.

largest frictions to switch from loans to bonds still match with well-capitalised banks, while those with smaller frictions no longer do so after the GFC. Thus, we separate unrated firms into two groups and include two interactions in the empirical models. We estimate the models in the post-GFC period, and Table 2.3 presents the results.

In column 1, the interaction term between the small unrated dummy and the capital ratio has a significantly negative coefficient. It shows that the smallest unrated firms have a 1.5% higher probability of borrowing from banks with (a one-standard deviation) higher capital ratios than rated firms. The result is consistent with our expectation that the smallest unrated firms still prefer more solvent banks because they are too small to enter the bond market. Also, because smaller unrated firms are more bank-dependent, they would have a stronger preference for a high-capital bank than an average unrated firm. This explains why the magnitude is five times the coefficient of the pre-GFC matching in Table 2.2. On the contrary, the coefficient between the large unrated dummy and the capital ratio is insignificant, indicating that there is not a higher probability of observing a matching between well-capitalised banks and large unrated firms. In Column 4, we confirm the finding using the semiparametric model. We can interpret the estimates by trading off the factors affecting the matching value. Specifically, a small unrated firm is willing to sacrifice, for example, geographical proximity to borrow from a well-capitalised bank, while a large unrated firm would not do so.

Then, we use estimated switching costs to split unrated firms, and Column 2 presents the result.[20] The interaction term of the costly-switch unrated dummy has a significantly positive coefficient. Thus, unrated firms that face high switching costs are more likely to match with well-capitalised banks than rated firms after the GFC. In the next row, the interaction between the costless-switch unrated dummy and the capital ratio has an insignificant coefficient. Thus, if unrated firms can issue bonds at a lower price, they will no longer prefer well-capitalised banks. The semiparametric model presents a similar finding in Column 5.

Column 3 reports the results using the measure of a firm's debt structure. The

[20]The reduction in the number of observations is because some Dealscan firms do not have matched FISD firms that satisfy the criteria.

coefficient of interaction between the loan-heavy unrated dummy and the capital ratio is positive, while the second interaction is insignificant. Thus, after the GFC, the matching only exists between well-capitalised banks and unrated firms that have borrowed predominantly from loans. Again, the semiparametric model returns the same finding. In all specifications, the control variables have the same expected signs as in the previous table.

Insert Table 2.3 here.

2.4.3 Unrated Firms that Do Not Rely on Well-capitalised Banks Prepare for the Transition

The preceding findings reveal that unrated firms that are capable of switching from loans to bonds do not depend on well-capitalized banks. Then, are they actively preparing for a transition to the bond market? We address this question by presenting an additional finding. A credit rating is a prerequisite for issuing public bonds. Thus, we test whether unrated firms borrowing from low-capital banks are more likely to obtain a credit rating compared to their counterparts relying on well-capitalized banks. We include all unrated firms' loans during 2010-2019. For each of these loans, we identify whether the borrower obtained a credit rating after the loan origination and create a dummy (Obtain Rating Dummy) accordingly. In total, 217 firms obtained a rating during the post-GFC period. To resolve the fundamental differences between the two groups, we use propensity score matching to match each loan whose borrower obtained a rating with two loans whose borrows did not. The matching criteria include Altman's z-score, asset tangibility, profitability, cash holdings, leverage, Tobin's Q, IPO years, industry, state, loan types, loan purposes, and loan maturity. In this matched loan-level sample (the sample size is 2,463), we then regress Obtain Rating Dummy on the bank capital ratio. Figure 2.2 plots a fitted line of the logit regression. The downward slope supports the expectation. Compared to those borrowing from a well-capitalised bank, an unrated firm that borrows from a low-capital bank has a higher probability of issuing a credit rating, suggesting that they are preparing to switch to the bond market.

Insert Figure 2.2 here.

2.5 The Impact of the Disappearing Matching In the COVID-19 Shock

2.5.1 The Testing Procedure on the Impact of the Disappearing Matching

So far, we have used bond market expansion to explain the disappearing matching and test unrated firms' willingness to transition to bonds. Then, what is the consequence of the disappearing matching? Schwert (2018) document that capital-based matching enhances the efficiency of credit allocation by directing the money from the surplus side (i.e., well-capitalised banks) to the deficit side in a crisis (i.e., bank-dependent firms). Given that we do not observe capital-based matching after the GFC, does this phenomenon deteriorate credit allocation? To address this question, we estimate the access of unrated firms to loans during the COVID-19 shock in two scenarios – when capital-based matching vanishes and when capital-based matching is reinstated. The former scenario serves as our benchmark, derived from observed matches, while the latter scenario is a counterfactual where we artificially adjust the coefficient of Bank Capitalisation$\times$Unrated Dummy to be significantly positive. By comparing the two scenarios, we can test whether unrated firms are better off by instead matching with well-capitalised banks.

The literature suggests that firms, especially bank-dependent firms, tend to borrow from their relationship banks in crises (Santos and Winton, 2008; Dewally and Shao, 2014; James et al., 2021). Also, Bolton et al. (2016) find that, in a crisis time, relationship lending has more informational advantages and provides favourable loan conditions than transaction lending. Thus, we identify an unrated firm's relationship bank before the COVID-19 shock (we define the COVID-19 period as 2020Q2-2020Q4 which is the most severe year) and calculate the growth rate of the relationship bank's total syndicated loan amounts before and after the shock as the measure of the firm's loan access. If the relationship bank significantly cuts lending, we expect the unrated firm to suffer from severe credit constraints. Therefore, we assert that the disappearing matching has a negative impact if unrated firms' lending access is significantly lower in the benchmark than in the counterfactual scenario.

We define the relationship bank as the lender that an unrated firm matched with in the most recent quarter before the COVID-19 shock, and the bank is different in the two scenarios. Under the benchmark, the relationship bank is the lender observed in the unrated firm's most recent loan contract in Dealscan. Under the counterfactual, we adjust parameters in the semiparametric matching model estimated in the pre-COVID sample during 2015Q1-2019Q4 and assign a bank to the unrated firm in the most recent quarter (we do not estimate the full pre-COVID (i.e., post-GFC) sample because lending relationships are conventionally defined in the past five years (see Schenone, 2010)). To ensure the bank is well-capitalised, we let Bank Capitalisation $\times$ Unrated Dummy be positive. We employ two strategies. First, we use the parameters estimated from the **pre-GFC** sample during 2002Q3-2007Q2. Under this counterfactual, firms follow the same matching practices as they did pre-GFC. Because capital-based matching is observed before the GFC, the relationship bank assigned to an unrated firm should be well-capitalised. Second, we take the absolute value of $\hat{\beta}_1$, and thus, it restricts unrated firms' pre-COVID relationship banks to having high capital ratios while holding all else equal. Next, we use the adjusted parameters in either strategy to estimate the matching value using Equation 2. Then, a firm's relationship bank is the one that generates the highest matching value among all bank-firm matches in the most recent quarter. The testing procedure is summarised Appexdix A.4.1 in five steps.

2.5.2 The Disappearing Matching Does Not Have a Detrimental Impact on Unrated Firms during the COVID-19 Shock

To perform the test, we first estimate Equation 3 from 2015Q1 to 2019Q4. Table A.2 in Appendix A.3 presents the results. The results are similar to Table 2.3. The coefficient of the interaction between bank capitalisation and the unrated dummy is insignificant during 2015Q1-2019Q4 and significantly positive during 2002Q3-2007Q2.

Next, we compare unrated firms' loan access in the COVID-19 shock under two scenarios, and the results are presented in Panel A of Table 2.4. The left side specifies the ways to define the relationship bank that an unrated firm borrows in the COVID-19 period. The number in the first row illustrates our benchmark, where unrated firms are matched with the banks observed in actual loan contracts. The result shows that if unrated firms borrow from their actual relationship banks during the COVID, they will have an average 69% reduction in loan access[21]. Then, we report the difference in loan access between the benchmark and each counterfactual in the Column "Benchmark – Counterfactual (%)". In the first counterfactual, we use the estimates from Column 2 in Table A.2. The result indicates that if unrated firms replicated what they did before the GFC, they would have a 1.22 percentage points lower reduction in lending growth relative to the benchmark. However, the difference is not statistically significant and considerably small. In the alternative counterfactual, we take the absolute value of $\hat{\beta}_1$ and keep the other estimates unchanged. Similarly, we find an insignificant difference in loan access. Therefore, the results suggest that, during the COVID-19 shock, there would have been no significant loss in credit availability if unrated firms chose not to borrow from well-capitalised banks.

Subsequently, we compare the insignificant results in the COVID-19 period to the impact of the disappearing matching on loan access during the GFC in Panel B. We replicate the same procedure, but now, the benchmark scenario is based on the

[21]the number is much smaller at 33%, as reported in Table A.3 where we calculate the lending growth by splitting the amount of each loan equally based on the number of lenders. This indicates that during the COVID-19 shock, the syndicate size shrank significantly. However, the main findings are the same in Table CH1C2. Moreover, the benchmark result in the GFC is 57.66%.

existence of capital-based matching. In the benchmark where unrated firms follow observed matches, the average lending reduction is 62.29% during the GFC. Then, we create two counterfactual analogising to the disappearing matching – making bank capitalisation irrelevant to unrated firms' choices of matching partners. To achieve this, we either replace the estimates with the **pre-COVID** estimates in Column 1 in Table A.3 or "shut off" $\hat{\beta}_1$ by setting it to be zero. The last column in Table 2.4 shows that the loan access in both counterfactual scenarios is significantly lower than the benchmark. For example, if the firms replicate the matching as they will do in the pre-COVID period, they would face a 3.49 percentage points reduction in loan supply in the GFC. Not only is this figure statistically significant, but it also triples the difference observed in the COVID-19 estimation. Thus, whether matching with well-capitalised banks or not leads to a considerable difference in credit supply during the GFC period. This is consistent with (Schwert, 2018). Moreover, the insignificant difference in loan access during the COVID-19 shock is not caused by the lack of variation in lending growth: in 2020, the standard deviation of bank lending growth is 23.83, which is higher than the 18.08 during the GFC.

To summarise, the pre-GFC prominent matching matters for the credit allocation, while the post-GFC disappearing matching does not lead to worse consequences during the COVID period.[22]

Insert Table 2.4 here.

2.5.3 Stricter Capital Requirements Improve Low-capital Banks' Ability to Provide Lending in the COVID-19 Pandemic

The results indicate that the disappearing matching does not have a detrimental impact on unrated firms. Although our bond market story suggests that bank-dependent

[22]It is understood that the COVID-19 shock is not a banking crisis like the GFC, and this may affect our interpretation of how bad the disappearing matching could be. However, we argue that the findings are still meaningful in two points. First, the COVID shock actually reduced aggregate demand and slowed down the economy, so credit availability was still a concern for bank-dependent firms. Second, the COVID-19 shock is the most recent testable shock representing a comparison with the GFC, and regardless of its origin and severity, the disappearing matching does not produce significant impacts on unrated firms' loan access.

firms can utilize bond access to mitigate the potential reduction in the loan market, it does not assert a difference or indifference if capital-based matching is still in place. Therefore, we put forth an explanation to interpret the findings in the previous subsection.

After the GFC, the U.S. implemented a set of stricter capital requirements, leading to a substantial improvement in the overall capital adequacy of the banking industry after the GFC.

The new capital requirements include stress tests as well as the Basel III. Starting in 2009, the Federal Reserve introduced three stress test programs, including the Supervisory Capital Assessment Program (SCAP), the Comprehensive Capital Analysis and Review (CCAR), and the Dodd-Frank Act Stress Test (DFAST). The programs assess banks' capital levels under worse-than-expected scenarios and require banks to submit plans on capital distributions. Also, if a bank fails the tests, the Fed would ask it to adjust and re-submit its capital plan promptly to improve its capital adequacy. Further, not only lifting capital levels, Basel III also enhances banks' capital quality by increasing the minimum requirement on the common equity tier-one capital ratio and limiting the proportion of the lower-tier capital. Additionally, Basel III requires systemically important banks (SIBs) to hold extra capital buffers. Because the sample used in this study comprises large banks, a majority of them (in 99% loan-level observations) are included in the stress test programs and classified as SIBs.

As a result, the improved capital adequacy renders low-capitalized banks sufficiently solvent to sustain continuous lending during a crisis. This explains the observed insignificant difference between borrowing from low- and high-capital banks. Li et al. (2020) make a similar argument by showing that the equity level does not affect bank lending during the COVID-19 pandemic. They explain that banks have accumulated "enough capital" after the GFC. Figure A.4 further supports the argument. We split banks into three groups based on their market capital ratios in a given quarter and plot the quarterly average capital ratio for both top and bottom terciles. It shows a smaller difference in the capital ratio from the pre-GFC to the post-GFC periods.

2.6 A Supply-side Explanation for the Disappearing Matching: "Chase for Yields"

Our exploration has predominantly centred on the borrower's standpoint to explain and test why there is a departure from unrated firms for well-capitalized banks. However, it is essential to acknowledge the role of the supply side in shaping capital-based matching. The result from the semiparametric model implies that the matching between unrated firms and well-capitalised banks no longer added value not only for borrowers but also for lenders. Therefore, in this section, we delve into a plausible supply-side explanation for the disappearing matching, attributing it to the prolonged low-interest-rate environment and tighter capital requirements. Section 2.6.1 elaborates on the story. Following that, Section 2.6.2 introduces a testing implication and presents the results. In Section 2.6.3, disentangle the supply-side factor from the demand-side factor (i.e., the bond market expansion), aiming to discern the relative importance of each factor.

2.6.1 The Supply-side Rationale behind the Capital-based Matching and Its Disappearance

For two reasons, well-capitalised banks are incentivised to make loans to bank-dependent firms as opposed to rated firms. First, a higher capital ratio enhances the capacity to absorb losses, placing well-capitalized banks in a superior position to deal with uncertainty and asymmetric information associated with lending to risky and opaque firms. Secondly, in the context of a syndicated loan, where the lead arranger holds only a portion of the loan but assumes responsibility for information gathering, there could be a lack of incentives for the bank to engage in monitoring (Ivashina, 2009; Sufi, 2007). This moral hazard issue is particularly severe when lending to an unrated firm whose creditworthiness is difficult to assess. As higher capital ratios encourage monitoring, well-capitalized banks are better positioned to arrange loans to unrated firms.

However, we argue that the incentives of poorly-capitalised banks to extend loans to unrated firms become stronger after the GFC. The post-GFC introduction of stricter capital regulations imposes regulatory pressure on low-capital banks, compelling them to replenish capital (Gambacorta and Mistrulli, 2004; Fonseca and González, 2010). At the same time, the prolonged period of low-interest rates constrains banks' earnings potential (Acharya et al., 2019; Schivardi et al., 2022). The combination of the two conditions prompts poorly-capitalised banks to pursue short-term earnings, a behaviour of "chase for yield". Consequently, these banks adopt a more aggressive lending stance toward bank-dependent firms, seeking to extract information rents and higher risk premiums. Therefore, the larger incentive to chase for yield crowds out a part of the matches between well-capitalised banks and unrated firms, eventually driving the disappearing matching.

2.6.2 Risky Banks Are More Likely to Lend to Unrated Firms to Chase for Yield

As in the supply-side explanation, the low-interest rate environment impedes banks' interest income, making low-capital banks pursue short-term earnings by aggressively lending to unrated firms. We test for the possibility of "chase for yield" by asking whether risky banks are likely to match with unrated firms. Similar to poorly-capitalised banks' intention to replenish capital, risky banks search for yields to meet target returns required by shareholders (Becker and Ivashina, 2015; Acharya and Naqvi, 2019).

As follows, we include an interaction between bank riskiness and the unrated dummy in Equation 1.

$$
\begin{aligned}
\text{Observed Match Dummy}_{bft} = {} & \beta_1 \text{Bank Riskiness}_{bt} \times \text{Unrated Dummy}_{ft} \\
& + \beta_1 \text{Bank Capitalisation}_{bt} \times \text{Unrated Dummy}_{ft} \\
& + \theta_1 \text{Bank Riskiness}_{bt} + \theta_2 \text{Bank Capitalisation}_{bt} \\
& + \theta_3 \text{Unrated Dummy}_{ft} + \sum \theta_i \text{Control Variables} \\
& + d_b + d_f + d_t + u_{bft} \qquad (5)
\end{aligned}
$$

We run the regression in the post-GFC period. The variable of interest is Bank Riskiness $\times$ Unrated Dummy, while other variables are the same in Equation 1. We expect a significant coefficient of the interaction and a sign that is consistent with a higher probability of observing matching between risky banks and unrated firms. We use three proxies for banks' riskiness. One is non-performing assets over total assets, reflecting the riskiness in a bank's total asset portfolio. The second proxy is loan loss provision over gross loans, capturing a bank's expectation of lending risk. The last one is liquidity assets over total assets, proxying for a bank's liquidity risk. A bank lacking liquidity is potentially at risk of a bank run (Gorton and Metrick, 2012). Also, the new liquidity requirements in the Basel III framework also need banks to increase their liquidity positions. Thereby, banks with low liquidity ratios are likely to chase for yield. In our analysis, the risk measures are lagged by one quarter to avoid any accounting-related change in the figures - resulting from increasing exposures to unrated firms - that may drive the results. Equation 5 keeps the interaction between the capital ratio and the unrated dummy because there is a high correlation between banks' financial variables.

Similarly, we estimate the semiparametric matching model by adding Bank Riskiness $\times$ Unrated Dummy in Equation 2.

Table 2.5 presents the results. The evidence in the first OLS regression shows the matching between risky banks and unrated firms existing after the GFC. From the first column, the matching probability increases by about 1% if the firm is unrated and the bank has a one-standard-deviation higher non-performing asset ratio. In the next two columns, the coefficient of the interaction between the unrated dummy and either loan loss provisions or liquidity ratios has the anticipated sign and is significant at the 10% level. Thus, banks prefer unrated firms over rated firms when they anticipate a higher likelihood of encountering bad loans or when they lack liquid assets. The semiparametric model also confirms the post-GFC matching between unrated firms and banks with high asset risks or credit risks. Moreover, the interaction between the liquidity asset ratio and the unrated dummy has an expected negative sign, though statistically insignificant. Overall, the results confirm the matching between risky banks and unrated firms in the post-GFC period. It suggests that the incentive of "chase for yield' motivates banks to lend to unrated firms. Thus, we deem it to be a plausible supply-side driver for the disappearing capital-based matching.

Insert Table 2.5 here.

2.6.3 Isolating the Supply-side Factor from the Demand-side Factor

Our results show that both bond market expansion and chasing for yield can potentially drive the disappearing matching. To test the relative importance of each factor, we isolate the supply-side factor from the demand-side factor based on banks' deposit ratios.

Conventionally, deposits are priced as a markdown on market rates. Thus, when interest rates are sufficiently low, banks are reluctant to reduce deposit rates to cross the zero lower bound (Borio and Gambacorta, 2017). However, as banks are likely to lower their loan rates to maintain competitiveness in the low-interest rate environment, the interest margin becomes narrow (Claessens et al., 2018; Molyneux et al., 2020; Lopez et al., 2020). Accordingly, If a bank holds a low level of deposits, the bank's potential to suffer from the reduced profit is minimised (Heider et al., 2019). Then, the bank has little incentive to pursue yields, implying a minor role of the supply-side factor. Therefore, if the supply-side factor solely explains the disappearing matching, we would expect that the capital-based matching be restored among banks with low deposits. Oppositely, if the capital-based matching is still not observed, we deem that the demand-side driving factor is necessary. Then, for banks that rely heavily on deposits, the heightened incentives of chasing for yield will likely dominate the matching between low-capital banks and unrated firms.

To test, we separate the sample into two based on the ratio of bank interest-bearing deposits over assets. The "High Deposit Ratio" sample includes banks falling into the top tercile while the "Low Deposit Ratio" sample contains the rest. Then, we estimate both Equation 1 and Equation 3 in the two subsamples. We expect a reverse in the capital-based matching in the "High Deposit Ratio" sample – a negative coefficient of

Bank Capitalisation × Unrated Dummy. Also, we expect an insignificant coefficient in the "Low Deposit Ratio" sample.

Table 2.6 presents the results. In the "High Deposit Ratio" sample (Column 1), the coefficient of the interaction term is negative and significant at the 10% level. This confirms that the supply-side factor dominates among banks with high deposit ratios. However, it is notable that the coefficient of the interaction is insignificant in the semiparametric model, possibly because the low number of inequalities reduces the precision of estimation.[23]. Next, when the incentive of chasing for yield is diminished in the "Low Deposit Ratio" sample, we do not find significant coefficients of the interaction term in both models. This suggests the importance of the bond market expansion in explaining the disappearance of capital-based matching.

Insert Table 2.6 here.

2.7 Further Discussions and Robustness Test

In this section, we conduct further tests and robustness checks for the previous results.

2.7.1 Addressing Concerns on the Finding of The Disappearing Matching

In this subsection, we discuss the robustness of the finding of the disappearing matching.

In our main empirical tests, we include all bank-firm matches. Some firms choose to borrow from their prior lenders. To those firms, past lending relationships could be the first-order consideration in forming the matching, while bank capitalisation may become less relevant. If the disappearing matching is the result of borrowers choosing to borrow from their relationship banks, the explanations we proposed are less convincing. In our main tests, we rule out this possibility by including lending relationships as a control. In this subsection, we test the capital-based matching

[23]The estimation of the model requires two different banks in an inequality. When we partition banks into terciles, the number of inequalities reduces sharply.

by directly excluding all loans where the bank and the firm have prior relationships during the pre-GFC period. We report the results in Columns 1&2 in Table 2.7. Column 1 reports the results from the post-GFC OLS regression, and the sample reduces by about 40% relative to the sample where we include all loans. Interestingly, the coefficient of the interaction term is significantly negative. This implies a matching between unrated firms and poorly-capitalised banks, consistent with our supply-side explanation. The reason for the negative coefficient could be that we exclude all pre-GFC relationship loans where the unrated firms match with well-capitalised banks in the past. Then, in the semiparametric model in Column 2, however, the coefficient is insignificant, suggesting the disappearing matching.

Next, we test our finding of the disappearing matching at the intensive margin. Our previous test on the probability of matching between unrated firms and well-capitalised focuses on the extensive margin. To test the intensive margin, we ask, conditional on matching, whether unrated firms borrow more from well-capitalised banks than from poorly-capitalised banks using the loan-level data. Specifically, we regress loan amount (in logarithms) on the interaction between bank capitalisation and the unrated dummy with firm-fixed effects and bank-fixed effects. The last two columns in Table 2.7 present the results. In the pre-GFC sample, the interaction term coefficient is significantly positive. An unrated firm's loan is 7.6% larger if the firm borrows from a well-capitalised bank whose capital ratio is one standard deviation higher than the average. In the next column, the coefficient becomes insignificant. Thus, after the GFC, unrated firms no longer borrow more from well-capitalised banks than from low-capital banks. This confirms the disappearing matching at the intensive margin.

Insert Table 2.7 here.

Lastly, recent papers such as Aldasoro et al. (2022, 2023) have documented the increasing participation of shadow banks (e.g., mutual funds and investment banks) in syndicated loans. These studies point out that shadow banks' loan portfolios differ from those of commercial banks. Also, shadow banks are more likely to provide transaction lending than relationship lending, so unrated firms may care less about these banks' capitalisation and the disappearing matching could be caused by the rising participation of shadow banks. According to the Dealscan item institution type, we find that only about 0.63% loan facilities have shadow banks as lead arrangers (see Aldasoro et al. (2022, 2023) for definitions of shadow banks). Thus, although we acknowledge that shadow banks' participation could affect bank-firm matching, it appears quantitatively less relevant in our study: while participation of non-banks in the market has increased relative to historical experience, it is still negligible.

2.7.2 The Bond Outstanding of Unrated Firms that Borrow from Low-capital Banks

Our story of the "bond market expansion" predicts that unrated firms that do not rely on well-capitalised banks will use bonds to finance a potential new source of funding beyond the loan market. We have already shown that these unrated firms are switching to the bond market by obtaining credit ratings. We now test whether their bond borrowing during the COVID increases more than that of the unrated firms who still borrow from well-capitalised banks. To test this, we calculate the changes in each unrated firm's debt outstanding for different debt types from the pre-COVID period (averaging across 2017-2019) to the end of 2020. We then regress these changes on bank capitalisation. Table 2.8 presents the results. The coefficient in Column 1 is significantly negative. Thus, unrated firms that borrow from low-capital banks increase their bond outstanding more than those borrowing from high-capital banks. A one-standard-deviation decrease in the capital ratio will increase the change in bond outstanding by about 7.66%. We then decompose the total outstanding bond into senior bonds and notes as well as subordinated bonds and notes. This decomposition shows that the larger increase in bond outstanding of unrated firms that borrow from low-capital banks during the COVID crisis mainly comes from an increase in senior bonds and notes. Although the coefficient in the regression of subordinated bonds and notes is positive, the result is likely, not representative because only less than 10% observations have non-zero changes in subordinated bonds and notes. We also test the impacts of bank capitalisation on changes in loan outstanding. Note that this is not a counterfactual scenario in Section 2.5; it is instead an observed change in loans given there is disappearing matching. The coefficient in the regression of revolving credit is significantly positive. Therefore, unrated firms that borrow from low-capital (high-capital) banks rely more on bonds (loans) than loans (bonds) to finance themselves during the COVID-19 shock. This finding is consistent with our "bond market expansion" explanation.

Insert Table 2.8 here.

2.7.3 Logit Regressions

Next, we check the robustness of our results by replacing OLS with logit estimators in Equation 1. Using a logit model restricts the probability of matching within 0-1 and allows for non-linearity but makes the interpretation of the coefficients of interaction terms more difficult. Table 2.9 presents the results, and we report the coefficients in the form of log-odd ratios. The logit results generally confirm the findings established in the previous sections. First, we document the disappearing matching by comparing the coefficient of the interaction between bank capitalisation and the unrated dummy in the pre-GFC sample to that in the post-GFC sample. The first coefficient is positively significant at the 1% level, while the second one is insignificant. Then, supporting the "bond market expansion" explanation, the next three columns show that while the unrated firms that have better access to other funding sources care less about bank capitalisation, most bank-dependent firms still prefer well-capitalised banks. In the rest of the table, we find evidence supporting the supply-side factor. The coefficient of the interaction between the non-preforming asset ratio and the unrated dummy is significantly positive. For other risk measures, the sign is consistent with the prediction of the matching between unrated firms and either banks with high credit risks or with high liquidity risks, although they are statistically insignificant.

Insert Table 2.9 here.

2.8 Conclusion

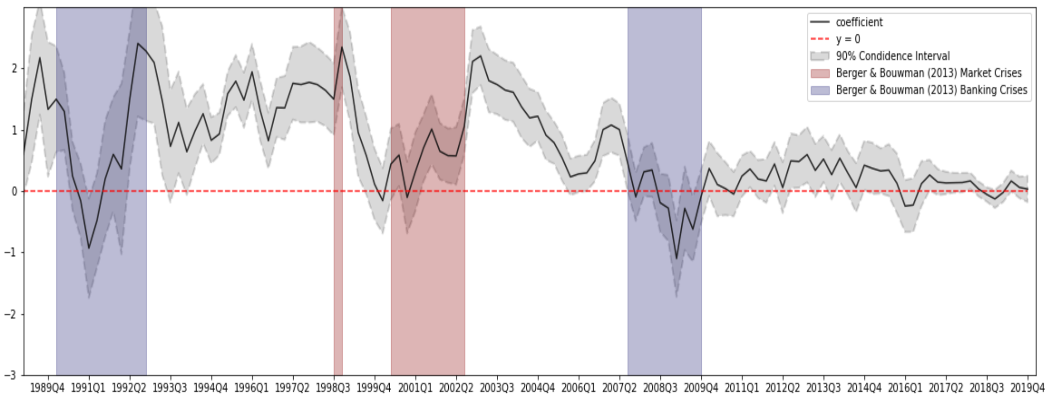
In this paper, we study changes in the matching structure between firms and banks in the syndicated loan market. We find that the previously documented matching between bank-dependent (unrated) firms and well-capitalised banks has essentially disappeared since 2010. We argue that the post-GFC prolonged low-interest environment provides firms with better access to the bond market, making bank solvency less of a concern to bank-dependent firms. The low-interest rates were a consequence of the accommodating monetary policy after the GFC. The policy rates started to rise gradually in 2016, but at the same time, the trend of reducing bond yields continued until 2021. Given the recent developments and surges in monetary policy, a natural follow-up question is whether the disappearing matching is a permanent phenomenon or a temporary response to the easing monetary policy, and therefore, whether there is a strong cyclical component of the capital-based matching or this has become a permanent feature of the syndicated loan market. We suggest that further studies can re-evaluate the capital-based matching after the rise of interest rates in 2022 when reliable data returns a sufficient post-COVID sample to explore this research question. Our findings suggest that prolonged low interest rates and the growth of the bond market ease firms' credit constraints by reducing their dependence on well-capitalized banks. This implies that the transmission of accommodating monetary policy can be more effectively achieved if it aligns with favorable bond market conditions. One recent example is the Federal Reserve's support for the bond market, including the primary dealer credit facility and the secondary market corporate credit facility, during the COVID-19 shock. The impact of bond market growth on firms' credit constraints could be even more significant in countries where firms rely more heavily on bank financing.

Then, we test the consequence of the disappearing matching by creating a counterfactual if capital-based matching exists after the GFC. We find that unrated firms' loan access in the COVID-19 shock is not improved even if they change their matching strategy to well-capitalised banks. We explain that the implementation of stress tests and Basel III capital requirements after the GFC ensures low-capital banks' solvency to maintain their lending in a crisis. An implication of this finding is that stricter capital requirements can promote financial stability by mitigating the adverse consequences of disordered bank-firm matching. While we focus on the U.S. in this study, we note that after the GFC and the European debt crisis, many other countries adopted Basel III and introduced novel stress test requirements. Future research will be able to explore matching in an international context. The different timeline for implementing capital requirements in different courtiers provides some cross-country variations to test whether improved capital adequacy was a driving factor in the disappearance of matching.

Moreover, we also propose a supply-side explanation for the disappearing matching, arguing that the low-interest rates erode banks' profitability and encourage low-capital banks to match with unrated firms. Because the supply-side and the demand-side factors are not mutually exclusive, we differentiate their impacts using banks' deposit rates and find both to be important to drive the weak matching.

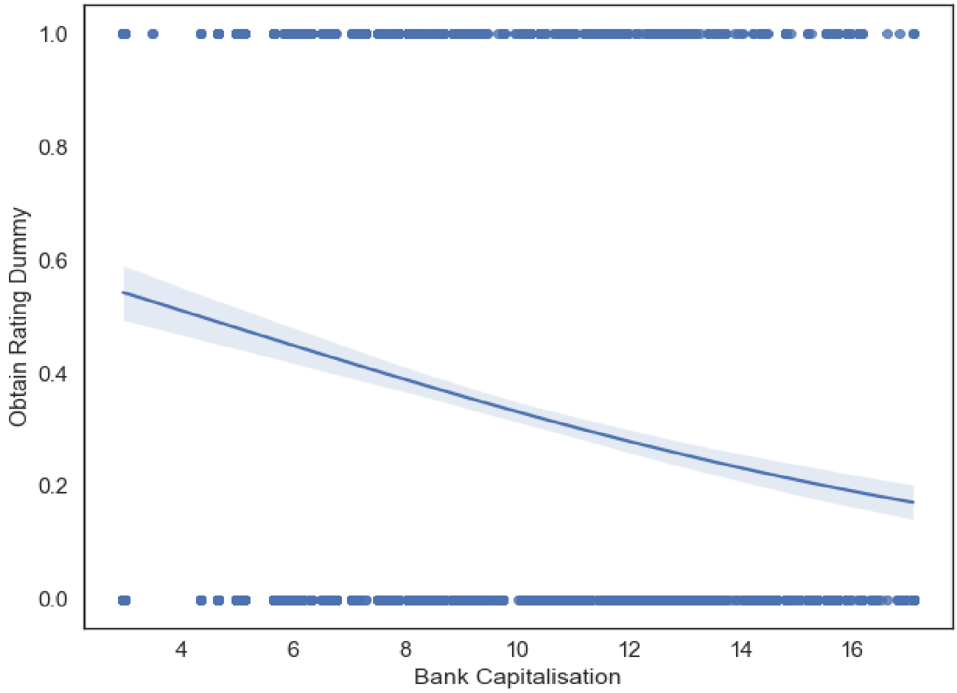
Last but not least, a recent study by Papoutsi and Darmouni (2022) document a sharp increase in bond issuance in the European bond market from small and unrated firms since 2010. However, the authors also find that banks' share of the unrated bond issuances is considerably larger than their holdings of rated bonds. This indicates that the lending relationships between firms and banks could have extended to the bond market. If a similar phenomenon is found in the U.S., it can be an important complement to our findings, such that capital-based matching is not only disappearing in the syndicated loan market but also shifting to the bond market. Thus, we suggest future works on whether there is a matching between bank-dependent firms and well-capitalised banks existing in other financing markets such as bonds or commercial paper.

Figure 2.1: A Plot of the relationship between Bank Capitalisation and Unrated Status Over Time



This figure illustrates the strength of the matching between well-capitalised banks and bank-dependent firms. It plots the coefficients of the unrated dummy from a series of regressions of bank capitalisation on Unrated and a set of controls, including lender size, lending relationship dummy, Altman's z-score, asset tangibility, profitability, cash, leverage, Tobin's Q, years since IPO, state dummies, industry dummies, covenant inclusion dummy, loan maturity, loan type dummies, loan purpose dummies, and quarter dummies. We measure bank capitalisation using the market equity value of a bank divided by the sum of market equity and book liabilities. Unrated Dummy takes one if a firm does not have an S&P long-term issuer rating in the quarter of loan origination. We run the regressions each quarter with a rolling window of four quarters, i.e., from t to t-4. The grey area surrounding the line depicts the 90% confidence intervals (with heteroskedasticity-consistent standard errors) for the coefficients; the horizontal red line is y=0; the red pillars represent two market crisis periods; the blue pillars represent two banking crisis periods, following the definition in Berger and Bowman (2013).

Figure 2.2: Unrated Firms that Borrow from Low-capital Banks Are More Likely to Obtain Credit Ratings



This figure plots a fitted line of regressing the Obtain Rating Dummy on bank capitalisation. The Obtain Rating Dummy takes one if the borrower in a loan obtains a rating after the loan origination. We use propensity score matching to match each treatment firm with two control firms. The matching criteria include Altman's z-score, asset tangibility, profitability, cash holdings, leverage, Tobin's Q, IPO years, industry, state, loan types, loan purposes, and loan maturity.

Table 2.1

Summary Statistics of Key Variables before and after the GFC

	Mean	SD	25^{th}	Median	75^{th}	Mean	SD	25^{th}	Median	75^{th}	Diff
	Panel A Pre-GFC					***Panel B Post-GFC***					
Observed Match Dummy	0.057	0.233	0	0	0	0.117	0.321	0	0	0	0.06***
Market Capital Ratio (%)	17.47	5.31	13.88	16.47	20.34	11.95	3.27	9.69	11.93	14.22	5.52***
Unrated Dummy	0.50	0.50	0	0	1	0.45	0.50	0	0	1	0.05***
Firm Size (in Logarithm)	6.83	1.92	5.56	6.79	8.05	8.05	1.58	6.93	7.93	9.05	-1.22***
Estimated Costs of Switching from Loans to Bonds (%)	1.96	2.23	.463	2.03	3.34	1.49	2.03	.096	1.27	2.71	0.47***
Loan Outstanding over Debts at Previous Year-end (%)	34.77	37.79	0.00	18.75	68.74	41.44	39.75	0.36	31.02	86.15	-6.67***
Non-performing Assets Ratio (%)	0.49	0.29	0.31	0.44	0.59	0.85	0.70	0.37	.589	1.11	-0.35***
Loan Loss Provision over Loans (%)	0.18	0.20	0.06	0.13	0.24	0.15	0.19	0.06	0.09	0.16	0.03
Liquidity Ratio (%)	10.04	6.31	5.32	8.70	13.71	17.32	11.82	4.60	18.45	27.49	-7.28***
Interest-bearing Deposit Ratio (%)	47.54	7.30	43.53	48.44	52.53	47.55	6.99	44.62	49.01	52.17	-0.01
Lender Size (in Logarithm)	11.48	1.34	10.79	11.43	12.41	12.58	1.315	11.72	12.201	14.03	-1.11***
Lending Relationship Dummy	0.06	0.23	0	0	0	0.12	0.32	0	0	0	-0.06***
Top Industry Dummy	0.104	0.31	0	0	0	0.15	0.35	0	0	0	-0.04***
Bank-firm Distance (in Kilometres)	1592	1118	721	1297	2296	1598	1138	712	1220	2353	-5.94
Altman's Z-score	67.62	92.05	31.18	79.44	123.47	72.25	75.13	29.77	74.34	119.5	-4.63***
Asset Tangibility (%)	29.88	22.47	12.70	23.83	41.70	27.01	23.47	9.26	18.57	38.01	2.87***
Profitability (%)	3.24	2.98	1.960	3.30	4.78	3.33	2.218	2.24	3.206	4.311	-0.09***
Cash Holdings (%)	8.02	10.51	1.41	3.68	10.14	9.83	10.18	2.65	6.36	13.61	-1.81***
Financial Leverage (%)	27.27	17.31	14.29	26.43	38.28	31.03	17.79	18.5	29.58	42.95	-3.76***
Tobin's Q	1.71	0.95	1.10	1.42	2.00	1.80	0.93	1.19	1.53	2.08	-0.09***

Notes: This table presents summary statistics for the sample of all potential bank-firm matches. Panel A reports statistics for the bank-firm matches in the quarters in the pre-GFC period (2000Q1-2007Q2), while Panel B reports statistics for post-GFC periods (2010Q1-2019Q4). Diff is the difference in mean values between the pre-GFC and the post-GFC sample. Observed Match Dummy indicates whether a bank-firm pair shares a loan contract in that given quarter. We measure bank capitalisation using the market equity value of a bank divided by the sum of market equity and book liabilities. Firm Size is a firm's total assets measured in logarithms. The estimated Costs of Switching from Loans to Bonds are estimated by matching the firm with the bond-issuing firms in the same terciles based on three financial variables and the two-digit SIC industry code. Loan Outstanding over Debts at the Previous Year-end is the loan outstanding divided by total debts at the end of the previous year. Non-performing Asset Ratio is a bank's non-performing assets divided by total assets. Loan Loss Provision over Loans is loan loss provision divided by total gross loans. Liquidity Ratio is a bank's liquidity assets divided by total assets. Interest-bearing Deposit Ratio is a bank's interest-bearing liabilities divided by total assets. Lending Relationship Dummy is a binary variable taking one if a firm and a bank have a past contract within 20 quarters prior to the loan origination quarter. Bank-firm Distance is the physical distance between a firm's headquarter and a bank's headquarter in thousand kilometres. Top Industry Dummy is a dummy taking one if a firm's industry falls in the top three industries of a bank's borrowers in a given quarter. See Table A1 for further information on variable definitions.

Table 2.2

The Matching between Well-capitalised Banks and Unrated Firms before and after the GFC

	1	2	3	4
	OLS Regression Model	OLS Regression Model	Semiparametric Model	Semiparametric Model
Bank Capitalisation	-0.0292	0.190**		
	(-0.81)	(2.60)		
Unrated Dummy	-1.296[a]	-2.712		
	(-1.64)	(-1.54)		
Bank Capitalisation×Unrated Dummy	0.0974**	0.193	15.42**	0.31
	(2.01)	(1.43)	[7.48, 32.49]	[-6.18, 24.24]
Bank Size×Firm Size	0.151	0.201	11.46**	30.71**
	(0.89)	(0.67)	[7.64, 40.50]	[11.27, 40.35]
Lending Relationship Dummy	53.91***	56.86***	1000	1000
	(27.72)	(24.75)	[-]	[-]
Top Industry Dummy	12.77***	10.02***	280.13**	397.58 **
	(22.78)	(11.01)	[222.46, 355.29]	[276.84, 395.54]
Bank-firm Distance	-0.00114***	-0.00045**	-0.0357**	-0.0252**
	(-5.41)	(-2.12)	[-0.1074, -0.0297]	[-0.0565, -0.0034]
Sample	Pre-GFC	Post-GFC	Pre-GFC	Post-GFC
Quarter-, Bank-, and Firm-FEs	Yes	Yes	N/A	N/A
Observations/Number of Inequalities	214901	64110	1443901	365620
Adjusted R^2/Fraction of Inequalities Satisfied	0.385	0.489	0.933	0.903

Notes: This table reports estimates from linear probability regression model (Column 1-2) and semiparametric model (Column 3-4). There are two sample periods – the pre-GFC period is 1987Q1-2007Q2 (excluding periods of banking crises), and the post-GFC is 2010Q1-2019Q4. The sample includes all possible matches between banks and firms recorded in Dealscan in a given quarter. The linear probability model regresses the observed match dummy on a set of banks' and firms' characteristics. The dependent variable is Observed Match Dummy taking one if the bank-firm pair is observed in Dealscan. This table reports the coefficients of key independent variables. Bank capitalisation is a bank's market equity value divided by the sum of market equity and book liabilities. Unrated Dummy takes one if a firm does not have an S&P long-term credit rating. Bank Size is a bank's total assets measured in logarithms. Firm Size is a firm's total assets measured in logarithms. There are three other variables constructed at the bank-firm level. Lending Relationship Dummy is a binary variable taking one if a firm and a bank have a past contract within 20 quarters prior to the loan origination quarter. Bank-firm Distance is the physical distance between a firm's headquarter and a bank's headquarter in thousand kilometres. Top Industry Dummy is a dummy taking one if a firm's industry falls in the top three industries of a bank's borrowers in a given quarter. Quarter-, bank-, and firm-fixed effects are included in OLS regressions. The other (unreported) controls include bank size, firm size, z-score, asset tangibility, profitability, cash, financial leverage, Tobin's Q, and years since IPO. See Table A.1 for further information on variable definitions. All OLS coefficients are multiplied by 100. T-statistics based on standard errors clustered by banks are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively. In the semiparametric model, we estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. All borrower and bank-specific characteristics are demeaned each quarter. Number of Inequalities is the number of total inequalities. Fraction of Inequalities Satisfied is the fraction of inequalities satisfied by the pairwise stability condition under the model prediction. 95% confidence intervals are constructed by subsampling and are reported in square brackets. ** indicates that the confidence interval does not contain zero.

Table 2.3

Unrated Firms that Face High Switching Costs Still Match with Well-capitalised Banks after the GFC

	1	2	3	4	5	6
	OLS Regression Model	OLS Regression Model	OLS Regression Model	Semiparametric Model	Semiparametric Model	Semiparametric Model
Bank Capitalisation×Small Unrated Dummy	0.459*			53.76**		
	(1.81)			[6.00, 84.94]		
Bank Capitalisation×Large Unrated Dummy	0.135			-5.62		
	(1.20)			[-14.29, 27.03]		
Bank Capitalisation×Costly-switch Unrated Dummy		0.332**			162.23**	
		(2.49)			[14.61, 169.85]	
Bank Capitalisation×Costless-switch Unrated Dummy		0.178			113.13	
		(0.86)			[-18.48, 142.78]	
Bank Capitalisation×Loan-heavy Unrated Dummy			0.430***			145.27**
			(3.24)			[15.34, 170.31]
Bank Capitalisation×Loan-light Unrated Dummy			0.123			70.60
			(0.79)			[-30.36, 125.33]
Bank Size×Firm Size	0.190	0.187	0.193	10.48**	44.90**	11.91**
	(0.63)	(0.78)	(0.64)	[1.64, 37.22]	[20.54, 80.47]	[9.61, 50.80]
Lending Relationship Dummy	56.85***	58.63***	56.50***	1000	1000	1000
	(24.69)	(27.95)	(24.13)	[-]	[-]	[-]
Top Industry Dummy	10.03***	8.417***	10.33***	339.97**	422.78**	427.78**
	(11.01)	(10.03)	(11.22)	[270.29, 569.61]	[317.19, 664.78]	[313.91, 651.80]
Bank-firm Distance	-0.00045**	-0.00056***	-0.343	-0.0935**	-0.0282**	-0.0037**
	(-2.14)	(-2.99)	(-1.64)	[-0.0793, -0.0348]	[-0.0561, -0.0059]	[-0.0325, 0.0037]
Sample	Post-GFC	Post-GFC	Post-GFC	Post-GFC	Post-GFC	Post-GFC
Quarter-, Bank-, and Firm-FEs	Yes	Yes	Yes	N/A	N/A	N/A
Observations/Number of Inequalities	64110	26998	59884	365620	38394	177444
Adjusted R^2/Fraction of Inequalities Satisfied	0.489	0.493	0.494	0.901	0.906	0.907

Notes: This table reports estimates from the linear probability regression model (Column 1-2) and the semiparametric model (Column 3-4). The sample period is 2010Q1-2019Q4. The sample includes all possible matches between banks and firms recorded in Dealscan in a given quarter. The linear probability model regresses the observed match dummy on a set of banks' and firms' characteristics. The dependent variable is Observed Match Dummy taking one if the bank-firm pair is observed in Dealscan. This table reports the coefficients of key independent variables. Bank capitalisation is a bank's market equity value divided by the sum of market equity and book liabilities. Small (Large) Unrated Dummy takes one if an unrated firm falls in the bottom (top four) size quintile(s). Costly-switch (Costless-switch) Unrated Dummy takes one if an unrated firm's estimated interest-rate difference between bonds and loans is positive (non-positive). Loan-heavy (Loan-light) Unrated Dummy takes one if an unrated firm's loan outstanding over total debt outstanding is 100% at the end of the previous year. Bank Size is a bank's total assets measured in logarithms. Firm Size is a firm's total assets measured in logarithms. For all interactions, we do not report the coefficients of the two interacting variables. There are three other variables constructed at the bank-firm level. Lending Relationship Dummy is a binary variable taking one if a firm and a bank have a past contract within 20 quarters prior to the loan origination quarter. Bank-firm Distance is the physical distance between a firm's headquarter and a bank's headquarter in thousand kilometres. Top Industry Dummy is a dummy taking one if a firm's industry falls in the top three industries of a bank's borrowers in a given quarter. Quarter-, bank-, and firm-fixed effects are included in OLS regressions. The other (unreported) controls include z-score, asset tangibility, profitability, cash, financial leverage, Tobin's Q, and years since IPO. See Table A.1 for further information on variable definitions. All OLS coefficients are multiplied by 100. T-statistics based on standard errors clustered by banks are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively. In the semiparametric model, we estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. All borrower and bank-specific characteristics are demeaned each quarter. Number of Inequalities is the number of total inequalities. Fraction of Inequalities Satisfied is the fraction of inequalities satisfied by the pairwise stability condition under the model prediction. 95% confidence intervals are constructed by subsampling and are reported in square brackets. ** indicates that the confidence interval does not contain zero.

Table 2.4

Loan Access during the GFC and COVID periods under the Benchmark and Counterfactual Scenarios

	Panel A Loan Access in COVID		***Panel B Loan Access in GFC***	
	Benchmark (%)	Benchmark − Counterfactual (%)	Benchmark (%)	Benchmark − Counterfactual (%)
Observed Matches	-68.98		-62.29	
	[-]		[-]	
Estimates from the Pre-GFC Sample		-1.22		
		[-3.49, 0.98]		
Absolute $\hat{\beta}_1 = \lvert\hat{\beta}_1\rvert$		-1.83		
		[-3.57, 1.34]		
Estimates from the Pre-COVID Sample				3.49**
				[1.99, 4.09]
Shut-off $\hat{\beta}_1 = 0$				2.35**
				[1.38, 3.40]

Notes: This table presents unrated firms' loan access during the COVID and the GFC. For the credit access, we proxy it using the average loan growth of all unrated firms' (most recent) pre-crisis relationship banks under observed matches (the benchmark) and counterfactuals. The counterfactual relationship banks are assigned by using the model estimates or altering the estimates. We estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. The estimated sample is 20 quarters prior to the COVID-19 shock (GFC) for Panel A (Panel B). Each bank's lending growth is the growth rate of the annualised total amount of syndicated loans from the pre-crisis period to the crisis period. For the COVID (GFC), the pre-crisis and crisis period are 2017Q1-2019Q4 (2004Q4-2007Q2) and 2020Q2-2020Q4 (2008Q4-2009Q2). In the benchmark scenario, an unrated firm's relationship bank is the one in its actual loan contract in the most recent loan facility. In the counterfactuals, each unrated firm's relationship bank is identified in three steps: adjusting the estimated parameters, using them to calculate the matching value, and then matching each firm with a bank that maximises the matching value. In Panel A, we make unrated firms' relationship banks to be well-capitalised. To do so, we let $\hat{\beta}_1$, the coefficient of the interaction between bank capitalisation and borrowers' unrated status, to be positive in two ways. First, we use the estimates from the pre-GFC sample. Second, we take an absolute of $\hat{\beta}_1$, while keeping all other parameter estimates unchanged. Similarly, in Panel B, we use the pre-COVID estimates and take $\hat{\beta}_1$ to be zero. 95% confidence intervals (reported in square brackets) are obtained by estimating 50 sets of parameter estimates based on subsampling, then drawing 20 times from the counterfactual matches for each set of estimates. ** indicates that the confidence interval does not contain zero.

Table 2.5

Risky Banks Match with Unrated Firms after the GFC

	1	2	3	4	5	6
	OLS Regression Model	OLS Regression Model	OLS Regression Model	Semiparametric Model	Semiparametric Model	Semiparametric Model
Non-performing Asset Ratio×Unrated Dummy	1.448*			163.47**		
	(1.99)			[40.37, 180.09]		
Loan Loss Provision×Unrated Dummy		1.470*			130.51**	
		(1.82)			[3.70, 166.81]	
Liquidity Ratio×Unrated Dummy			-0.0365*			-0.81
			(-1.81)			[-8.27, 15.83]
Bank Capitalisation×Unrated Dummy	0.265	0.216	0.149	2.92	4.96	-7.72
	(1.71)	(1.52)	(0.91)	[-14.12, 20.38]	[-5.16, 28.10]	[-17.01, 38.11]
Bank Size×Firm Size	0.186	0.204	0.188	28.84**	27.43**	21.61**
	(0.66)	(0.68)	(0.59)	[12.46, 45.70]	[12.41, 47.64]	[9.44, 57.23]
Lending Relationship Dummy	56.81***	56.86***	57.00***	1000	1000	1000
	(25.00)	(24.81)	(24.38)	[-]	[-]	[-]
Top Industry Dummy	10.05***	10.03***	9.914***	491.65**	450.03**	337.22**
	(10.98)	(11.01)	(10.30)	[336.90, 578.94]	[330.10, 577.39]	[282.21, 596.77]
Bank-firm Distance	-0.00044**	-0.00044**	-0.00040*	-0.050**	0.0035	-0.0345**
	(-2.13)	(-2.10)	(-1.96)	[-0.063, -0.015]	[-0.0477, -0.0047]	[-0.0546, -0.0002]
Sample	Post-GFC	Post-GFC	Post-GFC	Post-GFC	Pre-GFC	Post-GFC
Quarter-, Bank-, and Firm-FEs	Yes	Yes	Yes	N/A	N/A	N/A
Observations/Number of Inequalities	64110	64110	57439	365620	365620	332685
Adjusted R^2/Fraction of Inequalities Satisfied	0.489	0.489	0.491	0.903	0.903	0.899

Notes: This table reports estimates from the linear probability regression model (Column 1-2) and semiparametric model (Column 3-4). There are two sample periods – the pre-GFC period is 1987Q1-2007Q2 (excluding periods of banking crises), and the post-GFC is 2010Q1-2019Q4. The sample includes all possible matches between banks and firms recorded in Dealscan in a given quarter. The linear probability model regresses the observed match dummy on a set of banks' and firms' characteristics. The dependent variable is Observed Match Dummy taking one if the bank-firm pair is observed in Dealscan. This table reports the coefficients of key independent variables. Non-performing Asset Ratio is a bank's non-performing assets over total assets. Loan Loss Provision over Loans is loan loss provisions over total gross loans. Liquidity Ratio is a bank's liquidity assets over total assets. Bank capitalisation is a bank's market equity value divided by the sum of market equity and book liabilities. Unrated Dummy takes one if a firm does not have an S&P long-term credit rating. Bank Size is a bank's total assets measured in logarithms. Firm Size is a firm's total assets measured in logarithms. For all interactions, we do not report the coefficients of the two interacting variables. There are three other variables constructed at the bank-firm level. Lending Relationship Dummy is a binary variable taking one if a firm and a bank have a past contract within 20 quarters prior to the loan origination quarter. Bank-firm Distance is the physical distance between a firm's headquarter and a bank's headquarter in thousand kilometres. Top Industry Dummy is a dummy taking one if a firm's industry falls in the top three industries of a bank's borrowers in a given quarter. Quarter-, bank-, and firm-fixed effects are included in OLS regressions. The other (unreported) controls include z-score, asset tangibility, profitability, cash, financial leverage, Tobin's Q, and years since IPO. See Table A.1 for further information on variable definitions. All OLS coefficients are multiplied by 100. T-statistics based on standard errors clustered by banks are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively. In the semiparametric model, we estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. All borrower and bank-specific characteristics are demeaned each quarter. Number of Inequalities is the number of total inequalities. Fraction of Inequalities Satisfied is the fraction of inequalities satisfied by the pairwise stability condition under the model prediction. 95% confidence intervals are constructed by subsampling and are reported in square brackets. ** indicates that the confidence interval does not contain zero.

Table 2.6

The Matching between Well-capitalised Banks and Unrated Firms When Banks Have High or Low "Chase for Yield" Incentives

	1	2	3	4
	OLS Regression Model	OLS Regression Model	Semiparametric Model	Semiparametric Model
Bank Capitalisation	0.057	0.247		
	(0.26)	(1.34)		
Unrated Dummy	3.201**	-3.414		
	(2.59)	(-1.50)		
Bank Capitalisation×Unrated Dummy	-0.214*	0.220	63.13	1.18
	(-2.01)	(1.15)	[-18.94, 98.02]	[-18.07, 25.97]
Bank Size×Firm Size	-0.743**	0.286	58.15**	15.25 **
	(-2.23)	(0.91)	[3.87, 65.65]	[8.06, 57.98]
Lending Relationship Dummy	47.57***	58.33***	1000	1000
	(14.08)	(30.67)	[-]	[-]
Top Industry Dummy	11.91***	9.476***	558.36**	389.60 **
	(17.65)	(9.16)	[459.28, 789.58]	[284.41, 539.03]
Bank-firm Distance	-0.00056	-0.00067**	-0.0441**	-0.0308**
	(-1.49)	(-2.52)	[-0.0899, -0.0152]	[-0.0649, -0.0064]
Sample	Post-GFC	Post-GFC	Post-GFC	Post-GFC
Subsamples by Bank Deposit Ratio	High Deposit Ratio	Low Deposit Ratio	High Deposit Ratio	Low Deposit Ratio
Quarter-, Bank-, and Firm-FEs	Yes	Yes	N/A	N/A
Observations/Number of Inequalities	18049	41537	6256	232601
Adjusted R^2/Fraction of Inequalities Satisfied	0.390	0.498	0.907	0.896

Notes: This table reports estimates from the linear probability regression model (Column 1-2) and the semiparametric model (Column 3-4). The sample includes all possible matches between banks and firms recorded in Dealscan in a given quarter. We divide the sample into two based on the bank interest-bearing deposit ratio. For each model, we estimate it in two subsamples. Both the linear probability model and logit model regress the observed match dummy on a set of banks' and firms' characteristics. The dependent variable is Observed Match Dummy taking one if the bank-firm pair is observed in Dealscan. This table reports the coefficients of key independent variables. Bank capitalisation is a bank's market equity value divided by the sum of market equity and book liabilities. Unrated Dummy takes one if a firm does not have an S&P long-term credit rating. Bank Size is a bank's total assets measured in logarithms. Firm Size is a firm's total assets measured in logarithms. For all interactions, we do not report the coefficients of the two interacting variables. There are three other variables constructed at the bank-firm level. Lending Relationship Dummy is a binary variable taking one if a firm and a bank have a past contract within 20 quarters prior to the loan origination quarter. Bank-firm Distance is the physical distance between a firm's headquarter and a bank's headquarter in thousand kilometres. Top Industry Dummy is a dummy taking one if a firm's industry falls in the top three industries of a bank's borrowers in a given quarter. Quarter-, bank-, and firm-fixed effects are included in all regressions. The other (unreported) controls include z-score, asset tangibility, profitability, cash, financial leverage, Tobin's Q, and years since IPO. See Table A.1 for further information on variable definitions. T-statistics based on standard errors clustered by banks are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively. In the semiparametric model, we estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. All borrower and bank-specific characteristics are demeaned each quarter. Number of Inequalities is the number of total inequalities. Fraction of Inequalities Satisfied is the fraction of inequalities satisfied by the pairwise stability condition under the model prediction. 95% confidence intervals are constructed by subsampling and are reported in square brackets. ** indicates that the confidence interval does not contain zero.

Table 2.7
Robustness Checks on the Disappearing Matching

	1	2	3	4
	OLS Regression Model	Semiparametric Model	Loan-level Regression	Loan-level Regression
Bank Capitalisation	0.761**		-0.00819*	-0.00171
	(2.37)		(-1.93)	(-0.36)
Unrated Dummy	4.086**		-0.354***	-0.0568
	(2.36)		(-7.14)	(-1.00)
Bank Capitalisation×Unrated Dummy	-0.362**	-22.13	0.0137***	0.00281
	(-2.31)	[-35.72, 4.64]	(4.29)	(0.60)
Sample	Post-GFC	Post-GFC	Pre-GFC	Post-GFC
Quarter-, Bank-, and Firm-FEs	Yes	N/A	Yes	Yes
Observations/Number of Inequalities	39853	149993	18308	13535
Adjusted R^2/Fraction of Inequalities Satisfied	0.337	0.88	0.786	0.638

Notes: This table presents additional results checking the robustness of the disappearing matching. In Column 1-2, we report coefficients from the OLS regression model and the semiparametric model, respectively. The sample period is 2010Q1-2019Q4. We exclude all loans where the firm and the bank have prior relationships during the pre-GFC period. We then generate a sample by constructing all possible matches between banks and firms using the loan-level data. The OLS model regresses the observed match dummy on a set of banks' and firms' characteristics. The dependent variable is Observed Match Dummy taking one if the bank-firm pair is observed in Dealscan. The independent variables reported include Bank capitalisation which is a bank's market equity value divided by the sum of market equity and book liabilities, Unrated Dummy which takes one if a firm does not have an S&P long-term credit rating and their interaction. Quarter-, bank-, and firm-fixed effects are included in the OLS regressions. The other (unreported) controls include lending relationship dummy, top industry dummy, bank-firm distance, bank size, firm size, z-score, asset tangibility, profitability, cash, financial leverage, Tobin's Q, and years since IPO. See Table A.1 for further information on variable definitions. All OLS coefficients are multiplied by 100. T-statistics based on standard errors clustered by banks are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively. In the semiparametric model, we estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. All borrower and bank-specific characteristics are demeaned each quarter. Number of Inequalities is the number of total inequalities. Fraction of Inequalities Satisfied is the fraction of inequalities satisfied by the pairwise stability condition under the model prediction. 95% confidence intervals are constructed by subsampling and are reported in square brackets. ** indicates that the confidence interval does not contain zero. In Column 3-4, we report the results from loan-level OLS regressions. The sample period is 1987Q1-2007Q2 (excluding periods of banking crises) in Column 3 and the post-GFC is 2010Q1-2019Q4 in Column 4. The dependent variable is the loan amount in logarithms. The control variables are lender size, lending relationship dummy, Altman's z-score, asset tangibility, profitability, cash, leverage, Tobin's Q, years since IPO, covenant inclusion dummy, and loan maturity. We also include loan type-, loan purpose-, quarter-, firm- and bank-fixed effects. T-statistics based on standard errors clustered by banks are reported in brackets.

Table 2.8

Changes in Unrated Firms' Bond Outstanding during the COVID Shock

	1	2	3	4	5	6
Dependent Variable	ΔTotal Bond Outstanding	ΔSenior Bonds and Notes	ΔSubordinated Bonds and Notes	ΔTotal Loan Outstanding	ΔTerm Loan	ΔRevolving Credit
Bank Capitalisation	-2.565*	-2.726**	0.609**	-0.578	-10.00	6.955*
	(-2.03)	(-2.76)	(2.70)	(-0.16)	(-0.66)	(1.84)
Sample	Post-GFC	Post-GFC	Post-GFC	Post-GFC	Post-GFC	Post-GFC
Quarter-, Loan Type-, Loan Purpose-, and Bank-FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1113	1113	1113	1113	1113	1113
Adjusted R^2	0.220	0.216	0.239	0.211	0.358	0.177

Notes: This table reports estimates from loan-level OLS regressions. The sample period is 2010Q1-2019Q. The dependent variable is the change in a firm's debt outstanding from the pre-COVID period (averaging cross 2017-2019) to the end of 2020. The debt outstanding in Column 1-6 are Senior Bonds and Notes, Subordinated Bonds and Notes, Total Bond Outstanding, Term Loan, Revolving Credit, and Total Loan Outstanding, respectively. The independent variable is Bank capitalisation which is a bank's market equity value divided by the sum of market equity and book liabilities. The control variables are lender size, lending relationship dummy, Altman's z-score, asset tangibility, profitability, cash, leverage, Tobin's Q, years since IPO, covenant inclusion dummy, and loan maturity. We also include loan type-, loan purpose-, quarter- and bank-fixed effects. T-statistics based on standard errors clustered by banks are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively

Table 2.9

Replicating OLS Regressions using Logit Regressions

	1	2	3	4	5	6	7	8
	Logit Regression	Logit Regression	Logit Regression	Logit Regression	Logit Regression	Logit Regression	Logit Regression	Logit Regression
Bank Capitalisation×Unrated Dummy	0.0664***	0.0209				0.0228	0.0237	0.0177
	(4.22)	(0.94)				(1.18)	(1.02)	(0.67)
Bank Capitalisation×Small Unrated Dummy			0.0545					
			(1.11)					
Bank Capitalisation×Large Unrated Dummy			0.0135					
			(0.80)					
Bank Capitalisation×High-cost Unrated Dummy				0.0468*				
				(1.89)				
Bank Capitalisation×Low-cost Unrated Dummy				0.0378				
				(0.97)				
Bank Capitalisation×High-reliance Unrated Dummy					0.0683***			
					(5.36)			
Bank Capitalisation×Low-reliance Unrated Dummy					0.00975			
					(0.43)			
Non-performing Asset×Ratio Unrated Dummy						0.162**		
						(2.21)		
Loan Loss Provision×Unrated Dummy							0.171	
							(0.99)	
Liquidity Ratio UnratedDummy								-0.00132
								(-0.26)
Sample	Pre-GFC	Post-GFC	Post-GFC	Post-GFC	Post-GFC	Post-GFC	Post-GFC	Post-GFC
Quarter-, Bank-, and Firm-Dumies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	208651	63835	63835	26072	59616	63835	63835	56769
Pseudo R^2	0.498	0.531	0.531	0.549	0.537	0.531	0.531	0.531

Notes: This table reports estimates from a logit regression model. The sample includes all possible matches between banks and firms recorded in Dealscan in a given quarter. The logit regression regresses the observed match dummy on a set of banks' and firms' characteristics. The dependent variable is Observed Match Dummy taking one if the bank-firm pair is observed in Dealscan. This table reports the coefficients of key independent variables. Bank capitalisation is a bank's market equity value divided by the sum of market equity and book liabilities. Unrated Dummy takes one if a firm does not have an S&P long-term credit rating. Small (Large) Unrated Dummy takes one if an unrated firm falls in the bottom (top four) size quintile(s). High-cost (Low-cost) Unrated Dummy takes one if an unrated firm's estimated interest-rate difference between bonds and loans is positive (non-positive). High-reliance (Low-reliance) Unrated Dummy takes one if an unrated firm's loan outstanding over total debt outstanding is 100% at the end of the previous year. Non-performing Asset Ratio is a bank's non-performing assets over total assets. Loan Loss Provision over Loans is loan loss provision over total gross loans. Liquidity Ratio is a bank's liquidity assets over total assets. Bank Size is a bank's total assets measured in logarithms. Firm Size is a firm's total assets measured in logarithms. For all interactions, we do not report the coefficients of the two interacting variables. There are three other variables constructed at the bank-firm level. Lending Relationship Dummy is a binary variable taking one if a firm and a bank have a past contract within 20 quarters prior to the loan origination quarter. Bank-firm Distance is the physical distance between a firm's headquarter and a bank's headquarter in thousand kilometres. Top Industry Dummy is a dummy taking one if a firm's industry falls in the top three industries of a bank's borrowers in a given quarter. Quarter, bank, and firm dummies are included in logit regressions. The other (unreported) controls include z-score, asset tangibility, profitability, cash, financial leverage, Tobin's Q, and years since IPO. See Table A.1 for further information on variable definitions. We report the log-odd ratios. T-statistics based on standard errors clustered by banks are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Chapter 3

The Rise of Co-lead Arrangements in Loan Syndication

3.1 Introduction

Private information plays a crucial role in alleviating information asymmetry between lenders and borrowers (Fama, 1985; Rajan, 1992) and offers inside lenders a competitive advantage relative to outsiders (Sharpe, 1990). The literature has documented various ways in which lenders produce private information. Lenders can obtain extra information through repeating lending (Boot, 2000), social networking (Degryse et al., 2021), and mergers and acquisitions (Panetta et al., 2009). In syndicated loans, in which a group of lenders finance one borrower, lead arrangers are delegated to conduct due diligence and gather information from the borrower on behalf of other participating lenders. However, whether the delegated lenders can effectively screen and monitor the borrower is debated. For example, lead arrangers may reduce their responsibility to collect information (Sufi, 2007) or withhold their private information (Down et al., 2022). The literature suggests several resolutions to the problems, including requiring lead arrangers to retain large loan shares (Sufi, 2007; Ivashina, 2009) and forming reciprocal lending arrangements (Bräuning and Fecht, 2017). This paper proposes another way to improve information production in loan syndication – arranging multiple lead arrangers. I refer to this phenomenon as co-lead loans or co-lead arrangements.

Although it is typical to have only one lead arranger, I find that loans with multiple lead arrangers have increased significantly over the past two decades. Specifically, the proportion of co-lead loans reaches 20% in 2009 and has kept increasing since then. One potential reason for the development is that, as the loans become larger, lead arrangers need additional partners to share the risks and complexity of arranging a loan. Another motivation could be that banks face higher capital requirements and are more cautious about their direct exposure to credit risks after the Global Financial Crisis (GFC); thus, a co-lead arrangement is a way to relieve capital constraints. In this study, I do not discuss the motivations for the rise of co-lead loans. Rather, I test empirically whether and how a co-lead arrangement can also help to produce extra private information from the perspectives of screening and monitoring. Note that information production can be either a motivation or a by-product of co-lead arrangements.

I first examine the impact of co-lead arrangement on ex-ante screening. Co-lead syndication can facilitate information sharing among experienced lenders (Tykvová, 2007). Moreover, diversified lenders contribute to verifying the quality of information, thus reducing the false selection of bad borrowers (Sah and Stiglitz, 1984; Lerner, 1994; Casamatta and Haritchabalet, 2007). Having multiple lead arrangers also reduces the costs of conducting due diligence. Thus, co-lead loans can improve screening and mitigate the adverse selection problem. If lead arrangers share the benefit of improved screening with the borrower, one should observe lower loan spreads (Boot and Thakor, 1994; Berger and Udell, 1995; Bharath et al., 2011). However, the co-lead arrangement also provides lenders with more bargaining power and induces price collusion (Cai et al., 2018). Multiple lead arrangers can then extract higher information rents from borrowers, especially risky and opaque firms (Degryse and Van Cayseele, 2000; Santos and Winton, 2008). In this respect, the relationship between co-lead arrangements and loan spreads is ambiguous. Thus, regressing loan spreads on co-lead status cannot reveal the role of co-lead arrangements in screening. To tackle the identification issue, I follow Botsch and Vanasco (2019) to estimate the impact of co-lead arrangement on the correlation between loan spreads and borrowers' creditworthiness instead of the spreads per se. Loan pricing should incorporate information on borrowers' creditworthiness such that risky borrowers pay higher spreads while safer ones pay lower spreads. Thus, if co-lead arrangements improve screening, I expect a higher (more negative) correlation between spreads and creditworthiness. I measure a firm's creditworthiness using the lowest credit rating in the five years after loan origination. Because the variable is measured in future periods, it is not publicly observable at the time of loan pricing. Therefore, the test reveals whether the co-lead arrangement helps to distinguish the borrower type by allowing lenders to price loans more accurately based on the information on borrowers' privately observed riskiness. The main data is loan-level data sourced from Dealscan and spanning from 2000 to 20019. For single-lead arranger loans, I find an 8.25 basis points (bps) increase in loan spreads following a full-rating downgrading (e.g., from BBB to BB). This magnitude is low, suggesting that a single lead arranger can only disentangle risky from safe borrowers by a certain degree. However, having multiple lead arrangers expands the spread difference between a BBB-rated firm and a BB-rated firm by 9.49 bps, more than doubling the difference. The evidence indicates that co-lead arrangements help to produce extra information about a borrower's riskiness and incorporate it into ex-ante loan pricing.

Next, I explore a second role of co-lead arrangement in information production: its enhancement of ex-post monitoring. Active monitoring requires periodical in-person visits (Gustafson et al., 2021). Arranging a co-lead loan permits this by delegating more lead arrangers to collect borrowers' information during the maturity of a loan. Also, including an additional monitor can lower the marginal cost of monitoring, thus encouraging lead arrangers to behave diligently. Further, collaboration among lead arrangers amplifies each lender's speciality in monitoring (François and Missonier-Piera, 2007), which improves efficiency. Therefore, through more intensive and efficient monitoring, co-lead arrangements can limit borrowers' risk-taking behaviour, thus reducing the probability of technical defaults. In this respect, I expect borrowers in co-lead loans to violate covenants less frequently than those in single-lead arranger loans. However, having more than one lead arranger will also introduce the potential for free-riding (Tykvová, 2007; Shivdasani and Song, 2011). Because monitoring efforts are hard to detect, every lead arranger has an incentive to copy the assessment of other lenders and is discouraged from exerting efforts. I find an insignificant relationship between co-lead arrangements and the number of covenant violations, confirming the potential offsetting effects of the monitoring enhancement and the intrinsic free-rider problem. Because reputation can restrict free-rider incentives (Do and Vu, 2010; Shivdasani and Song, 2011), I expect a significant interaction impact of co-lead arrangement and lender reputation on lowering covenant violations. I use lender size and market shares to proxy for reputation. The results support the argument by showing that an increase in lead arrangers' reputation lowers the frequency of covenant violations in a co-lead loan relative to that in a single-lead arranger loan. For example, conditional on highly reputable lead arrangers (with average market shares at the 90th percentile), the co-lead arrangement will reduce the number of covenant violations during the life of a loan deal by 5.8%.

A potential risk of exploiting cross-sectional variation to identify the role of co-lead arrangements in information production is that the empirical results could still face endogeneity issues because the co-lead arrangements, loan spreads, and covenant violations are jointly determined. To solve the endogeneity problem, I use the number of past relationship lenders to instrument the co-lead dummy. A borrower with more relationship lenders has a larger pool of potential lead arrangers to choose from and, thus, is more likely to form a co-lead loan. However, by controlling the intensity of relationship lending, I expect that the number of past relationship lenders should not be correlated with either loan spreads or covenant violations, satisfying the exogeneity condition for an instrumental variable (IV). I then estimate a standard two-stage least square IV regression, and the results confirm the previous findings. I also use propensity score matching to mitigate the fundamental differences between co-lead loans and single-lead arranger loans. Estimating the propensity scores with several loan- and borrower-specific criteria, I identify each co-lead loan with the four closest single-lead arranger loans. I replicate the same regressions as in the main tests and obtained similar results.

I conduct additional robustness checks. First, in the event of a breach of a covenant, lenders would like to minimise the severity of the violation. Thus, I test whether co-lead arrangements allow lenders to reduce violation severity conditional on observing a violation. I measure the severity as the percentage difference between a covenant variable and the threshold. If a loan contains more than one covenant, I average the severity measure over the covenants. Subsampling loans experiencing violations, I find that co-lead loans with a higher lender reputation significantly reduce the severity of covenant violations. This finding confirms the role of co-lead arrangement in enhancing monitoring. In two additional tests, I replace the co-lead dummy with the number of lead arrangers, and I use two other definitions of lead arrangers that are also frequently used in the prior literature. All the robustness checks reach the same conclusion.

The findings of this paper have several implications. First, by promoting more accurate assessments of borrower creditworthiness, co-lead arrangements can enhance financial stability and reduce the likelihood of systemic risks associated with poorly underwritten loans. Second, by better distinguishing between risky and safe borrowers, co-lead arrangements enable more efficient credit allocation to creditworthy projects, thereby fostering economic growth and development. Third, co-lead arrangements provide a mechanism for lenders to share risks and responsibilities, particularly in complex loan transactions. This risk-sharing aspect improves lenders' ability to manage and diversify their portfolios, reducing individual exposure to credit risks and enhancing overall risk management practices. Additionally, by facilitating accurate pricing and risk assessment, co-lead arrangements promote transparency and trust among market participants, strengthening the competitiveness of financial markets and fostering a more robust and efficient financial system. This also suggest that regulators may consider incentivising or mandating co-lead arrangements to improve information production and mitigate systemic risks in loan syndication.

Literature Review: Prior literature produces limited results on the function of co-leadership in syndicated loans. Some examples include (François and Missonier-

Piera, 2007; Hao and Roberts, 2008). However, these papers focus only on the senior participants, not the lead arrangers. Thus, the current paper provides the first evidence of the impact of having multiple lead arrangers. This paper is complementary to the literature studying how lenders collect private information of borrowers in syndicated lending. One of the most widely-documented means of alleviating information asymmetry is relationship lending (Besanko and Thakor, 1995; Boot, 2000; Bharath et al., 2007; Schenone, 2010). With repeated lending, a lender can accumulate private knowledge of a borrower and establish better communication skills with the borrower. In examining the hypothesis, Bharath et al. (2011); Botsch and Vanasco (2019) test whether relationship lending reduces loan spreads. In terms of non-pricing terms, Berger and Udell (1995) find that relationship lenders require lower collaterals. Moreover, Prilmeier (2017) studies the association between covenant strictness and lending relationships. They find that relationships can substitute for the need to have stringent covenants. In addition to past relationships, Panetta et al. (2009) show that banks can inherit private information by acquiring other banks. More relevant to the present paper, Murfin (2012) finds that lenders tend to tighten loan covenants if they are sceptical about their own screening abilities. Additionally, Engelberg et al. (2012) argue that the interpersonal connections between lenders and borrowers, measured by educational and working histories, help to predict a borrower's future credit rating and stock performance. Supplementing the above-mentioned papers, I illustrate the role of co-lead arrangement in information production by showing its impacts on loan spreads and covenant violations.

Prior literature shows that lender networks can also smooth information production in the syndicated loan market. The current paper contributes to it by introducing another form of networking - collaboration in co-lead loans. Godlewski and Sanditov (2018) measure networks according to past syndication between lead arrangers and their participants and argue that it is easier for lead arrangers to obtain information if they have more connections with other lenders. As a result, they find that a loan originated by a lead arranger with a more centralised network provides more valuable certification for the borrower. Degryse et al. (2021) provide supporting evidence for learning from participants. Expanding the scope of these papers, I emphasise information sharing among lead arrangers. Harris et al. (2020) show that a lender with a more concentrated network has better communication skills and, thus, is more likely to be a lead arranger. My results indicate that co-leading with other lenders can result in a tighter network. Further, Cai et al. (2018) study the impact of portfolio proximity on screening. They find that loan spreads are lower if the syndicate lenders are closer in portfolio similarity because, arguably, proximity facilitates collaboration. In this paper, I use the co-lead structure as a more direct proxy for collaboration. Cai et al. (2018) also points out that price collusion, where closer lenders have more concentrated bargaining power to inflate loan spreads, may confound the collaboration effect. Taking a further step, I refine the identification strategy by testing the impact of collaboration on the risk-pricing relationship.

The two-layer structure of syndicated loans creates a moral hazard problem where lead arrangers may shrink their responsibility of monitoring. Carletti et al. (2007) propose that a larger degree of risk diversification encourages monitoring within a multiple-bank lending scenario because it increases marginal returns from monitoring. Another way to boost monitoring incentives is to raise the lender's stakes in loan performance. Consistently, Sufi (2007) find that lead arrangers express their commitment to monitoring by taking a higher share of funding. Likewise, Schwert (2018) argues that banks with high capital ratios exert intensive monitoring. The current study suggests that co-lead arrangements may act as a monitoring enhancement device.

Information production through collaboration and co-leaderships is also studied in other forms of syndication; for example, the topic is largely explored in Venture Capital (VC) syndication literature (e.g., Lerner (1994); De Clercq and Dimov (2004); Casamatta and Haritchabalet (2007)). Bygrave (1987) documents that a VC is more likely to invite other VCs when investing in information-intensive industries. Also, Hopp and Rieder (2011) find that when entering new industries, a VC tends to collaborate with VCs with more diversified experiences. In contrast with the papers that study the determinants of a VC partnership, I test the impact of the co-lead arrangement on pricing and monitoring. Focusing on IPO underwriting syndicates, Corwin and Schultz (2005) test the information production by senior syndicate managers. The authors use a similar identification strategy as the present paper. Specifically, acknowledging that private information allows an IPO underwriting to be priced more accurately, they find that IPO syndication with more co-managers is more likely to correct the offering price such that it corresponds more to the future market demand for the firm's stocks. Song (2004); Shivdasani and Song (2011); Carbó-Valverde et al. (2021), among others, study co-lead arrangements in bond underwriting syndication. Similar to the rise of co-lead syndicated loans, but preceding this phenomenon, Shivdasani and Song (2011) find that co-lead bond syndicates emerged rapidly in 2000. Complementary to the broad literature, this paper investigates co-leaderships in the syndicated loan market and also takes into consideration different confounding effects such as the price collusion and free-rider problem.

The remainder of this study is organised as follows. Section 3.2 discusses the theoretical intuitions that co-lead arrangements facilitate screening and monitoring. Section 3.3 describes the data used in this study and defines key variables. In this section, I also specify the regression models. Section 3.4 presents and interprets empirical results, including the impacts of co-lead arrangements on loan spreads and covenant violations. Section 3.5 conducts the robustness tests, and the last section concludes.

3.2 Rationales for Co-lead Arrangements in the Syndicated Loan Market

This section discusses first the potential motivations for having multiple lead arrangers and second how co-lead arrangements affect information production in syndicated loans. Note that the role of co-lead arrangements in improving information production could be either a motivation or a by-product. The aim of this study is not to distinguish the two. I would rather focus on testing the existence of information production.

3.2.1 Possible Motivations for Co-lead Arrangements

Foremost, the motivation for co-lead arrangements could be similar to the motivations for syndicating a loan in the first place (see Simons et al. (1993); Dennis and Mullineaux (2000); Carletti et al. (2007) for a discussion on motivations for loan syndication). Specifically, as a large contributor to the loan (Sufi, 2007), forming a co-lead loan helps a lead arranger with funding pooling, risk sharing, and relieving capital constraints. Thus, a co-lead arrangement is more likely to be considered when the borrowing amount is larger, when lead arrangers are more conservative in taking risks, and/or when the lead arrangers' capital buffers are more binding. This could be the reason why co-lead loans have risen in recent years, especially after the GFC, when the banking industry experienced severe damage and stricter capital requirements such as Basel III were implemented.

Secondly, collaboration among multiple lead arrangers can lighten the burden of organising and administrating a loan. It is the lead arrangers' responsibility to link the borrower with other participants and handle the flow of funds in between. Moreover, before inviting participants, lead arrangers also need to draft marketing materials and approach rating agencies (Blickle et al., 2020). Thus, it will be more efficient if the administrative tasks can be distributed to each lead arranger who has comparative advantages in doing the task. An alternative way to ease the management burden is to nominate co-agents and allocate tasks to them (Cai et al., 2018). For example, a "documentation agent" is the one who issues the document and selects law firms. Empirically, François and Missonier-Piera (2007) confirms the role of co-agents in reducing the costs of administrating a loan. Thus, having multiple lead arrangers and having more co-agents are substitutable, and one can expect a co-lead loan to have fewer co-agents than a single-lead arranger loan.

3.2.2 Roles of Co-lead Arrangements in Information Production

Next, as a focus in this paper, I propose that multiple lead arrangers have advantages in producing extra pirate information. Below, I discuss in more detail how co-lead arrangements benefit information production from two aspects.

In the first aspect, co-lead arrangements can improve ex-ante screening and alleviate adverse selection problems. I argue that the improvement mainly comes from information sharing. Millon and Thakor (1985) propose that compared to a single agent, information sharing is a necessary condition to form a group of screening agents. In their model, pooling information together a) increases the probability of receiving a pricing signal of a firm's true value and b) avoids duplication of information collection. In a syndicated loan, information sharing can a) make loan pricing more accurate and b) save costs of duplicated due diligence. Information sharing within syndication is largely documented in Venture Capitalist (VC) literature. Because the invested companies tend to be younger and more informationally opaque, having syndicate partners can verify each VC's opinion before making an investment decision (Lerner, 1994). As illustrated by Sah and Stiglitz (1984), pooling information can form a hierarchy structure and reduce the false acceptance of lemon projects. Casamatta and Haritchabalet (2007); Tykvová (2007) argue that syndicating an investment project faces the costs of losing part of the profits and the investment opportunity being stolen by others. In their models, the justification for a VC syndicate is that it can enhance an investment's net present value by improving the quality of information. Thus, I argue that lead arrangers can benefit from information sharing by having a co-lead arrangement. Further, Lerner (1994) finds that VCs tend to syndicate with experienced partners in the first-round investment but not in later rounds where the investment decision is already made. Thus, information sharing is more valuable before an investment decision and contracting terms are made. In loan syndication, lead arrangers conduct due diligence and make the lending decision before inviting participants. For example, lead arrangers usually commit to making a loan by underwriting the loan shares (Blickle et al., 2020). Also, loan terms are decided mainly by lead arrangers while participants make suggestions on adjustments (Dass et al., 2020). Thus, studying information production by having more lead arrangers is more meaningful than studying the role of having more participants.[1] Empirical evidence confirms information sharing in VC syndication. Bygrave (1987) documents that VC investment is more likely to be syndicated in information-intensive companies. Similarly, De Clercq and Dimov (2004); Hopp and Rieder (2011) point out that a less knowledgeable VC is more likely to invite partners. The role of collaboration in mitigating information asymmetry ex-ante is tested in other forms of syndication. For example, Corwin and Schultz (2005) study IPO underwriting syndication. They argue that co-managers have unique information about the local market demand for the issuer stocks, resolving under-pricing issues. Because commercial banks have advantages in screening borrowers, Song (2004) finds that inviting commercial banks as co-managers enhances the certification on screening in bond underwriting. Similarly, Jeon and Ligon (2011) confirm that having more co-managers underwriting a seasoned equity offering can mitigate information asymmetry between issuers and investors.

Focusing on syndicated loans, I propose that a co-lead structure can produce extra information about a borrower's creditworthiness. For example, lead arrangers can share their past experiences with a borrower (Botsch and Vanasco, 2019). Also, banks can infer borrowers' repaying ability from their history of deposit accounts (Fama, 1985). If the co-lead structure facilitates information sharing and reduces screening costs, one should expect lower loan spreads (Schenone, 2010; Bharath et al., 2011). The expectation is consistent with the results of Song (2004) and Jeon and Ligon (2011), who respectively document smaller floating costs and lower bond yields when there are more co-managers in the corresponding syndicates. However, studying the impact of co-lead arrangement on loan spreads cannot fully identify its role in information production due to a confounding effect. In fact, the co-lead structure can also lean bargaining power on the lender side. A direct consequence is lender collusion,

[1]In this study, I emphasise the information production by co-lead arrangements but do not deny the information shared from participants. For example, Degryse et al. (2021) point out that lead arrangers can learn from participants. Also, Sufi (2007) finds that lead arrangers are more likely to select knowledgeable participants.

which will lead to higher loan spreads (Cai et al., 2018). Also, more bargaining power allows the lenders to hold up risky and opaque borrowers, again by asking for higher loan spreads (Santos and Winton, 2008; Schwert, 2018) Given the opposite effects, I refine the identification similar to Corwin and Schultz (2005). Instead of estimating the impact of co-management on pricing per se, the authors quantify its impact on pricing accuracy. Likewise, I test whether syndicating with multiple lead arrangers helps with loan pricing accuracy, thus improving ex-ante screening. Loan spreads should reflect borrowers' creditworthiness Maudos and De Guevara (2004). Private information allows lenders to discriminate loan rates according to a borrower's creditworthiness. Thus, if multiple lead arrangers are more informative than a single lead arranger, I expect that the loan spreads should be lower for more creditworthy borrowers while higher for riskier borrowers.

Hypothesis 1 *Co-lead arrangements intensify the negative relationship between loan spreads and a borrower's creditworthiness.*

From another aspect, the co-lead arrangement can also enhance information production during the life of a loan, and I propose that it intensifies ex-post monitoring. Having more lead arrangers facilitates a more efficient allocation of monitoring tasks. This fits each lead arranger's speciality (François and Missonier-Piera, 2007; Shivdasani and Song, 2011). The co-lead arrangement also makes in-person visits easier and less costly. As in Degryse et al. (2021), learning from peers reduces monitoring costs. Moreover, monitoring requires lenders to periodically assess borrowers' financial reports and ratios to ensure covenant compliance and evaluate collateral values (Gustafson et al., 2021). The assessments require soft knowledge of, for example, which variables in financial reports are relevant and how precise the documents are (Liberti and Petersen, 2019). Having multiple lead arrangers diversifies lenders' experiences in assessing borrowers' financial conditions and documents, enhancing the efficiency and quality of monitoring. Further, the co-lead arrangement can facilitate monitoring by extracting information from interpersonal connections between the additional lead arrangers and the borrowing firm's executives (Engelberg et al., 2012; Karolyi, 2018; Botsch and Vanasco, 2019). As a result of monitoring enhancement, co-lead arrangements can limit a borrower's excessive risk-taking. Thus, I expect to observe lower occasions of technical defaults (i.e., covenant violations). Also, by giving lenders the right to accelerate loan repayment, covenant violations transfer a part of loan values from borrowers to lenders. Thus, if co-lead arrangement results in more careful supervision, borrowers are encouraged to avoid violations. In addition, covenant violations may generate extra negotiation costs (Billett et al., 2016; Berlin et al., 2020). Thus, although lenders can obtain control rights, they still have incentives to reduce technical defaults through monitoring. Therefore, I expect co-lead loans to have lower covenant violations than single-lead arranger loans.

However, having multiple lenders monitoring at the same time may introduce the problem of free riders, which discourages monitoring at all. In Degryse et al. (2021), the lead arranger becomes 'lazy' to monitor if they are too optimistic about the borrower's salvage value. In this study, I argue that lead arrangers are 'lazy' to monitor when the effort of monitoring is not perfectly observed and when they can free-ride others' outputs (Millon and Thakor, 1985). In Tykvová (2007)'s model, the free-rider problem prevents a VC from inviting another VC. In a more relevant empirical setting, Shivdasani and Song (2011) find that, due to the free-rider issue, firms in co-lead syndicates of bond underwriting are more likely to face lawsuits after the issuance. In this aspect, I expect co-lead loans to have more covenant violations. Thus, the opposite effects would make the correlation between co-lead arrangement and covenant violations ambiguous. In order to identify the role of information production, I explore the heterogenous impact of co-lead arrangements using lender reputation. Prestigious lenders have more incentives to monitor because they intend to preserve their reputation and because they are widely watched by the market (Do and Vu, 2010; Shivdasani and Song, 2011). Thus, I expect the monitoring enhancement effect dominates the free-rider problem if the lead arrangers' reputation is high. More specifically, co-lead arrangements by highly reputable lead arrangers reduce the frequency of covenant violations.

Hypothesis 2 *Co-lead arrangements reduce the frequency of covenant violations if the lead arrangers are more reputable.*

3.3 Data and Methodology

3.3.1 Data Sources and the Sample

In this subsection, I specify the data sources and describe the sample.

The main data source used in this study is loan-level data from Thomson Reuters Loan Pricing Corporation's DealScan (DealScan). This database contains detailed variables describing syndicated loans, but it only provides limited balance sheet information for the contracting parties. The primary source for lenders' and borrowers' quarterly data is Compustat North America. I use the linking tables provided by Chava and Roberts (2008) and Schwert (2018) to merge DealScan with Compustat datasets.[2] After the merge, the dataset contains DealScan loans for each quarter from quarter one of 1982 (1982Q1 for short) to 2016Q4. Since Chava and Roberts (2008)'s linking table only updates to 2017, I create a novel dataset by manually merging Dealscan firms with Compustat firms during the 2017-2020 period.[3] As shown later, the appearance of co-lead loans is limited before 2000. To ensure enough variation, the final sample is from 2000 to 2019.

In Dealscan, a loan deal is a package that includes several facilities. A typical package contains two facilities – a term loan and a revolving line of credit. Because a borrower's loan contract is signed at the package level, the unique observation in this study is a loan package rather than a facility. In addition, covenants are defined at the package level in Dealscan.[4]

To form a syndicated loan, lenders first bid for the leadership position, and the borrower will invite their relationship lenders to attend. Importantly, a borrower could mandate more than one lead arranger (Gadanecz, 2004; Sufi, 2007; Aldasoro et al., 2022).[5] This is the case of interest in this study. Then, the lead arranger(s)

[2] I thank Sudheer Chava, Michael Roberts, and Michael Schwert for making these data available on WRDS.
The lenders in Schwert (2018) linking table are those who acted as lead arrangers on at least 50 loans or at least $10 billion in volume in the set of loans in Chava and Roberts (2008)'s linking table.

[3] The authors update the linking table at the end of 2017. In case Dealscan updates more 2017 loans in the following years, my manual merge starts in 2017.

[4] In this study, I refer a loan to the loan package if not otherwise specified.

[5] There could be the case that one lender arranges one loan facility, making us observe co-lead

will negotiate preliminary loan terms with the borrower. After drafting the loan agreement, the lead arranger(s) starts marketing the loan and invites potential participants to join. Once the loan is contracted, it is the lead arranger's role to monitor the borrower, manage the loan, and distribute interest and principal payments while participants mainly provide the funding. Thereby, prior literature conventionally perceives lead arrangers as informed lenders while participants as uninformed investors. Lead arrangers can also appoint co-agents from participants. Although co-agents take part in managing, administrative, and monitoring tasks, lead arrangers directly communicate with the borrower and thus are more responsible for screening and monitoring. I follow Cai et al. (2018)'s definition to separate lead arrangers from co-agents. Specifically, I identify a lender as the lead arranger if the filed 'lenderrole' in Dealscan falls into the following categories: 'Admin agent, Agent, Arranger, Bookrunner, Coordinating arranger, Lead arranger, Lead bank, Lead manager, Mandated arranger, Mandated Lead arranger'. I cross-check the filed 'leadarrangercredit' and make sure the value is 'Yes'.[6] Then, for a lender to be a co-agent, the filed 'lenderrole' should fall in the following category: 'Co-agent, Co-arranger, Co-lead arranger, Co-lead manager, Co-lead underwriter, Co-manager, Collateral agent, Co-syndications agent, Documentation agent, Joint arranger, Joint lead manager, Managing agent, Senior co-arranger, Senior co-lead manager, Senior co-manager, Syndications agent'. In order to keep the integrity of the loan structure, thus reducing selection bias, the sample contains not only banks but also non-bank institutions. On average, a package contains 6.16 lenders. In Figure 3.1, I calculate the proportion of loans with multiple lead arrangers in total loans each quarter and plot the numbers from 1987Q1 to 2019Q4.[7] The proportion of co-lead loans starts to rise around 2000 and reaches 10% in 2004. After the Global Financial Crisis (GFC), there is a sharp increase in co-lead loans,

loans in the package level. Even if only one lead arranger is mandated at the package level, the lender can still form a consortium as co-leads (Blickle et al., 2020).

[6]Prior literature also specifies other ways to identify lead arrangers but does not sperate co-agents (e.g., Sufi (2007); Ivashina (2009); Bharath et al. (2011)). Whether the lender is a lead arranger defined in each of the different definitions has a correlation of more than 0.85 with the definition used in this study.

[7]I also plot the changes in the proportion of loans with different lender roles as the lead arranger over time in Figure B.1 and Figure B.2 in Appendix B.1.2.

which could be because loans are larger in amount and lenders are more conservative in taking risks.

Insert Figure 3.1 here.

Further, the sample excludes all packages to financial firms (SIC between 6000 and 6999) as well as all observations with non-positive assets and equity of both lenders and borrowers. I then merge the Dealscan-Compustat data with Capital IQ S&P Credit Ratings for borrowers' credit ratings.

3.3.2 Empirical Models and Variable Construction

I use two regression models to test the information production.

First, to identify whether having multiple lead arrangers improves ex-ante screening while acknowledging a "hold-up" issue, I evaluate the impact of co-lead arrangement on loan pricing. If a loan is correctly priced, riskier firms should pay higher spreads than safer borrowers, which indicates a high correlation between loan spreads and borrowers' creditworthiness. It is important to note that the information set about a borrower's creditworthiness is not fully publicly available to all investors, and private information is critical to screening and loan pricing (Boot, 2000). If more private information is produced, one should expect a higher spread-risk correlation (Botsch and Vanasco, 2019). As discussed in the previous section, having multiple lead arrangers can produce more private information by a) increasing the chance of obtaining a true single about the borrower's type, b) improving the efficiency of due diligence, c) verifying the quality of the private information, and d) sharing each other's information in past lending experiences. As a result, I expect that the correlation between loan spreads and a borrower's creditworthiness is more negative in co-lead loans than in single-lead arranger loans. The regression model is specified as follows.

$$
\begin{aligned}
\text{Loan Spreads}_l = &+\beta_1 \text{Co-lead Arrangement}_l \times \text{Creditworthiness}_{ft} \\
&+ \theta_1 \text{Co-lead Arrangement}_l + \delta_1 \text{Creditworthiness}_{ft} \\
&+ \sum \boldsymbol{\theta} \text{Loan-level Controls}_l + \sum \boldsymbol{\delta} \text{Borrower-level Controls}_{ft} \\
&+ \sum \boldsymbol{\eta} \text{Lender - level Controls}_{bt} + \alpha_f + \alpha_t + \varepsilon_l
\end{aligned} \tag{1}
$$

This is a loan-level regression. Subscript l, f, b, and t stand for loan, firm, bank and year of loan origination, respectively. Variables are defined in Table B.1 in Appendix B.1.1. The dependent variable is loan spreads, which is the Dealscan item "all-in-drawn spreads". If a package contains two or more facilities, I average the loan spreads across all facilities.

Creditworthiness measures a borrower's creditworthiness that is only privately observable at the time of loan origination. Co-lead Arrangement$_l$ is a dummy taking one if a loan has more than one lead arranger. The intention term in Equation 1 is the variable of interest. I follow Botsch and Vanasco (2019) to measure creditworthiness using a firm's future credit rating. Only a privately observable risk proxy is relevant in testing information production. Otherwise, a publicly observable risk measure can be priced into loan spreads even if there is no additional private information. Because the variable is observed from a period outside the sample, it is publicly unobservable but can be inferred from private knowledge or through due diligence. In particular, Creditworthiness is a borrower's lowest S&P credit rating within five years after the loan origination. This makes the variable loan specific.[8] To digitise the ratings, I assign 1 to the lowest rating and 28 to the highest rating.[9] This variable measures how worse a firm's credit performance could be, as an assessment from one of the largest American credit agencies. Because a borrower granted a lower credit

[8]The results are similar if I collect the ratings during the maturity of a loan. However, because some loans have low maturity, the future ratings could be too close to the current rating observed before or at the time of loan origination.

[9]I follow Botsch and Vanasco (2019), who "convert the ordinal S&P long-term debt ratings into a cardinal scale from 1 to 28, in which an increment of ± 3 represents a full rating change and ± 1 a sub-rating (plus or minus) change: 28 = "AAA", 25 = "AA", 22 = "A", ..., 4 = "C", 3 = "D" (Default) and 1 = "SD" (Selective Default)."

rating is likely to default in the future, lenders compensate for this by asking for higher spreads. I expect a negative correlation between a firm's future rating and loan spreads (i.e., a negative δ_1). If the co-lead arrangement reveals more private information, I expect a more negative correlation; that is, a negative β_1. Regarding θ_1, I expect a positive sign, which is consistent with the "hold-up" problem. That is, compared to a single-lead arranger loan, multiple lead arrangers can exert their bargaining power to impose higher "hold-up" costs on risky borrowers (Botsch and Vanasco, 2019).

Equation 1 also controls loan-level characteristics. These include several loan terms representing the non-pecuniary pricing of a loan, such as the loan amount, loan maturity, and the number of covenants. To further capture differences in loan types, I control for loan purposes and include two dummies – whether a package has a revolving facility and whether a package is secured. The loan-level variables also include syndicate size – number of co-agents and number of all participating lenders and relationship variables – distance and relationship intensity. For the distance measure, I calculate the geographic distance between the lead arrange and the borrower; if there are multiple lead arrangers, I average the values across each lead arranger. For relationship intensity, I follow Bharath et al. (2007); Schenone (2010). I first count the number of loans that the firm and the lead arranger(s) of a loan have ever contracted within five years before the loan origination. I then divided it by the number of total loans the firm has borrowed in the past five years. It is necessary to control for related variables. Because a lead arranger is more likely to be chosen from relationship lenders, having more lead banks means the borrower is more likely to have a stronger relationship with the lenders. If I do not control for relationships, it is possible that the impact of the co-lead dummy on loan pricing comes only from higher relationship intensity, not from extra private information produced by additional lead arrangers.

Then, I include borrower-specific controls in Equation 1. Importantly, I control for a borrower's credit rating at the time of loan origination, capturing the correlation between current and future ratings. Other borrower-specific variables contain default risks that are publicly observable, proxied by Altman's z-score, informationally opacity, measured by the number of analyst coverages, Tobin's Q, and asset tangibility, financial constraints, measured by total assets, profitability, and leverage.

The last set of controls in Equation 1 are lender-specific variables, including total assets, the capital ratio, and exposures to the loan. The last variable proxies the stake of a lender in the loan. A higher stake will encourage more diligent screening and monitoring. The literature conventionally uses lend arrangers' shares in the loan. Although Dealscan reports the lead arranger share, it has many missing observations. Thus, I use the total loan amount over a lender's assets as a proxy, though the results are qualitatively the same if I instead control for the lend arranger share. For co-lead loans, all lender-specific variables are averaged across the lead arrangers.

I lag all of the borrower-and lender-specific variables by one quarter to mitigate the simulated bias issue. Equation 1 also includes industry-fixed effects (α_f) to capture unobservable time-invariant industry-related factors and year-fixed effects (α_t) controlling for macroeconomic factors. The standard errors are clustered at the borrower level to account for the within-borrower correlation in the error terms (Petersen, 2008).

Next, the second regression model tests whether the co-lead arrangement enhances ex-post monitoring. Specifically, I regress the occurrence of covenant violations on the co-lead dummy. Because of the free-rider problem, the impact of covenant violations can be ambiguous. I then interact with measures for lender reputation in the following regression model.

$$\begin{aligned}\text{Covenant Violations}_l = &+\beta_1 \text{Co-lead Arrangement}_l \times \text{Reputation}_{bt} \\ &+ \theta_1 \text{Co-lead Arrangement}_l + \eta_1 \text{Reputation}_{bt} \\ &+ \sum \boldsymbol{\theta} \text{Loan-level Controls}_l + \sum \boldsymbol{\delta} \text{Borrower-level Controls}_{ft} \\ &+ \sum \boldsymbol{\eta} \text{Lender - level Controls}_{bt} + \alpha_f + \alpha_t + \varepsilon_l \end{aligned} \tag{2}$$

In Equation 2, the dependent variable is the frequency of covenant violations during the loan life. I define a violation if the following condition is satisfied before the loan matures: the underlying variable of a covenant is lower than the minimum threshold or higher than the maximum threshold in a given quarter-end.[10] Thus, the frequency of covenant violation measures the number of quarters where a covenant is violated. If a loan contains more than one covenant, I sum the violations across all covenants. If borrowers are monitored more diligently, I expect a lower number of violation times. Thus, without the interaction term in Equation 2, the sign and statistical significance of θ_1 will depend on the relative strength of the monitoring enhancement and free-rider problem.

Then, because reputation encourages monitoring, I expect the monitoring enhancement to dominate the free-rider problem. To test this, I include the interaction between a reputation measure and the co-lead dummy. Following related literature, I use both size (Puri, 1996) and market share (Shivdasani and Song, 2011) to proxy lender reputation. For larger lead arrangers and those having more market shares, in order to maintain reputation, they are more likely to dedicate their own monitoring efforts instead of reaping from others. A lender's market share is the number of loans taken by the lender divided by the number of all loan packages in a given quarter. I then average all lead arrangers' market shares for co-lead loans.[11] Thus, I expect the impact of co-lead arrangement on restricting covenant violations to be more prominent when Reputation is larger, which predicts a significantly negative β_1. For the other right-hand-side variables, I include most of the controls in Equation 1. Because the ex-ante covenant strictness can affect the probability of ex-post violation (Murfin, 2012), I also control for the measure of ex-ante strictness developed by Demerjian and Owens (2016). In addition, I replace the current credit rating with a dummy indicating whether a borrower has an S&P rating or not. This allows the inclusion of unrated firms, thus increasing the sample size.

[10]Following Murfin (2012), covenant variables with minimum thresholds include the current ratio, quick ratio, tangible net worth, total net worth, EBITDA, fixed charge coverage, and interest coverage, while those with maximum thresholds are the debt to EBITDA, debt to equity, debt to tangible net worth, and capital expenditure.

[11]I use average instead of sum to make it comparable to the lead arranger's market share in a single-lead arranger loan.

3.3.3 Endogeneity Issues

A co-lead loan could be quite different to a single-lead arranger loan. If the differences affect the spreads and covenant violations through a channel other than information production, the results from OLS regressions will be biased. Although I use a large set of controls and fixed effects to minimise the omitted variable bias, there could be unobservable or unmeasurable variables correlated with the co-lead dummy being omitted in the error term. To solve this, I use an exogenous variable to instrument the co-lead dummy. A valid instrumental variable (IV) should satisfy both the exclusion restriction and the relevance condition. The former condition restricts the IV to be orthogonal to the error term, and the latter requires the IV to be correlated with the endogenous variable. In other words, the IV should only affect loan spreads and covenant violations through its impact on the co-lead dummy. In this study, I use the number of a borrower's past relationship lenders as the IV for co-lead arrangement. The past relationship lenders are the count of lead arrangers that have ever lent to the borrower within five years before the loan origination. At the beginning of loan syndication, a borrower is likely to invite its relationship lenders to arrange the loan. If a firm has more relationship lenders, it has more potential lead arrangers to choose from and thus is more likely to form a co-lead loan. This satisfies the relevance condition. Among all past relationship lenders, those that are not chosen as lead arrangers of the current loan will not affect loan spreads and covenant violations, which satisfies the exclusion restriction; for those being current lead arranger(s), they could affect spreads and violations through relationship lending, which may violate the exogenous condition. However, this is resolved because I include relationship intensity as control (Jordà et al., 2020). After identifying the IV, I use a two-stage regression. First, I run a probit regression of the co-lead arrangement dummy on the number of relationship lenders and a set of controls. Second, I replace the co-lead dummy with the fitted value from the first-stage regression and estimate Equation 1 and Equation 2.

3.4 Empirical Results

Section 3.4.1 describes summary statistics. In Section 3.4.2 and Section 3.4.3, I test the impacts of co-lead arrangement on loan spreads and covenant violations, respectively.

3.4.1 Descriptive statistics

Table 3.1 describes summary statistics of key variables tested in this study. There are, on average, 14.43% loans having multiple lead arrangers, and an average loan package has 1.46 lead arrangers and 2.26 co-agents in a loan package. The average loan spreads are nearly 2%, which is higher than the conventional threshold of leveraged loans. Most borrowers have at least one relationship lender, and less than half of the borrowers have more than one. Also, more than 80% of the packages include a revolving facility, and just below half of the packages have a secured facility. Regarding covenants, the ex-ante strictness is highly skewed. There are more than half of the packages have covenants, and the average violation frequency is 5.35 times. In terms of the credit rating, the future rating (averaging between BB and BB+) is lower than the current rating (averaging between BB+ and BBB-), with just above a full-rating standard deviation. This is not surprising because the variable is measured by the future lowest rating. On average, the lead arranger(s)'s market share and capital ratio are 4.11% and 7.84%, respectively.

Insert Table 3.1 here.

3.4.2 Co-lead Arrangements Improve Ex-ante Screening

This section presents regression results of loan spreads on the co-lead dummy. As discussed in Section 3.2, having multiple lead arrangers allow the lenders to share their private information about a borrower and to reduce the cost of due diligence. Then, the improved screening can be reflected in lower loan spreads. However, because the co-lead arrangement also gives the lender side more bargaining power, the lead arrangers can exert higher information rents from the borrower. Thus, whether the loan spreads are higher or lower in a co-lead loan than in a single-lead arranger loan is an empirical question. Table 3.2 presents the results. In the first column, I estimate Equation 1 without the interaction term. The coefficient of the co-lead dummy is significantly positive in Column 1. Thus, after controlling for borrowers' and lenders' characteristics as well as other loan terms, the spreads of a co-lead loan are 8.62 basis points (bps) higher than that of a single lead arranger loan. This indicates that the effect of higher bargaining powers dominates the effect of screening improvement. Although I include a large set of control variables, the unobservable difference between co-lead and single-lead loans can cause an issue of omitted variables. Thus, I use the number of past relationship lenders as an IV to run a two-stage IV regression.

The coefficients in the first-stage Probit regression are reported in Column 2. The evidence confirms the risk-sharing as a motivation for co-lead arrangements. It shows that larger loans tend to have multiple lead arrangers. Also, co-lead syndicates are more likely to happen if borrowers have lower credit ratings or lower Altman's z-score. Next, given the negative coefficient of the bank capital ratio, co-lead loans also tend to be used to resolve banks' capital constraints. Further, the number of co-agents is negatively correlated with the probability of having multiple lead arrangers. Thus, co-lead arrangement substitutes co-agents' function to reduce management burdens. Lastly, the positive coefficient of the lender-borrower distance indicates that the co-lead arrangement is a substitute for geographic proximity as an informational advantage.

Turning to the second-stage regression in Column 3, the coefficient of the co-lead dummy becomes insignificant, meaning that the opposite effects of co-lead arrangement on loan spreads cancel each other. Thus, the OLS estimate in Column 1 is biased upward. However, an alternative explanation for the insignificant coefficient is that neither the screening improvement nor the "hold-up" problem is valid. To further test the role of co-lead loans in improving ex-ante screening, I estimate Equation 1, which includes the interaction term between the co-lead dummy and the creditworthiness proxy measured by a borrower's lowest future credit rating. The coefficient measures how much the borrower's privately available risk information is included in loan pricing in co-lead loans relative to single-lead arranger loans. Column 4 and Column 5 present the results, respectively, for OLS and IV regressions. The coefficient of the creditworthiness proxy is significantly negative, consistent with the risk-return trade-off. Importantly, I include the credit rating in the loan origination month as a measure of publicly observable riskiness. The current credit rating also has a negative coefficient and does not absorb the impact of the unobservable proxy. Thus, the future credit rating contains additional private information on a borrower's creditworthiness. Regarding the variable of interest, both OLS and IV regressions present a negative coefficient of the interaction term, suggesting that co-lead arrangement intensifies the correlation between loan spreads and a borrower's future credit performance. Thus, with multiple lead arrangers, lenders can more precisely distinguish healthy from risky borrowers. In other words, only creditworthy borrowers benefit from co-lead arrangements, while the risky borrower has to pay higher loan spreads. Lastly, many contractual terms of a loan could be jointly determined. To address this issue, I exclude loan terms from the regression, and the results presented in column 6 remain unchanged.

To interpret the magnitude, consider two borrowers with a full-rating difference (e.g., an A-rated firm and a BBB-rated firm). A firm's publicly observable riskiness should be fully priced into loan spreads. Thus, I use the coefficient of the current credit rating as a benchmark. The coefficient indicates that the risk premium of a full-rating downgrade should be 38 bps. Instead, the coefficient of the lowest future rating proxying for unobservable riskiness is -2.75, which is much smaller than the coefficient of the current credit rating. This means that, in a single-lead arrange loan, the borrower with a lower future rating only pays 8.25 bps higher than another solvent borrower. Thus, a single lead arranger has limited ability to reveal the borrower type. However, the co-lead arrangement enlarges the spread difference to 17.74 bps (using the magnitude in Column 4), making loan pricing more informative. The coefficient in the IV regression further enlarges the distinction between co-lead loans and single-lead arranger loans, making the spread difference in a co-lead loan three times as much as that in a single-lead arranger loan. Overall, the results confirm that the co-lead arrangement improves ex-ante screening by uncovering more information about a borrower's future credit performance.

Next, I briefly discuss the coefficients of other variables. Larger and short-term loans have lower spreads. Also, the syndicate size, measured by the number of lenders, has a negative coefficient. This indicates that lenders in a larger syndicate enjoy a higher diversification effect. The coefficients of the lending relationship intensity are consistent with prior literature, such as Schenone (2010); Bharath et al. (2011). Specifically, higher lending relationships are associated with lower loan spreads. This follows the expectation that repeated lending reduces the information asymmetry between lenders and borrowers, thus lowering loan spreads. Moreover, the negative coefficient of Altman's z-score aligns with the risk-return trade-off. Regarding bank characteristics, a loan led by poorly-capitalised banks tends to have higher spreads. This contradicts the prediction that lenders holding more expensive capital pass the cost to borrowers (Bolton and Freixas, 2000; Bolton et al., 2016). However, the negative relationship can be interpreted as a "chase-for-yield" behaviour of insolvent banks to replenish their capital levels (Santos and Winton, 2019).

Insert Table 3.2 here.

3.4.3 Co-lead Arrangements Enhance Ex-post Monitoring

In this subsection, I test another reflection of the co-lead arrangement's role in information production – enhancing ex-post monitoring. If lenders in a co-lead loan can collect information and supervise a borrower's behaviour more diligently and efficiently than in a single-lead arranger loan, they can limit the firm's excessive risk-taking. As a result, I expect the borrower's covenant variables to breach the thresholds less frequently in a co-lead loan than in another loan. Thus, in Table 3.3, I regress the number of covenant violations (in logarithms) on the co-lead dummy. In both OLS and IV regressions, the impact of the co-lead arrangement is negative. Although the sign of the coefficient is consistent with the prediction of co-lead arrangement enhancing monitoring, it is not statistically significant. As outlined in Section 3.2, one explanation for the insignificance is the free-rider problem. The presence of multiple lead arrangers would impair monitoring incentives because the effort of monitoring cannot be perfectly detected. Thus, each lender tends to wait and take advantage of others' efforts. Because reputation can limit lenders' opportunistic behaviours, I expect that the monitoring enhancement of the co-lead arrangement dominates the free-rider problem if lenders are prestigious. To do so, I interact two measures of lender reputation with the co-lead dummy in Column 3&4 and Column 5&6, respectively. I first use lender size to proxy reputation. Larger firms are more transparent and have more analyst coverage, so the market pays more attention to them compared to smaller banks. Also, because of "too-big-too-fail", regulators tend to supervise larger firms more closely. Therefore, I expected larger lead arrangers to be more reputable will monitor the borrowers' ex-post behaviours more diligently. The result in the OLS regression supports the expectation. Specifically, an increase in average lead arranger size leads to less frequent covenant violations in co-lead loans relative to that of single-lead arranger loans. The IV regression presents a negative but insignificant coefficient of the interaction term between co-lead arrangement and lender size. In the next columns, I use lead arrangers' quarterly market shares to proxy lender reputation. Lenders with more market shares are the top lenders in the syndicated loan market and, thus, tend to be more reputable. In both OLS and IV regressions, the coefficients of the interaction term are significantly negative. The result indicates that market share could be a stronger proxy for reputation than lender size. Thus, the co-lead loans with, on average, higher market shares induce less frequent covenant violations than those with lower reputable lead arrangers. To interpret, a one-standard-deviation increase in market shares makes the impact of co-lead arrangements on the violation frequency to be 2% more negative. Thus, the lead arranger reputation intensifies the co-lead arrangement's role in monitoring enhancement. Next, I quantify the marginal impact of co-lead arrangement conditional on high lead arrangers' reputation. To do so, I first take the derivative of the co-lead dummy – $\frac{\partial \text{Covenant Violations}}{\partial \text{Co-lead Arrangement}} = \theta_1 + \beta_1 \text{Reputation}$. I extract the value of market shares in the 90th percentile (i.e., 0.058) to represent highly reputable lead arrangers. I also conduct an F-test on whether the impact of co-lead arrangement conditional on reputable lead arrangers is statistically different from zero. In both regressions, the marginal impacts are significant at, at least, a 10% level. Thus, according to the coefficient in the OLS regression, the number of covenant violations is reduced by 5.8% in a co-lead loan than in a single-lead arranger loan when the lead arrangers have, on average, the largest market shares. Overall, the results suggest that co-lead arrangement can enhance ex-post monitoring but only when the lead arrangers are highly reputable.

Next, I briefly discuss the control variables. Demerjian and Owens (2016)'s measure of ex-ante covenant strictness is positively related to the number of covenant violations. The result is not a surprise. A stricter covenant means that the difference between the covenant variable and the threshold is initially lower; thus, it is more likely to observe more occasions of violation. This is also true for the number of covenants. Moreover, covenants are violated less frequently when the relationship between the lead arranger(s) and the borrower is more intense. This further shows that lending relationships facilitate information production and, thus, monitoring. Also, larger lead arranger(s) induce more violations because a large and complex corporate structure has disadvantages in conveying messages and monitoring. This could explain why I find weaker results when using lender size to proxy for monitoring incentives.

Insert Table 3.3 here.

3.5 Further Tests and Robustness Checks

3.5.1 The Intensive Margin of Monitoring Enhancement

In Section 3.4, I test the role of co-lead arrangements in enhancing monitoring in terms of the extensive margin. In this subsection, I explore the intensive margin. Specifically, I test the severity of covenant violations. I first restrict the sample to all loans that experience covenant violations. Then, I run Equation 2 by replacing the dependent variable with the relative difference between the threshold and the covenant variable in a given quarter-end. I take the absolute value of the difference, so a lower value of the dependent variable means a less severe violation. If there are multiple violations during the loan life, I average the differences. The results are presented in Column 1 of Table 3.4. Here, I only report the IV regression and use market share as the proxy for reputation. The variable of interest is the interaction between the co-lead dummy and the lead arranger market share. The coefficient of the interaction terms is significantly negative. The result suggests that the severity of covenant violations can be reduced by a co-lead arrangement with highly reputable lenders.

3.5.2 Other Robustness Checks

In this subsection, I conduct several robustness checks. First, I replace the co-lead dummy with the number of lead arrangers (in logarithms). I run the IV regressions of Equation 1 and Equation 2, respectively, in Column 2 and Column 3 of Table 3.4.

Then, I use a stricter model specification by changing the industry-fixed effects to industry-year fixed effects, which captures industry-specific time-varying characteristics of borrowers. The results are reported in Column 4&5 of Table 3.4.

Next, I check the robustness of the main results using different definitions of lead arrangers. The prior literature has used several ways to define lead arrangers in Dealscan. I use Cai et al. (2018)'s definition because it allows me to separate the lead arrangers from co-agents which are the tier-two senior lenders in syndicated loans. I then use two other definitions, which are widely used in existing papers. The first one is from (Ivashina, 2009). A lead arranger is identified if the Dealscan item "lenderrole" is admin agent. If there is no admin agent in the loan, the lead arranger is identified if the Dealscan item "lenderrole" is one of agent, arranger, bookrunner, lead arranger, lead bank, and lead manager. Column 6&7 present the results. The second definition is from (Bharath et al., 2011), who study the relationships between banks and firms. In this definition, a lead arranger is identified if the Dealscan item "Lead Arranger Credit" is "Yes" or the Dealscan item "lenderrole" is one of admin agent, arranger, lead bank. Also, following (Bharath et al., 2011), I do not define the syndications agent as a lead arranger because its role is less specified. Column 8&9

present the results.

In all regression specifications, the main findings are quantitatively unchanged.

Insert Table 3.4 here.

3.5.3 Propensity Score Matching

Previously, I address the endogenous difference between co-lead loans and single-lead arranger loans using the IV regression. In this section, I use the method of propensity score matching. Specifically, I first run a logit regression of the co-lead dummy on a set of characteristics of the loan, the borrower, and the lead arranger(s). These characteristics include loan amount, the longest facility maturity, revolving facility dummy, secured dummy, number of covenants, number of co-agents, number of lenders, number of analyst coverage, lender-borrower distance, lending relationship intensity, Altman's Z-score, profitability, asset tangibility, firm size, financial leverage, Tobin's Q, lead arranger capital ratio, lead arranger exposure, loan purpose dummies, borrower industry dummies, and year dummies. According to the propensity scores, I match each co-lead loan with the four closest single-lead arranger loans. Using the sample, I run OLS regressions. Table 3.5 presents the results. All the results confirm previous findings. That is, riskier borrowers pay higher spreads in co-lead loans compared to single-lead arranger loans, while creditworthy firms pay less. Also, condition on larger lead arranger size or higher market shares, the co-lead arrangement induces lower covenant violations relative to single-lead arranger loans.

Insert Table 3.5 here.

3.6 Conclusion Remarks

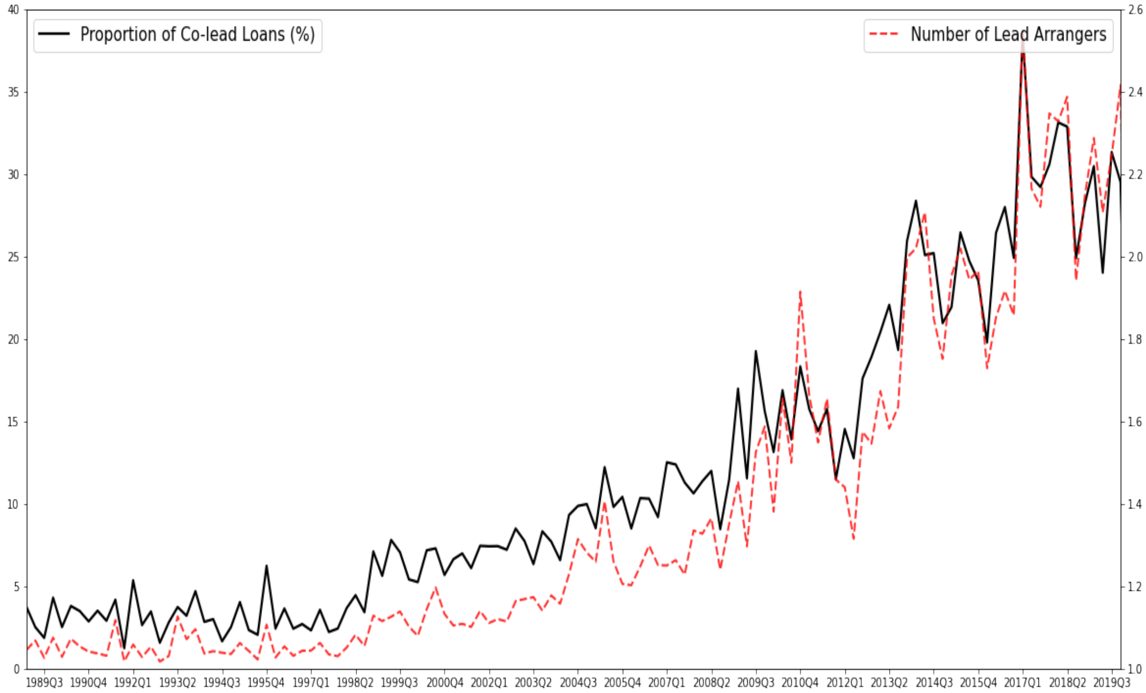
In this study, I study the role of co-lead arrangements in helping produce private information. I find that the proportion of loans with multiple lead arrangers has risen dramatically in the past two decades. I use the loan-level data and find that the co-lead arrangement improves ex-ante screening by incorporating more borrowers' privately available information into loan spreads. The co-lead arrangement also facilitates ex-post monitoring but is only conditional on high lead arrangers' reputations. Specifically, I find that when lead arrangers, on average, have large market shares, the frequency of covenant violations during the loan life is lower in co-lead loans than in single-lead arranger loans. This paper focuses on testing the role of co-lead arrangements in information production. Although I propose several motivations for the co-lead arrangements to emerge, I do not discuss in detail whether information production is the motivation or a by-product. Understanding the motivations is crucial to explain the increasing co-lead loans, especially after the GFC; future research can conduct a further and formal test of the reasons behind it.

Conventionally, the literature recognises lead arrangers as informed lenders while participants as uninformed lenders (Sufi, 2007; Schwert, 2018). In this paper, evidence indicates that lead arrangers share information in order to price a loan more precisely. However, participants can also accumulate their own private information about a borrower through repeated lending. Prior literature provides indicative evidence. For instance, Sufi (2007) find that lead arrangers tend to select knowledgeable lenders as participants, acknowledging the value of participants' information. Also, Botsch and Vanasco (2019) and Degryse et al. (2021) point out that lead arrangers can learn not only from other lead arrangers but also from participants. Therefore, whether there is information sharing from participants to lead arrangers deserves further investigation.

Recent papers such as Aldasoro et al. (2022, 2023) have documented the increasing participation of shadow banks (e.g., mutual funds and investment banks) in syndicated loans. These studies point out that the loan portfolios of shadow banks are different from those of commercial banks. Also,Billett et al. (2016); Berlin et al. (2020) find that the trend of increasing participation of non-bank institutions in leveraged syndicated loans leads to a rise in covenant-lit loans. Moreover, lenders from other countries may have incentives to co-lead with domestic lenders who are more familiar with the market. Thus, I suggest future research to investigate whether co-leading with non-bank lenders or foreign lenders affects the loan terms.

Last but not least, this study focuses on co-lead arrangements at the time of loan origination. Whether the co-lead status remains the same during the whole life of a syndicated loan is worth studying. Future work can explore the consistency of the co-lead status using the data in the secondary market.

Figure 3.1: The Trend of the Co-lead Loans



This figure plots the proportion of co-lead loans (solid black line; left axis) and the average number of lead arrangers (dotted red line; right axis) in each quarter.

Table 3.1
Summary Statistics

	Mean	SD	25^{th}	Median	75^{th}
Co-lead Arrangement	0.1443	0.3514	0	0	0
No. of Lead Arrangers	1.4553	1.6169	1	1	1
No. of Co-agents	2.2587	2.6798	0	1	4
No. of Lenders	6.4717	5.1375	2	5	9
Loan Spreads	197.395	129.6230	100	175	266.6667
No. Past Relationship Lenders	2.3522	2.8615	1	1	3
Log(Loan Amount)	19.5789	1.5607	18.6438	19.6734	20.7233
Log(Loan Maturity)	49.6075	24.9232	36	60	60
Revolving Facility Dummy	0.8103	0.3921	1	1	1
Secured Dummy	0.4518	0.4977	0	0	1
Covenant violations	5.3484	8.8008	0	1	7
Covenant Strictness	0.311	0.4018	0.004	0.057	0.782
No. of Covenant	1.1618	1.3511	0	1	2
Lender-borrower Distance	1.4635	1.1769	0.5235	1.1558	2.2812
Lending Relationship Intensity	0.539	0.4225	0	0.5556	1
Lowest Future Rating	16.4088	4.3041	14	17	19
Current Credit Rating	17.6111	3.5097	15	18	20
Unrated Dummy	0.4499	0.4975	0	0	1
Altman's Z-score	0.5532	0.9158	0.1881	0.6163	1.1025
Analyst Coverage	2.3921	5.6496	0	0	0
Tobin's Q	1.7503	0.9963	1.1293	1.4378	2.0075
Asset Tangibility	0.3225	0.2514	0.1116	0.2477	0.5025
Profitability	0.0326	0.0267	0.0201	0.0313	0.0448
Firm Size	7.7813	1.9547	6.4619	7.741	9.1431
Financial Leverage	0.3064	0.2080	0.1611	0.2868	0.4146
Log(Lead Arranger Size)	13.6419	1.1150	13.1578	13.9882	14.4876
Lead Arranger Market Share	0.0411	0.0227	0.0283	0.0383	0.0497
Lead Arranger Capital Ratio	0.0784	0.0222	0.065	0.0811	0.0927
Lead Arranger Exposure	0.001	0.0017	0.0002	0.0004	0.001

Notes: This table presents summary statistics for the key variables used in this paper from 2000Q1 to 2019Q4. Co-lead arrangement is a dummy taking one if the loan package has more than one lead arranger. Loan spreads are the Dealscan item " all-in-drawn spreads" averaging across all facilities. No. Past Relationship Lenders is the number of lead arrangers that have ever lent to the borrower within five years before the loan origination. Log(loan maturity) is the longest facility maturity (in logarithms) in a loan package. Revolving facility dummy takes one if a loan package includes a revolving facility. Secured dummy takes one if a loan package includes a secured facility. Covenant violations is the number of violation times of all covenants in a loan package. A violation happens when the underlying variable of a covenant breaches the threshold in a given quarter-end. Covenant Strictness is the ex-ante measure for covenant Strictness, sourced from Demerjian and Owens (2016). Lendin relationship intensity is the number of loans that the firm and the lead arranger(s) of a loan have ever contracted within five years before the loan origination divided by the number of total loans the firm has borrowed in that period. Lowest future rating is a borrower's lowest S&P credit rating within five years after the loan origination date. Current credit rating is the S&P rating in the loan origination month. Unrated dummy takes one if a borrower does not have an S&P credit rating in the month of loan origination. Lead arranger market share is the number of loans taken by one lender divided by the number of all loans in a given quarter. Lead arranger exposure is the total loan amount over the lender's assets. All lead arranger-specific variables are averaged across all lead arrangers. All borrower- and lender-specific variables are lagged by one quarter. See Table B.1 for further information on variable definitions.

Table 3.2

Co-lead Arrangements Intensify the Relationship between Loan Spreads and a Borrower's Privately-observable Creditworthiness

	1	2	3	4	5	6
	OLS Regression	IV Regression-1st Stage	IV Regression-2nd Stage	OLS Regression	IV Regression-2nd Stage	IV Regression-2nd Stage
Co-lead Arrangement	8.623**		-7.578	58.93***	69.42***	94.92***
	(2.23)		(-0.71)	(3.68)	(2.92)	(3.66)
Lowest Future Rating				-2.749***	-2.497***	-2.347***
				(-4.00)	(-3.60)	(-3.23)
Co-lead Arrangement×Lowest Future Rating				-3.164***	-5.053***	-5.430***
				(-3.33)	(-3.50)	(-3.43)
Log(No. Past Relationship Lenders)		0.455***				
		(8.12)				
Log(Loan Amount)	-3.800*	0.114***	-3.441	-3.982*	-3.658*	
	(-1.78)	(2.73)	(-1.58)	(-1.87)	(-1.69)	
Log(Loan Maturity)	12.01***	0.185***	12.33***	12.14***	12.36***	
	(5.21)	(3.71)	(5.23)	(5.34)	(5.34)	
Revolving Facility Dummy	-28.35***	-0.194***	-29.31***	-27.92***	-29.08***	
	(-7.24)	(-2.91)	(-7.28)	(-7.21)	(-7.30)	
Secured Dummy	36.55***	0.261***	37.37***	36.87***	38.02***	
	(9.47)	(3.96)	(9.51)	(9.59)	(9.72)	
Log(No. of Covenant)	-3.125	-0.399***	-4.254**	-2.947	-4.048*	
	(-1.55)	(-7.21)	(-1.98)	(-1.47)	(-1.90)	
Log(No. of Co-agents)	-1.158	-1.061***	-4.494	-1.343	-5.709*	-3.392
	(-0.57)	(-19.32)	(-1.52)	(-0.67)	(-1.95)	(-1.13)
Log(No. of Lenders)	-26.89***	0.955***	-24.07***	-26.82***	-22.63***	-30.98***
	(-10.12)	(13.96)	(-8.13)	(-10.30)	(-7.62)	(-10.86)
Lender-borrower Distance	0.515	0.0886***	0.732	0.901	1.115	1.412
	(0.34)	(2.59)	(0.47)	(0.60)	(0.73)	(0.91)
Lending Relationship Intensity	-6.036**	0.577***	-4.638*	-6.334**	-5.334**	-6.995**
	(-2.33)	(6.29)	(-1.77)	(-2.46)	(-2.06)	(-2.57)
Current Credit Rating	-15.79***	-0.0551***	-15.97***	-12.67***	-12.71***	-15.89***
	(-17.90)	(-3.81)	(-17.86)	(-11.83)	(-11.80)	(-15.30)
Altman's Z-score	-7.321**	-0.106**	-7.736***	-6.849**	-7.151**	-7.547***
	(-2.57)	(-1.96)	(-2.71)	(-2.41)	(-2.53)	(-2.62)
Log(Analyst Coverage)	-0.555	-0.0362	-0.631	-0.339	-0.376	-0.196
	(-0.71)	(-1.55)	(-0.80)	(-0.44)	(-0.49)	(-0.24)
Tobin's Q	-1.885	0.0572	-1.544	-1.443	-1.321	-2.877
	(-0.96)	(1.46)	(-0.78)	(-0.73)	(-0.67)	(-1.41)
Asset Tangibility	0.873	-0.136	0.363	-1.576	-2.145	-4.343
	(0.10)	(-0.61)	(0.04)	(-0.18)	(-0.25)	(-0.49)
Profitability	-295.6***	2.342	-299.3***	-263.6***	-270.2***	-238.1***
	(-3.70)	(1.50)	(-3.73)	(-3.35)	(-3.41)	(-2.96)
Firm Size	4.008**	0.0203	4.168**	4.354**	4.371**	3.005*
	(2.24)	(0.55)	(2.32)	(2.45)	(2.45)	(1.85)
Financial Leverage	23.31**	-0.591***	22.33**	20.61**	21.03**	32.04***
	(2.58)	(-3.25)	(2.44)	(2.32)	(2.34)	(3.39)
Lead Arranger Capital Ratio	-176.6**	-34.53***	-290.1***	-173.2**	-293.3***	-283.9**
	(-2.05)	(-16.78)	(-2.71)	(-2.02)	(-2.77)	(-2.53)
Lead Arranger Size	-9.312***	-0.347***	-10.33***	-9.552***	-10.47***	-12.58***
	(-3.70)	(-6.90)	(-3.92)	(-3.86)	(-4.06)	(-5.01)
Lead Arranger Exposure	17.44	28.30	115.1	-21.09	190.9	-504.9
	(0.02)	(1.43)	(0.14)	(-0.03)	(0.23)	(-0.63)
Sample Period	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019
Loan Purpose-, Industry-, State-, Year-FEs	Yes	Yes	Yes	Yes	Yes	Yes
No. of Observations	8189	8763	8096	8175	8083	8163
Adjusted/pseudo R^2	0.651	0.478	0.650	0.655	0.654	0.630

Notes: This table reports estimates from the OLS regression model and the two-stage IV regression model. The unique observation is a loan package in Dealscan. The sample period is 2000Q1-2019Q4. In Column 1&4, I regress loan spreads on co-lead arrangement and its interaction term with the borrower's lowest future rating. Loan spreads are the Dealscan item " all-in-drawn spreads" averaging across all facilities. Co-lead arrangement is a dummy taking one if the loan package has more than one lead arranger. Lowest future rating is a borrower's lowest S&P credit rating within five years after the loan origination date. In Column 3, 5, 6, I use a two-stage IV regression model. The IV is log(No. Past Relationship Lenders), measured by the number of lead arrangers (in logarithms) that have ever lent to the borrower within five years before the loan origination. Column 2 reports the results of the first-stage probit regression, where I regress co-lead arrangement on past relationship lenders. The results of the second-stage regression where I replace co-lead arrangement with the fitted value in the first-stage regression, is presented in Column 3, 5, 6. The control variables are also reported and are defined in Table B.1. Lendin relationship intensity is the number of loans that the firm and the lead arranger(s) of a loan have ever contracted within five years before the loan origination divided by the number of total loans the firm has borrowed in that period. Current credit rating is the S&P rating in the loan origination month. Lead arranger exposure is the total loan amount over the lender's assets. All lead arranger-specific variables are averaged across all lead arrangers. All borrower- and lender-specific variables are lagged by one quarter. Loan purpose-, industry-, state- and year-fixed effects are included in all regression specifications. T-statistics based on standard errors clustered by firms are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Table 3.3

Covenant Violations are Less Frequent in Co-lead Loans with Highly Reputable Lead Arrangers

	1	2	3	4	5	6	7	8
	OLS Regression	IV Regression-2nd Stage	OLS Regression	IV Regression-2nd Stage	IV Regression-2nd Stage	OLS Regression	IV Regression-2nd Stage	IV Regression-2nd Stage
Co-lead Arrangement	-0.254	-0.0751	1.602*	1.621	1.450	0.213	0.364	0.177
	(-1.54)	(-1.11)	(1.70)	(1.26)	(1.08)	(1.42)	(1.35)	(0.63)
Co-lead Arrangement×Log(Lead Arranger Size)			-0.118*	-0.134	-0.123			
			(-1.75)	(-1.46)	(-1.30)			
Lead Arranger Market Share						0.102	0.371	-0.368
						(0.14)	(0.48)	(-0.45)
Co-lead Arrangement×Lead Arranger Market Share						-6.272*	-15.28***	-11.47**
						(-1.73)	(-2.69)	(-1.98)
Covenant Strictness	1.183***	0.805***	1.170***	1.184***	1.264***	1.169***	1.179***	1.260***
	(27.73)	(16.30)	(28.49)	(27.77)	(30.00)	(28.44)	(27.65)	(29.89)
Log(Loan Amount)	0.177***	0.140***	0.167***	0.180***		0.167***	0.180***	
	(7.37)	(5.16)	(7.14)	(7.45)		(7.12)	(7.50)	
Log(Loan Maturity)	0.320***	0.350***	0.317***	0.319***		0.317***	0.322***	
	(12.93)	(11.59)	(13.30)	(12.86)		(13.32)	(13.02)	
Revolving Facility Dummy	-0.0545	-0.000185	-0.0347	-0.0519		-0.0386	-0.0564	
	(-1.16)	(-0.00)	(-0.76)	(-1.11)		(-0.85)	(-1.21)	
Secured Dummy	0.140***	-0.0755	0.138***	0.140***		0.138***	0.141***	
	(3.77)	(-1.53)	(3.86)	(3.76)		(3.86)	(3.80)	
Log(No. of Covenant)	0.451***	0.591***	0.503***	0.460***		0.489***	0.447***	
	(7.70)	(7.27)	(8.88)	(7.83)		(8.70)	(7.64)	
Log(No. of Co-agents)	-0.0392	0.0154	-0.0160	-0.0388	0.00791	-0.0159	-0.0383	0.00554
	(-1.07)	(0.46)	(-0.55)	(-1.06)	(0.21)	(-0.55)	(-1.04)	(0.14)
Log(No. of Lenders)	-0.0196	-0.0317	-0.0548	-0.0230	0.128***	-0.0565	-0.0303	0.116***
	(-0.46)	(-0.79)	(-1.49)	(-0.55)	(3.01)	(-1.48)	(-0.70)	(2.64)
Lender-borrower Distance	-0.00964	-0.0159	-0.00869	-0.0109	-0.00480	-0.00754	-0.0105	-0.00524
	(-0.57)	(-0.61)	(-0.54)	(-0.65)	(-0.27)	(-0.47)	(-0.62)	(-0.29)
Lending Relationship Intensity	-0.0388	0.0203	-0.0327	-0.0442	-0.0307	-0.0290	-0.0373	-0.0261
	(-1.19)	(0.56)	(-1.04)	(-1.36)	(-0.91)	(-0.92)	(-1.15)	(-0.77)
Unrated Dummy	0.00447	0.0228	-0.00403	0.00784	-0.0239	-0.00520	0.00792	-0.0226
	(0.11)	(0.28)	(-0.11)	(0.20)	(-0.58)	(-0.14)	(0.20)	(-0.55)
Altman's Z-score	-0.0165	0.141***	-0.00313	-0.00999	0.0189	-0.0112	-0.0162	0.0195
	(-0.89)	(3.57)	(-0.17)	(-0.52)	(0.97)	(-0.62)	(-0.87)	(1.00)
Log(Analyst Coverage)	-0.0202	-0.00547	-0.0240*	-0.0211a	-0.0171	-0.0227*	-0.0205	-0.0167
	(-1.56)	(-0.43)	(-1.91)	(-1.63)	(-1.29)	(-1.81)	(-1.59)	(-1.27)
Tobin's Q	-0.0270*	0.0114	-0.0350**	-0.0282*	-0.0531***	-0.0338**	-0.0263*	-0.0527***
	(-1.76)	(0.43)	(-2.29)	(-1.83)	(-3.25)	(-2.21)	(-1.71)	(-3.23)
Asset Tangibility	0.147	0.450a	0.121	0.146	0.143	0.120	0.148	0.148
	(1.33)	(1.64)	(1.13)	(1.31)	(1.25)	(1.12)	(1.34)	(1.30)
Profitability	-5.321***	-3.373***	-5.129***	-5.266***	-4.547***	-5.163***	-5.327***	-4.532***
	(-8.77)	(-3.93)	(-8.75)	(-8.71)	(-7.29)	(-8.78)	(-8.77)	(-7.26)
Firm Size	-0.0969***	0.0155	-0.0873***	-0.0963***	-0.0996***	-0.0865***	-0.0941***	-0.0973***
	(-4.88)	(0.33)	(-4.51)	(-4.86)	(-6.29)	(-4.47)	(-4.74)	(-6.08)
Financial Leverage	0.268***	0.134	0.263***	0.278***	0.490***	0.251***	0.271***	0.495***
	(2.85)	(0.90)	(2.88)	(2.95)	(5.06)	(2.77)	(2.91)	(5.13)
Lead Arranger Capital Ratio	-2.239*	-1.276	-1.553	-2.335*	-2.590**	-1.188	-1.870	-2.545**
	(-1.85)	(-0.99)	(-1.47)	(-1.90)	(-2.01)	(-1.14)	(-1.55)	(-2.00)
Log(Lead Arranger Size)	-0.0608***	-0.0664**	-1.280***	-1.212**	0.0112	-0.0532**	-0.0594***	0.0153
	(-3.05)	(-2.14)	(-2.71)	(-2.45)	(0.55)	(-2.49)	(-2.67)	(0.68)
Lead Arranger Exposure	-7.266	-21.05	-12.84	-11.75	38.27***	-12.56	-11.86	41.26***
	(-0.58)	(-1.55)	(-1.09)	(-0.89)	(2.92)	(-1.05)	(-0.92)	(3.27)
Sample Period	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019
Loan Purpose-, Industry-, State-, Year-FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Observations	6613	6145	7149	6613	6613	7149	6613	6613
Adjusted R^2	0.378	0.563	0.376	0.379	0.331	0.375	0.379	0.331

Notes: This table reports estimates from the OLS regression model and the two-stage IV regression model. The unique observation is a loan package in Dealscan. The sample period is 2000Q1-2019Q4. In Column 1, 3, 6, I regress the frequency of covenant violations on co-lead arrangement and its interaction term with two measures of lender reputation. The frequency of covenant violations is the number of violation times of all covenants in a loan package. A violation happens when the underlying variable of a covenant breaches the threshold in a given quarter-end. Co-lead arrangement is a dummy taking one if the loan package has more than one lead arranger. Log(Lead Arranger Size) is the total assets (in logarithms). Lead arranger market share is the number of loans taken by the lender divided by the number of all loans in a given quarter. In Column 2, 4, 5, 7, 8, I use a two-stage IV regression model where I replace co-lead arrangement with the fitted value in a first-stage probit regression. The IV is the number of lead arrangers that have ever lent to the borrower within five years before the loan origination. The control variables are also reported and are defined in Table B.1. Covenant Strictness is sourced from Demerjian and Owens (2016). Unrated dummy takes one if a borrower does not have an S&P credit rating in the month of loan origination. Lendin relationship intensity is the number of loans that the firm and the lead arranger(s) of a loan have ever contracted within five years before the loan origination divided by the number of total loans the firm has borrowed in that period. Lead arranger exposure is the total loan amount over the lender's assets. All lead arranger-specific variables are averaged across all lead arrangers. All borrower- and lender-specific variables are lagged by one quarter. Loan purpose-, industry-, state- and year-fixed effects are included in all regression specifications. T-statistics based on standard errors clustered by firms are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Table 3.4

Robustness Tests

	1	2	3	4	5	6	7	8	9
Dependent Variable	Violation Severity	Loan Spreads	Covenant Violations	Loan Spreads	Covenant Violations	Loan Spreads	Covenant Violations	Loan Spreads	Covenant Violations
Co-lead Arrangement	2.530**			74.74***	0.232	88.32	0.383	48.33**	0.699***
	(2.41)			(2.92)	(0.74)	(1.51)	(0.75)	(2.28)	(3.27)
Log(No. of Lead Arrangers)		110.5***	0.379						
		(3.47)	(0.78)						
Lead Arranger Market Share	-0.687		8.987*		0.389		0.0473		0.832
	(-0.33)		(1.82)		(0.48)		(0.05)		(1.03)
Co-lead Arrangement×Lead Arranger Market Share	-47.22**				-14.00**		-22.95*		-15.76***
	(-2.02)				(-2.01)		(-1.89)		(-3.42)
Log(No. of Lead Arrangers)×Lead Arranger Market Share			-11.96*						
			(-1.86)						
Lowest Future Rating		2.586**		-2.409***		-2.461***		-2.769***	
		(2.00)		(-3.34)		(-3.42)		(-3.59)	
Co-lead Arrangement×Lowest Future Rating				-4.768***		-5.510*		-1.584	
				(-3.12)		(-1.79)		(-1.38)	
Log(No. of Lead Arrangers)×Lowest Future Rating		-6.886***							
		(-4.66)							
Lending Relationship Intensity	-0.169**	-6.648**	-0.0280	-5.751**	-0.0331	-5.125*	-0.0355	-8.918***	-0.0394
	(-2.27)	(-2.28)	(-0.83)	(-2.08)	(-0.97)	(-1.87)	(-1.03)	(-3.19)	(-1.21)
Covenant Strictness	0.0537		1.170***		1.201***		1.186***	-12.38***	1.166***
	(0.52)		(28.40)		(25.95)		(25.59)	(-11.22)	(28.34)
Current Credit Rating		-12.67***		-12.72***		-13.02***			
		(-11.81)		(-11.20)		(-11.20)			
Sample Period	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019
Industry-Year FEs	No	No	No	Yes	Yes	No	No	No	No
Loan Purpose-, Industry-, State-, Year-FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. of Observations	3359	8175	7149	7913	6436	7181	5807	8238	7157
Adjusted R^2	0.127	0.655	0.375	0.658	0.392	0.646	0.379	0.654	0.375

Notes: This table reports estimates from the two-stage IV regression model. The IV is the number of lead arrangers that have ever lent to the borrower within five years before the loan origination. The unique observation is a loan package in Dealscan. The sample period is 2000Q1-2019Q4. In Column 1&2, the dependent variable is violation severity. It is measured as the relative difference between the threshold and the covenant variable in a given quarter-end conditonal on a covenant violation, averaging across all covenants and violations. In Column 2, 4, 6, and 8, the dependent variable is loan spreads, represented by the Dealscan item " all-in-drawn spreads", averaging across all facilities. In Column 3, 5, 7, and 9, the dependent variable is covenant violations, being the number of violation times of all covenants in a loan package. A violation happens when the underlying variable of a covenant breaches the threshold in a given quarter-end. Log(No. of Lead Arrangers) is the number of lead arrangers (in logarithms) in a loan package. Co-lead arrangement is a dummy taking one if the loan package has more than one lead arranger. In Column 6&7, the lead arrangers are defined using (Ivashina, 2009)'s definition, while I use (Bharath et al., 2011)'s definition in Column 8&9. Lead arranger market share is the number of loans taken by the lender divided by the number of all loans in a given quarter. Lowest future rating is a borrower's lowest S&P credit rating within five years after the loan origination date. The table reports the coefficients from the second-stage regression where I replace co-lead arrangement with the fitted value in a first-stage probit regression. The control variables are also reported and are defined in Table B.1. Lendin relationship intensity is the number of loans that the firm and the lead arranger(s) of a loan have ever contracted within five years before the loan origination divided by the number of total loans the firm has borrowed in that period. Covenant Strictness is sourced from Demerjian and Owens (2016). Current credit rating is the S&P rating in the loan origination month. All lead arranger-specific variables are averaged across all lead arrangers. All borrower- and lender-specific variables are lagged by one quarter. Loan purpose-, industry-, state- and year-fixed effects are included in all regression specifications. T-statistics based on standard errors clustered by firms are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Table 3.5

Results from the Propensity Score Matching

	1	2	3	4	5	6
Dependent Variable	Loan Spreads	Loan Spreads	Covenant Violations	Covenant Violations	Covenant Violations	Covenant Violations
Co-lead Arrangement	96.91***	77.87***	2.974***	1.947**	0.270*	0.231*
	(4.54)	(4.31)	(2.80)	(2.26)	(1.72)	(1.68)
Lowest Future Rating	-17.22***	-2.610***				
	(-35.27)	(-3.32)				
Co-lead Arrangement Lowest Future Rating	-3.523***	-3.102***				
	(-2.76)	(-2.82)				
Log(Lead Arranger Size)			-0.0363**	0.0218		
			(-2.09)	(1.22)		
Co-lead ArrangementLog(Lead Arranger Size)			-0.213***	-0.139**		
			(-2.80)	(-2.26)		
Lead Arranger Market Share					-5.550***	-2.185*
					(-4.43)	(-1.81)
Co-lead ArrangementLead Arranger Market Share					-6.356*	-5.534*
					(-1.72)	(-1.73)
CR_CapitalIQ		-20.78***				
		(-23.02)				
Covenant Strictness				1.524***		1.511***
				(34.19)		(33.98)
Unrated Dummy				-0.0248		-0.0378
				(-0.66)		(-1.03)
Sample Period	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019	2000-2019
No. of Observations	6301	5749	3836	3378	3836	3378
Adjusted R^2	0.348	0.439	0.003	0.254	0.007	0.254

Notes: This table reports estimates from the propensity score matching. The unique observation is a loan package in Dealscan. The sample period is 2000Q1-2019Q4. For each co-lead loan package, I use the method of propensity score matching to allocate the four closest single-lead arranger loans. The matching criteria include loan amount (in logarithms), the longest facility maturity (in logarithms), revolving facility dummy, secured dummy, number of covenant (in logarithms), number of co-agents (in logarithms), number of lenders (in logarithms), number of analyst coverage (in logarithms), lender-borrower distance, lending relationship intensity, Altman's Z-score, profitability, asset tangibility, firm size, financial leverage, Tobin's Q, lead arranger capital ratio, lead arranger exposure, loan purpose dummies, borrower industry dummies, and year dummies. All lead arranger-specific variables are averaged across all lead arrangers. All borrower- and lender-specific variables are lagged by one quarter. After obtaining the matched sample, I conduct OLS regressions. The dependent variable is either loan spreads, represented by the Dealscan item " all-in-drawn spreads", averaging across all facilities, or covenant violations, being the number of violation times of all covenants in a loan package. A violation happens when the underlying variable of a covenant breaches the threshold in a given quarter-end. Co-lead arrangement is a dummy taking one if the loan package has more than one lead arranger. Lowest future rating is a borrower's lowest S&P credit rating within five years after the loan origination date. Current credit rating is the S&P rating in the loan origination month. Log(Lead Arranger Size) is the total assets (in logarithms). Lead arranger market share is the number of loans taken by the lender divided by the number of all loans in a given quarter. Log(No. of Lead Arrangers) is the number of lead arrangers (in logarithms) in a loan package. Covenant Strictness is sourced from Demerjian and Owens (2016). T-statistics based on robust standard errors. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Chapter 4

International Spillovers from Changes in Capital Regulation: Evidence from the Syndicated Loan Market

4.1 Introduction

Banks play a crucial role in providing credit, which, in turn, fosters economic growth. In recent years, scholars have increasingly focused on the role of banks in open economies. For instance, by reallocating resources to their foreign subsidiaries, multinational banks can influence local credit supply in foreign markets. Beyond internal capital markets, banks also impact foreign economies by contracting directly lending to foreign borrowers. Empirical studies have validated the role of multinational banks in transmitting monetary policy and financial shocks across countries.[1] This paper

[1]For instance, Temesvary et al. (2018); Avdjiev and Takáts (2019); Morais et al. (2019); Demirgüç-Kunt et al. (2020); Takáts and Temesvary (2020, 2021); Correa et al. (2022); Aldasoro et al. (2023) explore the impact of monetary policy shocks on bank cross-border lending to other countries. Another line of research developed in Cetorelli and Goldberg (2011); Giannetti and Laeven (2012); De Haas and Van Horen (2013); Hale et al. (2020); Aldasoro et al. (2023) documents the potential for multinational banks to propagate financial shocks from their home nation to other jurisdictions through cross-border lending or internal capital markets.

investigates a relatively less-explored aspect of international transmission of policy changes – specifically, the spillover of capital regulation changes on banks' cross-border lending. While previous studies, such as Houston et al. (2012); Aiyar et al. (2014); Bremus and Fratzscher (2015); Berrospide et al. (2016); Buch and Goldberg (2016); Bussière et al. (2016); Danisewicz et al. (2017); Forbes et al. (2017); Forbes (2021); Reinhardt et al. (2023) have examined the impact of stricter capital regulation on bank lending, the emphasis has largely been on quantity variables like lending flows or lending growth. Although these metrics are pivotal in shaping overall investment in an economy, changes in interesting rates of corporate lending are equally crucial in influencing firms' decisions regarding saving and spending. This paper fills the gap by investigating loan spreads with a specific focus on syndicated loans, given their growing importance in corporate lending. In particular, we raise the following research question: how does capital regulation tightening in the lead arranger's country affect the spreads of the lender's cross-border loans? We define a cross-border loan if the lead arranger's country differs from the borrower's.

To examine the spillover effect, we employ a standard difference-in-difference (DiD) regression on loan-level data. The test is based on the implementation of stress tests in the U.S. Several reasons support this choice. First, stress test implementation can be effectively considered as a form of capital regulation tightening. Although it does not explicitly lift minimum requirements, it mandates banks to maintain sufficient capital levels under hypothetical scenarios. Second, stress tests, introduced after the Global Financial Crisis (GFC), represent probably the more influential regulatory change alongside Basel III. Additionally, while the Basel Accord covers almost all U.S. banks, stress tests apply selectively to certain banks. Thereby, it enables a comparison between the treated and controlled banks to isolate the movements of loan spreads from a pre-crisis period to a low-interest rate environment. The first stress test was introduced in the Supervisory Capital Assessment Program (SCAP) in 2009 and later evolved into a series of annual stress tests known as the Comprehensive Capital Analysis and Review (CCAR). Initially, 19 banks were part of these programs. However, since the 2014 CCAR, several banks entered and exited the programs, coinciding with the adoption of additional capital regulations such as the leverage ratio framework. To mitigate potential confounding effects from these changes, the baseline testing sample period spans from 2004 to 2014 and then extends to 2019, where similar results are observed. Thus, the treatment group encompasses loans made by banks participating in the initial stress tests. In the regression, we include a set of loan-, bank-, and borrower-level controls, as well as bank fixed effects, borrower industry fixed effects, and year fixed effects. The GFC has propagated from the U.S. to other countries. Thus, to account for fundamental differences in the destination countries, we also include borrower country fixed effects. The results reveal that, relative to loans from non-stress-test banks, the spreads of loans from the stress-test banks reduce more, by 71.1 basis points (bp), after the implementation of the first stress test. To further reduce the impact of the financial crisis, we exclude the GFC period and find similar result. Also, the reduction in loan spreads remains robust when we use borrower industry-year fixed effect to mitigate the demand-side effect and when we include GDP growth and inflation rates of the borrower country as additional controls. Then, we examine the dynamics of the spillover effect over time, showing that the difference in loan spreads between the two groups continues to decrease until the 2012 CCAR, after which it stabilises. This pattern aligns with prior literature indicating that the impact of initial stress tests is more salient and unexpected than subsequent ones. We also find a larger reduction in loan spreads to the countries where the lead arrangers are more experienced. This underscores that lending relationships are important in international banking and cross-border lending. Overall, the results suggest that the U.S. stress test implementation spillovers over to foreign countries through lower loan spreads. This finding raises the question of the origin of the observed spillover effect.

To establish the connection between loan spreads and the lead arranger-side regulatory change, we propose a novel mechanism based on a lender-level moral hazard issue in syndicated loans.[2] A syndicated loan involves multiple lenders funding a

[2]There could also be an adverse selection problem where the lead arranger overestimates the quality of a lemon borrower in exchange for private benefits. The mechanism proposed in this paper is motivated by the Ivashina (2009)'s model, framed as a moral hazard problem rather than an adverse selection issue. However, the two are the same in determining the equilibrium loan spreads.

single borrower, and there is typically one lead arranger and several participants. Crucially, the lead arranger is delegated, on behalf of the participants, to collect information from the borrower. Then, a moral hazard problem could arise if the leader does not have enough incentives to monitor. If so, the participants typically demand higher loan spreads to compensate. Now, let's consider a cross-border loan and stricter capital regulation, such as the stress test introduced in the lead arranger's country. Compliance is costly for the bank because it requires either a rapid increase in additional regulatory capital or a sharp reduction in risky-weighted assets.[3] Thereby, the lead arranger seeks to avoid the more stringent regulation, a behaviour commonly termed "regulatory arbitrage" (e.g., Houston et al., 2012; Beltratti and Paladino, 2016). And importantly, cross-border lending can help to achieve regulatory arbitrage for at least two reasons: a) banks have more discretion to report the riskiness of foreign borrowers, and b) banks can bypass the stricter regulation by lending through foreign subsidiaries. Therefore, the lead arranger will invest more in a cross-border loan after stricter capital regulations are introduced. As the bank has more stakes in the loan, the monetary incentive to monitor is higher (Carletti et al., 2007; Sufi, 2007). Then, the loan spread required by participants falls. This explains the observed spillover effect in the DiD regression. To test whether lead arrangers indeed take more shares, we replace loan spreads with lead arranger shares as the dependent variable. The data supports the argument. In particular, it shows that the lead arranger share increases by 8% after the stress test implementation in loans made by stress-test banks relative to other loans.

Next, we provide additional evidence supporting the *Regulatory Arbitrage Channel*. Since the 2014 CCAR, stress-test banks have been further classified into two classes: advanced-approach banks and other stress-test banks. The advanced approach is an internal-rating based approach allowing banks to use more of their own inputs to calculate regulatory capital ratios. This flexibility enables banks to utilize cross-border lending for regulatory arbitrage. To test the impact of the introduction of the advanced approach, we first extend the sample period to 2019. The result

[3] Alternatively, to meet higher capital requirements, the bank can increase retained earrings by widening loan spreads, but this is not observed in the empirical results.

shows a persistent treatment effect: the post-implementation reduction in spreads of loans from stress-test banks is still significant in the post-2014 period compared to non-stress-test banks[4]. Then, we add a triple interaction term by introducing a dummy that identifies the advanced-approach banks. The result reveals that before the introduction of the advanced approach, there is no difference between advanced-approach banks and the other stress-test banks. However, after 2014, the treatment effect is only observed among the advanced approach banks, suggesting that the spillover effect in the second half of the sample stems from the introduction of the advanced approach when banks have substantial discretion in reporting the riskiness of foreign lending. This is consistent with the *Regulatory Arbitrage Channel*.

Next, we expand the sample to include ten non-U.S. countries.[5] For each country, we identify quarterly events of capital regulation tightening recorded in the integrated Macroprudential Policy Database (iMaPP) of the IMF. We then supplement this information with the records of Basel accords implementations from another dataset developed by Cerutti et al. (2016). To ensure there are no double-counted events, we impose no duplicates between the two datasets. There are in total 74 events, with the first one occurring in 2007Q1 and the last one in 2019Q3.[6] Because the tightening events vary in countries and time, we employ a stacked DiD regression. Each event represents a cohort, and the event window spans eight quarters before and after the event quarter. In a given event, the treatment group comprises loans from the tightening country, while the control group consists of loans from other countries that do not tighten capital regulation during the event window. We include lender country-cohort fixed effects, borrower country-cohort fixed effects, and borrower industry-cohort fixed effects. The result shows that, relative to the loans from non-tightening countries, the spreads of loans from the tightening country reduce more by 11.39 bp after the event. The finding is robust if we also include quarter

[4]In the regression, we interact two period dummies – 2010-2014 and 2015-2019 with the treatment dummy, so the reference group is still loans made by non-stress-test banks during 2004-2009

[5]The 11 countries, including the U.S., are the only countries whose lenders have records in Compustat where we collect the financial data. These countries are Australia, Canada, France, Germany, Japan, Netherlands, Switzerland, Singapore, Spain, the U.K., and the U.S.

[6]We do not include tightening events occurring after 2019 because the sample does not contain loans originating during the COVID-19 pandemic.

fixed effects. When we exclude the U.S. events, the coefficient remains significantly negative, although the magnitude is smaller. Therefore, a spillover effect exists in non-U.S. countries. Houston et al. (2012) state that banks exploit regulatory arbitrage by engaging in cross-border activities "in countries with weaker regulations". If the borrower's country also tightens capital regulations within the event window, the feasibility of regulatory arbitrage diminishes. Consequently, the lead arranger may hesitate to increase its share in a cross-border loan, leading to no reduction in the loan spread. The empirical result supports this expectation.

Then, we explore a cross-country difference in implementing Basel II. In particular, we test whether Basel II being implemented on an unconsolidated basis or not affects the spillovers. According to the 2011 Bank Regulation and Supervision Survey from the World Bank, Basel II was introduced on an unconsolidated basis in Germany, Switzerland, the U.K., and the U.S. while on a consolidated basis in the rest of the sample countries. If an unconsolidated basis is applied, a bank can achieve regulatory arbitrage because its foreign subsidiary is exempt from the regulation. The result is consistent with this expectation. Conditional on a cross-border loan being lent through a foreign subsidiary, a post-implementation decrease in loan spreads is observed only in countries adopting the unconsolidated basis.

Finally, there are three additional findings worth noting. First, we observe that the impact of stress test implementation on cross-border loan spreads is non-existent when a loan is secured by collateral or when the borrower is rated. In both scenarios, information asymmetry between borrowers and lenders is minimal. Consequently, participants display less concern for the lead arranger's monitoring incentives, resulting in loan spreads being insensitive to capital regulation tightening. Secondly, the spillover effect diminishes as the submission of capital plans in a given year's CCAR approaches. This suggests that, when faced with escalating regulatory pressures, banks opt to reduce cross-border exposures by increasing loan spreads rather than pursuing regulatory arbitrage. Lastly, banks with higher capital adequacy are less impacted by stress tests, making them less likely to engage in regulatory arbitrage. Consistent with this argument, we show that the spillover effect is negligible when stress-tested BHCs holding possess larger capital buffers, measured as the smallest difference between the minimum capital requirements and the stressed tier-one capital, total capital, or leverage ratio.

Literature Review and Contributions: The contributions in this paper are threefold. First, it relates to the prior literature exploring the impact of capital regulation on bank lending behaviour. A majority of the papers focus on lending growth, focusing on quantities. Regarding domestic lending, the evidence indicates that implementing higher capital requirements slows lending growth in the short run (e.g., Akinci and Olmstead-Rumsey (2015); Gropp et al. (2019); De Marco et al. (2021)). This reflects banks' efforts to maintain risk-based capital ratios over a threshold. Papers testing the impact on foreign lending – capital regulation tightening/loosening spills over to lending to other countries – are more related to the current paper. However, the evidence is mixed (see Buch and Goldberg (2016); Forbes (2021) for a review of the empirical evidence). Houston et al. (2012); Bremus and Fratzscher (2015); Bussière et al. (2016) point out an outflow of bank lending from countries with stricter capital regulations, either through foreign subsidiaries or direct cross-border loans. On the contrary, papers including Aiyar et al. (2014); Berrospide et al. (2016); Danisewicz et al. (2017); Forbes et al. (2017); Reinhardt et al. (2023) find a reduction in foreign lending after a bank's country experiences capital regulation tightening. There are also several papers presenting no spillover effect. For instance, Buch and Goldberg (2016) apply a meta-analysis in 15 countries and find an insignificant impact of capital regulation tightening on lending outflow. Another cross-country study by Kang et al. (2017) also show an insignificant spillover effect of capital regulation. The present study contributes to the above-mentioned literature by exploring the impact of capital regulations on loan spreads. This study relates to another paper by Ongena et al. (2013) that studies the spillover of capital regulations on foreign lending standards. Focusing on European banks, the paper finds that more stringent capital requirements in domestic countries improve borrowers' loan access in foreign markets. However, the authors measure lending standards using qualitative survey data instead of loan spreads. There are several papers linking capital regulations with loan spreads. Acharya et al. (2018); Cortés et al. (2020); Berrospide and Edge (2024) document a positive impact of U.S. stress tests on loan spreads, while Lambertini and Mukherjee (2022) find lower loan spreads following the implementation of a U.S. stress test program. However, these papers focus exclusively on domestic loans. Similarly, Gao and Jang (2021) find a negative relationship between loan spreads and the stringency of capital regulation in a lead arranger's country, but it is not specific to cross-border lending.

This paper also contributes to the literature documenting the international transmission of financial shocks and monetary policy through banking lending activities. Research such as Cetorelli and Goldberg (2011); Giannetti and Laeven (2012); Hale et al. (2020) finds that multinational banks can transmit financial shocks from their home countries to other countries through either cross-border lending or the internal capital market. Moreover, Temesvary et al. (2018); Avdjiev and Takáts (2019); Morais et al. (2019); Takáts and Temesvary (2020) document an international bank lending channel of monetary policy transmission; that is, a tightening monetary policy reduces cross-border lending. This study adds to the existing literature by enhancing the understanding of the international transmission of capital regulations. It proposes a mechanism that is specific to the syndicated loan market and centres on the conflict between lead arrangers and participants.

Lastly, the paper contributes to previous papers studying the leader-participants conflict in the syndicated loan market. Sufi (2007) shows that lead arrangers need to retain a specific portion of the loan to ensure their commitment to monitoring. Further, participants require higher loan spreads for a given lead arranger share (Ivashina, 2009). In addition, Dass et al. (2020) find covenants are more frequently used in syndicated loans in the presence of severe conflicts. This study complements the literature by exploring the agency issue in the context of the global syndicated market.

The remainder of this paper is organised as follows. Section 4.2 discusses the potential impacts of capital regulation tightening on cross-border loan spreads and proposes two channels that lead to a reduction in loan spreads. In this section, we also introduce possibilities to differentiate the two channels. Section 4.3 elaborates on data sources, the sample, and the regression model that are used to test the spillover effect of the U.S. stress test implementation. Section 4.4 presents the empirical results. Then, we analyse the spillover effect of capital regulation tightening in a cross-country sample in Section 4.5. In this section, we also explore a country-level difference in the implementation of Basel II. Finally, Section 4.6 concludes.

4.2 Theoretical Foundations

In this section, we propose a mechanism that channels the spillover effect of capital regulation tightening. To rationalise the negative impact of the regulatory tightening on loan spreads, we borrow Ivashina (2009)'s model. This model establishes the equilibrium loan spread of a syndicated loan accounting for the presence of a moral hazard issue between the lead arranger and participants. In Ivashina (2009), the lead arranger is considered a whole-equity bank; thus, changes in capital regulations have no impact. We incorporate the lead bank's capital ratio and capital requirements and assume capital regulation tightening exogenously affects the equilibrium. The theoretical model is presented in Appendix C.2, while this section focuses on discussing the intuition. The equilibrium scenario is detailed in Section 4.2.1, and we propose how the tightening of capital regulations would alter this equilibrium in Section 4.2.2.

4.2.1 Two channels for lower loan spreads

Much like demand-side factors such as borrowers' riskiness, lender-side factors are also pivotal in shaping loan spreads (Bolton and Freixas, 2000; Murfin, 2012). However, within the context of syndicated loans involving multiple lenders, the influence of the lead arranger alone cannot exclusively determine the spread. Instead, the spread is determined through an equilibrium that takes into account participants' demand as well. Figure 4.1 depicts the equilibrium of a cross-border loan, with the horizontal axis representing the loan share retained by the lead arranger and the vertical axis indicating the loan spread. The equilibrium point is the intersection between the participants' demand curve (black) and the lead arranger's demand curve (red).

Participants' demand for loan spreads emerges from the moral hazard issue when the lead arranger exhibits reluctance in monitoring. In the syndicated loan process, banks bid for the lead arranger position by presenting potential price-quantity combinations they are willing to offer. After mandating the lead arranger, the bank engages with the borrower to negotiate preliminary loan terms and often determines the underwritten amount. Subsequently, the lead bank markets the loan to potential participants. Participants' joining decisions and funding allocation are based on the loan terms, such as the spread, and the lead arranger's commitment to the loan. Once the contract closes and the allocation is finalised, the lead arranger assumes responsibility for communicating with and monitoring the borrower; thus, it bears the entirety of the monitoring costs. However, as the lead arranger retains only a fraction of the loan, it may lack sufficient monetary incentives to monitor. Thereby, when the lead arranger share is small, the participants will demand a higher loan spread to compensate (Carletti et al., 2007; Degryse et al., 2021). This dynamic is depicted by the downward-sloping curve Figure 4.1. In the next subsection, we elaborate on how the introduction of stricter capital requirements incentivises the lead arranger to monitor, resulting in a downward shift of the black curve.

The lead arranger's curve is derived by solving its willingness to invest in a loan. If the loan spread is higher, the bank would retain more because the marginal benefit of doing so is higher. In other words, for a larger lead arranger share, the lender will demand higher loan spreads to achieve optimality. This explains the red upward-slopping curve. We propose that capital regulation tightening will enhance the marginal benefit of investing in a cross-border loan. This leads to a higher lead arranger share and shifts the red curve outwards.

Insert Figure 4.1 here.

4.2.2 Two channels for lower loan spreads

In this subsection, we discuss the potential impacts of capital regulation tightening on cross-border loan spreads. We first introduce two channels which either shift the participants' curve or the lead arranger's curve downwards, resulting in lower spreads. Then, we briefly touch upon the possibility of an opposing impact where loan spreads could increase following capital regulation tightening.

Regulatory Arbitrage Channel We begin with the shift in the lead arranger's curve. The term "regulatory arbitrage" characterises the practice of avoiding stricter regulations. In response to heightened requirements, banks can acquire additional capital, but this comes with high costs. Alternatively, banks can reduce risky assets (Gropp et al., 2019). However, this carries other costs, such as fire-selling assets at a loss or cutting back on profitable investments.[7] Thus, confronted with capital regulation tightening in the home country, a bank may seek regulatory arbitrage. Existing literature suggests that banks avoid stricter regulations by engaging in cross-border lending (Houston et al., 2012; Berrospide et al., 2016; Gao and Jang, 2021). Specifically, regulatory arbitrage can be achieved through cross-border lending in three ways:

a) *Lending through subsidiaries:* If the stricter capital regulation is implemented on an unconsolidated basis, multinational banks can bypass these regulations by conducting lending activities through their foreign subsidiaries (Danisewicz et al., 2017). As an illustration, the iMaPP database documents that, in 2019, Austria adopted a systemic risk buffer on an unconsolidated basis. Also, as mentioned earlier, the difference in implementing Basel II across countries is another example. Empirical studies provide supporting evidence for this condition. Houston et al. (2012); Karolyi and Taboada (2015); Temesvary (2018); Frame et al. (2020) demonstrate that stricter capital regulations prompt banks to establish a presence in foreign countries and reallocate credit there, particularly to jurisdictions with less stringent capital regulations. Additionally, Cerutti and Zhou (2018) outflow of bank capital, triggered by stricter macro-prudential policies, is more pronounced through foreign affiliates than through direct cross-border flows.

b) *Discretion in Reporting Riskiness of Foreign Lending:* If banks have greater

[7]Even if the capital ratios are not binding, the inclination to maintain a capital buffer above the minimum requirement can drive banks to pursue regulatory arbitrage (see Fonseca and González, 2010; Shim, 2013, among others, for discussion on capital buffers)

discretion in reporting the riskiness of foreign lending, regulatory arbitrage becomes more feasible. (Gao and Jang, 2021) argue that the information advantage of banks, when lending abroad, makes it challenging for less informed regulators to detect the under-reporting of riskiness. With risk-based capital ratios having become widely built into capital regulations, the ability to manipulate risk exposures becomes critical in realising regulatory arbitrage. Moreover, the discretion associated with foreign lending becomes more substantial when a bank uses the internal ratings approach (IBR). This is because the IBR involves more complexity and provides banks with more flexibility in using their own inputs to calculate capital ratios. For example, the foundation IRB allows banks to calculate the probability of default by themselves and the advanced IRB further allows banks to estimate loss given default and exposure at default on their own.[8]. In support of this, Plosser and Santos (2014); Begley et al. (2017) point out that banks tend to under-report risk exposures when utilizing the internal ratings-based approach, particularly when their capital ratios are closer to the threshold. Consequently, regulatory arbitrage through foreign lending becomes more attainable when the IBR is applied.

c) *Exemption of Foreign Lending from Capital Regulation:* In cases where foreign lending is exempt from the capital regulation implemented, cross-border lending becomes an ideal investment source that generates no regulatory burden. For example, recorded in the iMaPP database, the systemic risk buffer framework introduced in Bulgaria, Iceland, and Poland applies only to domestic risk exposures.

In light of the aforementioned conditions, cross-border lending emerges as a strategy to achieve regulatory arbitrage. Thus, after capital regulation tightening, lead arrangers will anticipate a higher marginal benefit and exhibit a higher willingness to invest in a cross-border loan. Empirical evidence from previous studies supports this notion. For example, Bussière et al. (2016) find capital regulation tightening in France increases cross-border lending. Using country-level data, Houston et al. (2012); Bremus and Fratzscher (2015) document that countries with more stringent capital regulations tend to experience higher capital outflows from banks.[9] Recall-

[8]See Basel II documentation: `https://www.bis.org/publ/bcbs107.pdf`

[9]These studies analyze the overall cross-border lending portfolios of banks rather than focusing

ing the moral hazard issue, as the lead arranger share becomes larger, participants require a smaller loan spread. This new equilibrium is illustrated in the left panel of Figure 4.2. Specifically, capital regulation tightening induces a larger lead arranger share for a given loan spread, shifting the red curve to the right; the curve then intersects with the downward-slopping participants' curve, changing the equilibrium from the blue point to the green one. This results in a lower loan spread.[10]

Monitoring Enhancement Channel The alternative channel predicts a shift in the participants' demand curve. A stricter capital regulation will, in general, raise the minimum standards of capital ratios. For example, Basel III, which was implemented in many countries in 2013 and 2014, lifts the minimum requirement for the common equity tier-one capital ratio from 4% to 4.5%. Other regulatory implementations, such as countercyclical buffers and global systemically important bank surcharges, also came with an increase in minimum capital ratios. With a higher capital ratio, shareholders' stakes in loan portfolios intensify. Accordingly, banks have greater incentives to monitor. This argument aligns with previous studies that recognise bank equity capital as "the device of monitoring incentives" or "skin in the game" (Sufi, 2007; Allen et al., 2011; Goodhart, 2013; Laeven, 2013). For example, in a multiple-bank lending scenario, Carletti et al. (2007) discover that higher bank equity promotes monitoring. Schwert (2018) argues that financially opaque firms rely on well-capitalised banks to mitigate information asymmetries because the banks ensure intensive monitoring. The literature also suggests that a higher capital ratio increases a bank's franchise value (Furlong and Keeley, 1989). From this view, banks with higher capital ratios have more to lose, so they will dedicate more effort to monitoring. Additionally, prudential regulators are usually required to provide more rigorous supervision under tighter capital regulations (the introduction of Pillar 2 in

on a bank's individual share in a loan. Nevertheless, both of Chu et al. (2019); Benincasa et al. (2022) observe an increased loan share in response to regulatory innovations. This suggests that regulatory changes indeed play a significant role when banks make investment decisions for a loan.

[10] There is an alternative channel to the regulatory arbitrage that also shifts the lead arranger's curve outwards. The lead bank could also increase its cross-border loan share following capital regulation tightening due to searching for yield. Specifically, as stricter capital regulations lift lending standards, banks become more incapable of generating higher returns domestically. Thus, they are more likely to seek foreign opportunities (Ongena et al., 2013; Danisewicz et al., 2017).

Basel II and qualitative assessments in U.S. stress tests are two examples). This regulatory oversight also compels lead arrangers to monitor loan performance diligently. Due to the combined impact of capital regulation tightening on monitoring, participants perceive less severe moral hazard issues. Thus, they are willing to accept lower loan spreads. This shifts the black curve downwards. In the right panel of Figure 4.2, the equilibrium changes to the green point, resulting in lower loan spreads.

In summary, either because lead arrangers are more inclined towards foreign lending or due to higher capital ratios encouraging monitoring, we expect that capital regulation tightening will lead to lower spreads in cross-border loans.

Insert Figure 4.2 here.

There are two potential ways to distinguish the two channels. The first distinction comes from the prediction of the lead arranger share. In the *Monitoring Enhancement Channel*, a higher capital ratio promotes monitoring, resulting in lower required loan spreads by participants. As the return becomes lower, the lead arranger invests less in the loan. As in the right panel of Figure 4.2, the shift in the participants' demand curve predicts a smaller lead arranger share. Conversely, the *Regulatory Arbitrage Channel* predicts a larger lead arranger share, as shown in the left panel of Figure 4.2. It is important to note that these two channels can coexist. Thus, the observed change in lead arranger shares only indicates the relative strength of each channel. For instance, if the lead arranger share increases after capital regulation tightening, the *Regulatory Arbitrage Channel* dominates.

The second distinction arises from how the loan spreads change in cross-border versus domestic loans. Regulatory arbitrage reveals a unique role of foreign lending, whereas a larger monitoring incentive induced by a higher capital ratio is uniformly applied to both domestic and foreign lending. Thereby, the *Regulatory Arbitrage Channel* predicts that lower loan spreads can only be observed in cross-border loans, while the *Monitoring Enhancement Channel* predicts the same negative impact of capital regulation tightening on loan spreads in both lending accounts. According to this criterion, the existing literature leans more to the *Regulatory Arbitrage Channel*. For example, Acharya et al. (2018) documents an increase, instead of a decrease, in domestic loan spreads after the U.S. introduced stress tests. Similarly, testing the same capital regulation change, Cortés et al. (2020) find higher loan spreads in the U.S. small business loans.[11]

Lastly, we briefly discuss the possibilities of an opposite spillover effect. The lead arranger may be less inclined to lend abroad following capital regulation tightening if regulatory arbitrage is not achievable and/or banks want to preserve domestic borrowers and/or the new regulations impose stricter requirements on cross-border loans (Danisewicz et al., 2017; Bremus and Fratzscher, 2015). Aiyar et al. (2014); Forbes et al. (2017); Reinhardt et al. (2023), find that higher bank-specific capital requirements are associated with a reduction in the growth of cross-border lending of the U.K. banks.[12] Similar results are found in Berrospide et al. (2016) for the U.S. evidence and in Danisewicz et al. (2017) for cross-country evidence. As a result of a reduction in lead arrangers' supply of foreign lending, participants would require higher loan spreads – an upward shift in the lead arranger's demand curve pushes up the loan spread. Thus, the direction of the spillover effect depends on empirical results. We present the results in Section 4.4 and Section 4.5. The next section introduces the background of stress test programs and describes the data sources as well as the sample.

4.3 Data and Methodology

Section 4.3.1 describe data sources. Section 4.3.2 introduces the stress test program and specifies a baseline model to test the impact of the stress test implementation on loan spreads. Section 4.3.3 presents the summary statistics of key variables.

[11]In an unreported test, we also find a rise in spreads of U.S. domestic loans after stress test implementation, using the regression introduced in the next section.

[12]Forbes et al. (2017) provide the rationale for why a negative relationship between capital regulation tightening and cross-border lending is largely found in the UK. They explain that an unconventional monetary policy (i.e., the Funding for Lending Scheme) introduced in 2012 makes U.K. banks more inclined to domestic than foreign lending under stricter capital requirements.

4.3.1 Data sources and sample description

The main data source used in this study is loan-level data from Thomson Reuters Loan Pricing Corporation's DealScan (DealScan). This database identifies the borrowers and lenders in each loan and records detailed loan terms in the contract. Nevertheless, it only provides limited balance sheet information. Thus, we collect borrowers' quarterly financial data from Compustat. We use the linking tables provided by Chava and Roberts (2008) to merge DealScan with Compustat datasets.[13] After the merge, the dataset contains DealScan loans for each quarter from quarter one of 1982 (1982Q1 for short) to 2016Q4. We then manually extended the merge to 2019 before the COVID-19 Pandemic. Following the literature, we drop all financial borrowers which have a SIC in between 6000 and 6999. We define lenders in syndicated loans at the bank holding company (BHC) level. Thus, we merge lenders' financial information from the U.S. BHC data from FR Y-9C with Dealscan. The existing linking source to merge is limited. We first use Chakraborty et al. (2018)'s linking table to merge the two databases up to 2014; then, we use Schwert (2018)'s linking table to merge Dealscan with Compustat and use the public source to merge Compustat with FR Y-9C during the period of 2015-2019; lastly, for the lenders that have not been merged, we manually check the legal name and incorporated state of each parent bank in Dealscan with those of the BHCs in FR Y-9C. Further, following Acharya et al. (2018), we drop all BHCs that are not commercial banks – those with RSSD9331 differing from 1.

In Dealscan, a loan package/Deal could contain several facilities/tranches. A typical package comprises two facilities, including a term loan and a revolving line of credit. Because loan spreads are determined at the facility level, the unique observation in this study is a loan facility.[14] We define a cross-border loan if the borrower's country is different from the country of the lead arranger. In the case of the U.S., a cross-border loan is made to a non-U.S. firm. Because the lead arranger is defined at the parent level, a cross-border loan in this sample could be either direct lending

[13]We thank Sudheer Chava, and Michael Roberts for making these data available on WRDS.

[14]In this study, we attribute a loan to the loan facility unless otherwise specified.

from a BHC or a loan from one of its foreign subsidiaries (e.g., JP Morgan Chase Bank Sydney lent to an Australian company Qantas Airways Ltd). We use the information from Dealscan on the country of an individual, as opposed to a parent, lender to identify whether the loan is made by a foreign subsidiary. The definition of lead arrangers follows Ivashina (2009). Specifically, for a lender to be identified as the lead arranger, the field "lenderrole" should be "admin agent". If there is no admin agent recorded in the loan, the lead arranger is identified if "lenderrole" is one of "agent", "arranger", "bookrunner", "lead arranger", "lead bank", and "lead manager". Following the common practice in literature such as (Acharya et al., 2018), if there is more than one lead arranger, we select the one that is largest in assets.[15] Thus, the unique observation of the final sample is a loan facility.

4.3.2 The stress test programs and the baseline regression

To investigate the spillover effect of capital regulation tightening on cross-border loan spreads, we focus on the U.S. implementation of the stress tests in the aftermath of the 2008 Global Financial Crisis (GFC). Several reasons support the choice of the U.S. stress tests for this study. First, U.S. lenders play a leading role in making cross-border syndicated loans, establishing the U.S. as a pivotal contributor to the spillover effect in the global market. Second, conducting analysis within one country (as opposed to a cross-country study in a later section) helps isolate the impact from other concurrent regulatory or macroeconomic policies. Lastly, stress tests serve as an effective form of capital regulation tightening as they aim to examine large BHCs' capital adequacy under worse-than-expected scenarios. Importantly, as discussed later, the design of the stress test allows both channels to function.

The Federal Reserve introduced the first stress test in 2009 (the testing period covers both 2009 and 2010), namely the Supervisory Capital Assessment Program (SCAP). Then, starting in 2011, the Fed extended the scope of the SCAP and conducted annual assessments on BHCs' capital adequacy, collectively known as the

[15] However, instead of selecting the largest ones, when we define treated loans as those that arranged by at least one stress-test BHC and use average value across all lead arrangers as controls, we found similar results.

Comprehensive Capital Analysis and Review (CCAR).[16] The CCAR comprises a qualitative part evaluating banks' capital plans and a quantitative part assessing BHCs' ability to withstand stress tests. Under the stress test, the Fed staff estimate each BHC's revenue, expenses, losses, and provisions under severe economic and financial conditions.[17] The estimations are then reflected in the changes in regulatory capital (see 2012 CCAR). The resulting regulatory capital is divided by risk-weighted assets, as estimated and submitted by BHCs in their capital plans, to calculate the stressed capital ratios that should be above the minimum requirements.

The Fed will issue an objection or conditional objection if the capital ratios fall or almost hit the minimums and then ask for adjustments and re-submissions. Therefore, when a lead arranger participates in the stress test program, the BHC is likely to lift its current capital ratio. Moreover, in the CCAR, the projected regulatory capital also takes into consideration the BHC's planned capital distribution. Thus, the BHC has an incentive to reduce dividend payouts or share repurchases, which further increases the bank's stakes in future loan performance. In both cases, according to the *Monitoring Enhancement Channel*, the lead will put more effort into monitoring, and loan spreads will fall. The *Regulatory Arbitrage Channel* is also applicable under the implementation of stress test. To estimate credit losses, BHCs should assign an internal credit rating to each large C&I loan and map it onto one of the risk categories set by the Fed. This provides banks with an opportunity to under-report riskiness, allowing them to circumvent the stringent tests by engaging in foreign loans. Additionally, commencing in 2014, specific stress-testing banks are required to employ the advanced IRB approach for calculating capital ratios. As discussed in the preceding section, this presents an additional condition conducive to cross-border lending for the purpose of regulatory arbitrage.

In order to test the impact of stress test implementation on cross-border loan spreads, we run a standard difference-in-difference (DiD) regression. Specifically, we

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[16] In 2013, the Fed initiated another program, namely Dodd-Frank Act Stress Tests (DFAST). Because DFAST includes the same BHCs but is introduced later, the discussions and tests focus on the SCAP and the CCAR.

[17] See `https://www.federalreserve.gov/newsevents/pressreleases/files/bcreg20120313a1.pdf`

compare the spreads of loans by the BHCs included in the programs with those by other banks before and after the first implementation of the stress test. We follow Acharya et al. (2018) to design the baseline regression. Specifically, the sample period is 2004-2014. In a robustness check, we drop the GFC period to remove the impact of the crisis on loan spreads. During the period, 19 BHCs participated in both the SCAP and the CCAR, with MetLife Inc. dropping after 2012 (see Appendix C.1 for details). Because not all BHCs made foreign lending, only 14 BHCs are included in the sample comprising the treatment group, and the control group contains 13 BHCs (note that a majority of the loans are made by the treated BHCs). The participating BHCs expanded to 30, 31, 33, 34, and 35 from 2014 to 2018. Because the merged Dealscan data covers loans originating up to 2019, we also expanded the testing sample to 2019. However, this only increases the treatment group by 3 BHCs. To avoid that a longer sample period may contain other regulatory changes that confound the results, our primary testing period is 2004-2014.[18] The baseline DiD regression is specified as follows.

$$\begin{aligned}\text{Loan Spreads}_l = {} & \theta_1 \text{Treatment}_b \times \text{Post}_t + \sum_{i=1}^{6} \beta_i \text{Loan-level Controls}_l \\ & + \sum_{i=7}^{10} \beta_i \text{Lender-level Controls}_{bt-1} + \sum_{i=11}^{15} \beta_i \text{Borrower-specific Controls}_{ft-1} \\ & + d_l + d_b + d_f + d_t + u_{lbft} \end{aligned} \tag{1}$$

Equation 1 is a standard DiD regression with the subscripts referring to loan (l), lead arranger (b), borrower (f), and year (t). The dependent variable is the all-in-drawn spreads of a cross-border loan$_l$. The main testing variables are two dummies. Treatment takes one if lead arranger$_b$ of loan$_l$ participated in the 2009 SCAP and the following CCAR. Post$_t$ takes one if the loan originates during 2010-2014 – the period after the first stress test was implemented. The coefficient of the interaction between the two variables is θ_1, and it measures the treatment effect. Both the *Monitoring*

[18] We report the distribution, by borrower countries, of the (treated) loan sample in Table C.1.4. Dropping all missing values, there are in total 49 borrower countries. The treated BHCs lend the most to Canada, followed by the U.K. and Germany.

Enhancement Channel and the *Regulatory Arbitrage Channel* predict a negative θ_1.

Equation 1 includes non-pricing loan terms as controls.[19] They are loan maturity, loan amount (We convert it to a common currency using the exchange rate on the date of loan origination), a dummy indicating a loan is secured by collaterals, number of lenders, number of covenants, and relationship intensity[20].

The second set of variables includes the lead arranger's characteristics that control for credit supply. We include the lender's size measured by total book assets, capital adequacy measured by book capital ratio, profitability measured by return on assets, and a ratio of liquidity assets to total assets. Then, we also include borrower-level characteristics which control borrowers' demand for bank loans and firm-specific risks. The variables are size, asset tangibility, cash and equivalents, profitability, and interest coverage ratio. All lender- and borrower-specific variables are lagged for one quarter to alleviate simultaneous bias, and the financial ratios are winsorised within the range of 1% and 99%.

Lastly, we include several fixed effects in Equation 1. To capture the fundamental differences in various types of loans, we include loan type fixed effects and loan purpose fixed effects. We also include lead arranger fixed effects and year fixed effect, so the coefficients of Treatment_b and Post_t are omitted.[21] Borrower country fixed effects and borrower industry (defined using tow-digit SIC codes) fixed effects control for time-invariant institutional differences and country risks that affect loan pricing.

4.3.3 Summary statistics

Table 4.1 presents summary statistics of the variables utilized in this study. The mean value of loan spreads is 145.34 basis points (bp), slightly smaller than those reported for domestic loans in previous papers. This difference is expected, as firms borrowing

[19]Considering that loan contractual terms are jointly determined, we check the robustness by excluding these terms from the regression in Table C1, and the results are largely unchanged.

[20]We follow Bharath et al. (2007); Schenone (2010) to measure the relationship intensity. We first count the number of loans that the firm and the lead arranger of a loan have ever contracted within five years before the loan origination. We then divide it by the number of total loans the firm has been borrowed for in the past five years.

[21]The results remain unchanged when we include quarter fixed effects; see Appendix C1.

in the international market tend to be more transparent and larger. As in the table, the average firm assets and asset tangibility are both larger than those reported in Acharya et al. (2018); Lambertini and Mukherjee (2022). The lead arranger, on average, retains 12% of a loan. The differences in loan spreads and lead arranger shares between this paper and Ivashina (2009) highlight variations in equilibriums between the cross-border and domestic syndicated loan markets. An average loan involves about 12 participants with most of them being non-U.S. lenders. A majority of loans are made by stress-test banks, which is common in prior studies using syndicated loan-level data. Moreover, one-third of loans have collateral, while the use of covenants is not prevalent in cross-border lending. The banks in the sample are large in assets and have an average book capital ratio of 9% and an average liquidity asset ratio of 20%.[22]

Insert Table 4.1 here.

4.4 Empirical Results

This section presents the empirical results using the U.S. sample and the implementation of stress tests.

4.4.1 The impact of stress test implementation on cross-border loan spreads and the lead arranger share

Table 4.2 presents the results of estimating different variants of Equation 1. The table reports the coefficients of all variables. We include the loan- and lender-level controls in the first two columns, respectively. The coefficients of the variable of interest are both negative and significant at the 1% level.[23] When we include the

[22] Note that the average firm profitability is large and the variable is quite right-skewed. All of the results are the same if we drop the extremely large observations of Firm ROA or take the logarithm of the variable.

[23] In the main results, we use borrowers' incorporated countries reported to Compustat to define cross-border loans. Thus, the borrowers should have records in Compustat, which is for public firms. The sample size becomes much larger if we use the country codes reported in Dealscan to define, and the results, presented in Appendix C.3, are largely unchanged.

borrower-specific controls in Column 3, the sample size drops because the financial data in Compustat are limited for borrowers in certain countries. The coefficient in magnitude is smaller but remains statistically significant at the 10% level. Thus, the finding of a negative coefficient implies the presence of spillover and is consistent with a reduction in the equilibrium loan spread predicted in Figure 4.2. In particular, relative to other loans, the spread of cross-border loans led by stress-test BHCs reduce by 71.1 bp after the first stress test is implemented. Notably, the magnitude of this treatment effect is larger than in the previous literature that examines the impact of stress test implementation on U.S. domestic loans. For example, using a comparable DiD approach, Acharya et al. (2018) find a 47.8 bp increase in domestic loans, and Lambertini and Mukherjee (2022) document an effect of 25-32 bp. The stronger treatment effect presented in Table 4.2 underscores the substantial role played by the lender-side factors (stress test implementation in the case of the current paper) in shaping the spreads of cross-border loans relative to domestic loans. In contrast, borrower-side factors such as creditworthiness are more important in pricing domestic loans. In the next column, we exclude the GFC period (2007Q4-2009Q2), aiming to eliminate potential confounding effects arising from banks tightening lending standards and altering loan rates due to conservative considerations during this crisis period. Although the sample size drops by almost 10%, the coefficient of interest remains unchanged. Then, building upon the third specification, we employ stricter FEs in the next two regressions. Because the stress test implementation is imposed at the BHC-level, the loans made by each BHC could not be independent. Therefore, in the next two columns, we double cluster standard errors at the lender and borrower levels and the lender and year levels. The coefficients of the interaction term are still significantly negative. To further shut down the demand-side effects, we include industry-year FEs. This removes any time-varying credit demand at the industry level.[24]. The treatment effect remain largely unchanged. Lastly, to further capture the differences in borrowing countries, we control GDP growth and inflation. Again, the coefficient of Treatment × Post remain significantly negative. However,

[24]We cannot include firm by time FEs as in Khwaja and Mian (2008). This is due to the lack of observations where a firm borrows from both treated and control banks within the same year

the coefficients of two country-level controls are insignificant, possibly suggesting that the effects are already captured by the borrower country FEs.

Turning to the control variables, the signs of most coefficients align with the expectation. A loan with a larger amount and larger syndicate size tends to be cheaper, attributed to economies of scale and the benefit of diversification. The absence of an impact from the number of covenants on loan spreads could due to the fact that covenants are finalised at the end of the syndicate process after loan spreads have been set (Dass et al., 2020). Lending relationship intensity is associated with lower loan spreads, consistent with theoretical predictions and empirical findings in Boot (2000); Bharath et al. (2007); Schenone (2010); Bharath et al. (2011). Larger banks are more reputable and, thus, will dedicate more effort to monitoring. Thus, participants require lower spreads in loans led by larger lead arrangers. Loans from well-capitalised banks are more expensive because the banks pass the expenses of holding higher capital ratios onto borrowers Bolton and Freixas (2000); Schwert (2018). Next, borrowers that are larger, more profitable, and possess higher interest coverage ratios tend to incur lower spreads, reflecting their lower perceived risk. Lastly, the propensity of borrowers to hold substantial cash reserves signals an expectation of future cash shortages. This makes lenders demand higher loan spreads to compensate for potential delinquencies.

Insert Table 4.2 here.

Next, we plot dynamics before and after the stress test implementation in Figure 4.3. To perform, we create a dummy for each year from 2005 to 2014. Then, we regress loan spreads on the interactions between the treatment dummy and each of the year dummies and plot the corresponding coefficients. Note that, since the first year of the sample is removed because of colinearity, the plot describes changes in the spread difference between the treatment group and the control group over time. In the figure, the difference changed merely in the first three years, followed by small and insignificant increases after the GFC hit. Thus, the parallel trend assumption is satisfied. Then, immediately in the year after the first stress test was implemented, loan spreads in stress-test bank-led loans witness a dramatic drop relative to those led by non-stress-test banks. The drop persists for about three years before stabilising, making the difference between the two groups significantly lower than that at the beginning of the sample. The continuous reduction over the initial three years may be attributed to the time required for lead arrangers or participants to fully digest the impact of stress tests and for the equilibrium to gradually reflect the negotiations between the involved parties. On the contrary, the stabilisation at the end of the sample could be because the annual stress tests are more predictable and less unexpected. It is noteworthy that, in Acharya et al. (2018), the impact of stress tests on loan spreads lasts only for two years. The more prolonged effect is anticipated, given the adjustment in loan pricing in international lending is expected to be more complex than in domestic lending

Insert Figure 4.3 here.

Overall, the evidence suggests that capital regulation tightening spills over to lower cross-border loan spreads, in line with the prediction of both channels proposed in Section 4.2. In order to distinguish the relative importance of the two channels, we replace the dependent variable in Equation 1 with Lead Arranger Share. The results are presented in Table 4.3. Despite limitations in the sample size due to the missing observations on lender shares in Dealscan, the coefficients of Treatment $\times$ Post are statistically significant in all specifications. As it shows, after stress test implementation, the lead arranger tends to take an additional 8.03% share of a cross-border loan. This finding is consistent with an outward shift of the lead arranger's curve, so it supports the *Regulatory Arbitrage Channel*. Alternatively, to take part from the existing participants, the lead arranger could secure a greater loan share by inviting fewer participants. Thus, in the next three columns, we replace Lead Arranger Share with the number of participants (i.e., No. Participants) and use ordered probit estimates. The coefficient has a negative sign, as expected, but it is not statistically significant in the full-control specification. Thereby, although the lead arranger wants to invest more into a cross-border loan following stress test implementation, it achieves this by directly asking for more shares instead of reducing the syndicate size. Regarding controls, other loan terms have a more substantial influence on the syndicate size, compared to the lead arranger share. Also, when a larger lead arranger lends to a larger borrower, it tends to invite more participants.

Insert Table 4.3 here.

4.4.2 Testing the Regulatory Arbitrage Channel by exploring the advanced approach

In this subsection, we extend the sample period to 2019, which serves two purposes: to test whether there is a similar spillover effect when new BHCs joined in the CCAR in 2014, and more crucially, to provide additional supporting evidence for the *Regulatory Arbitrage Channel* by examining the introduction of the advanced approach. Accordingly, we create two dummies – Later-included BHC takes one if the lead arranger of a loan is included in the 2014 CCAR and Advanced-approach BHC takes one if the lead arranger of a loan is classified as one of the advanced approach BHCs in the 2014 CCAR[25]. During 2004-2019, there were 27 BHCs in the sample, with 14 of them being BHCs that initially joined CCAR, and 3 of them joining in 2014. Among the 14 BHCs, 9 BHCs are classified as advanced-approach banks.[26] Table 4.4 presents the results estimated in the extended sample. Because the BHCs joined the program after 2014, we include an interaction term between the Later-included BHC dummy and a Post-2014 dummy. The interaction term has negative coefficients in all of the first three columns; however, they are not statistically significant at the 10% level. This suggests that no spillover effect occurs at the later-included BHCs. This finding is reasonable for two main reasons. First, the later inclusion of these BHCs is not as surprising as the initial implementation of stress tests for the initially included BHCs. Second, although these new BHCs are only included in the quantitative tests at a later stage, they have already been considered in earlier stress tests for qualitative assessments. Consequently, the later-included BHCs react less to

[25]Although there are a few BHCs are excluded from or included in different years' CCAR, the BHCs that are defined as later-included BHCs or advanced-approach BHCs are unchanged after 2014

[26]None of the three later-included BHCs is classified as advanced-approach banks, so we drop them from the sample in later tests.

stress tests, making the spillover effect less pronounced. In contrast, the coefficient of Treatment $\times$ Post remains significantly negative, indicating that the impact of the stress test implementation on loans made by initially-joined BHCs persistent. The coefficient is still significant when we drop the later-included BHCs from the sample in Column 4.

Next, we focus on the initially-joined BHCs to test the impact of the advanced approach. We first split the Post dummy into two – Period 2010-2014 (which is effectively the *Post* dummy in Table 4.2) and Post-2014. In Column 5, the interaction between Treatment and either Period 2010-2014 or Post-2014 has significantly negative coefficient. This result suggests that the reduction in loan spreads persists into the second half of the sample period following the initial implementation of stress tests. Does this persistence indicate a permanent reduction in loan spreads, or is it attributed to the introduction of the advanced approach in 2014?

The advanced approach allows banks to use more of their own inputs to calculate regulatory capital ratios. In pursuit of regulatory arbitrage, advanced-approach BHCs will demonstrate a greater willingness to lend to foreign borrowers than other stress-test banks. Thus, if the *Regulatory Arbitrage Channel* is the driving force behind the observed spillover effect, we would anticipate a more pronounced decline in spreads for loans originating from the advanced-approach BHCs. We test the conjecture in the last column. Specifically, we add two triple interaction terms – Treatment $\times$ Advanced-approach BHC $\times$ Period 2010-2014 and Treatment $\times$ Advanced-approach BHC $\times$ Post-2014. Now, the double interactions represent the group of banks that are not classified as advanced-approach BHCs that are included in stress tests, and the triple interaction terms measure any marginal impact from the advanced-approach BHCs (the treatment group is still the non-stress-test banks). It shows that the coefficient of Treatment $\times$ Advanced-approach BHC $\times$ Period 2010-2014 is insignificant. It means that, before the introduction of the advanced approach, there was no difference between advanced-approach BHCs and other stress-test BHCs. Then, a divergence emerges after 2014 with the coefficient of Treatment $\times$ Advanced-approach BHC $\times$ Post-2014 being significantly negative. This means that, following the introduction of the advanced approach, the spreads of loans led by the affected BHCs decrease relative to those led by other stress-test BHCs. Also, the coefficient of Treatment × Post-2014 is not significant, indicating that the treatment effect after 2014 is entirely attributable to the introduction of the advanced approach. The findings robustly support the *Regulatory Arbitrage Channel*.

Insert Table 4.4 here.

4.4.3 Cross-sectional heterogeneity of the spillover effect

The last set of results explores the cross-sectional heterogeneity of the treatment effect in Equation 1 by adding a triple interaction between Treatment × Post and a loan- or borrower-level variable; that is, to test how the spillover effect varies in different loans and borrowers. The regression is specified as follows.

$$
\begin{aligned}
\text{Loan Spreads}_l = {} & \beta_1 \text{X} + \beta_2 \text{X} \times \text{Treatment}_b + \beta_3 \text{X} \times \text{Post}_t \\
& + \theta_1 \text{Treatment}_b \times \text{Post}_t + \theta_2 \text{X} \times \text{Treatment}_b \times \text{Post}_t \\
& + \sum_{i=4}^{9} \beta_i \text{Loan-level Controls}_l + \sum_{i=10}^{13} \beta_i \text{Lender-level Controls}_{bt} \\
& + \sum_{i=14}^{18} \beta_i \text{Borrower-specific Controls}_{ft} + d_l + d_b + d_f + d_t + u_{lbft} \qquad (2)
\end{aligned}
$$

In the equation, the sample period is 2004-2014 and X represents one of the loan- and borrower-level variables. The results are reported in Table 4.5, and it only shows the coefficients of interest – θ_1 and θ_2. In the first column, we interact Borrower Country Experiences. It measures a lead arranger's experiences of lending to a borrower country. Specifically, we divide the number of loans that the lead arranger of a loan has ever lent to the country in the past five years by the total loans in the country.[27] The mean and median of the variable are 0.17 and 0.14, respectively. De Haas and Van Horen (2013) find that, when a crisis hits, banks are more likely to maintain cross-border lending to the countries with which they have had a closer

[27] The result is similar if we calculate Borrower Country Experiences using the number of loans that the lead arranger lent to the country divided by the total cross-border loans of the lead arranger.

historical connection. Hence, we expect a more pronounced spillover effect when the lead arranger has greater experience, and this expectation is supported by the significant and negative coefficient of the triple interaction term. Thus, following the stress test implementation, banks reduce loan spreads more substantially to their relationship countries. We then report the F statistic of testing the marginal effect of Treatment $\times$ Post at the 75th percentile value of lenders' experiences – $\theta_1 + \theta_2 \times p75(\text{Borrower Country Experiences}) = 0$. The test shows that the effect is statistical significant.

In the following specifications, we interact Treatment $\times$ Post with two measures of the intensity of information asymmetry between lenders and borrowers. Monitoring becomes more smooth and less costly when information asymmetry is less severe. Then, participants are less concerned about an increase in the lead arranger's desire to invest in a cross-border loan induced by capital regulation tightening, and thus, loan spreads would remain unaffected. The two measures are Secured Loan and Unrated Borrower, given that the collateral inclusion makes the borrower more visible (Sufi, 2007) and rated firms are more transparent (Faulkender and Petersen, 2006). Indeed, the results in Column 2 and Column 3 show that the spillover effect does not exist when a loan is secured by collateral (indicated by the F test on $\theta_1 + \theta_2 = 0$) or lending to a rated borrower (indicated by the insignificant coefficient of Treatment $\times$ Post).

Next, in Column 4, we explore the situation when a loan is made through the lead arranger's foreign subsidiary, which sets up an alternative condition to achieve regulatory arbitrage. Foreign Subsidiary Dummy takes one if the individual lender recorded in Dealscan is a foreign subsidiary of the U.S. lead BHC. The coefficient of the triple interaction term is significantly negative, while the other coefficient is positive. This suggests that the existence of the spillover effect is contingent upon the satisfaction of conditions to gain regulatory arbitrage, supported by the F test on $\theta_1 + \theta_2 = 0$.

Although regulatory arbitrage can mitigate the impact of stricter capital regulation, a more direct and quicker way to comply with higher requirements is to reduce lending. Therefore, we expect that when the submission date of the capital plan for a given year's CCAR approaches, BHCs will curtail overall loan exposures and tighten lending standards. This leads to an inward shift of the lead arranger's curve and neutralises the spillover effect. Following Rapp and Waibel (2023), we define Pre-Stress Test Quarter Dummy if a loan originates within one quarter before the submission date of the next CCAR. In Column 5, the F statistic of 1.89 confirms the argument that, facing regulatory pressure, the lead arrangers forego the chance of regulatory arbitrage; instead, they load off cross-border exposures; then, in equilibrium, the loan spreads go up.

Further, Cortés et al. (2020) find that banks being more exposed to stress tests are more keen to adjust the overall riskiness of their lending portfolios. Therefore, in the last column, we interact the stress test exposure of each BHC.[28]. Specifically, Minimum Capital Distance is the minimum of the differences between the tier-one capital ratio, total capital ratio, and leverage ratio in the CCAR in a given year and the minimum requirements for each capital ratio. It measures a BHC's capacity to withstand recessions. Banks with higher Minimum Capital Distance are less affected by the stress tests and thus are less inclined to engage in regulatory arbitrage. Supporting this argument, Column 6 shows a significantly positive coefficient for the triple interaction term. According to the F test, we find that when banks' capital buffer is sufficiently adequate (at the 95th percentile value of $MinimumCapitalDistance$), the spillover effect disappears.

Insert Table 4.5 here.

4.5 A cross-country analysis

In this section, we expand the sample to other countries and conduct a stacked DiD regression, exploring quarterly events of capital regulation tightening in 11 countries from 2007 to 2019. This cross-country analysis has two advantages. First, it helps to test the existence of the spillover effect in non-U.S. countries. Second, we can

[28]Note that, because the stressed capital ratios are only observed for BHCs during stress testing years, we do not include the individual variables in the interaction term

explore differences in capital regulations to test the *Regulatory Arbitrage Channel*. Specifically, in Section 4.5.2 we test whether implementing new capital regulations on a consolidated basis or an unconsolidated basis affects the spillover effect. In the cross-country tests, we collect lead arrangers' financial data from Compustat.

To identify the events of capital regulation tightening, we use the integrated Macroprudential Policy Database (iMaPP) constructed by Alam et al. (2019). iMaPP records the tightening and loosening actions of various macroprudential policy tools for 134 countries from 1990 to 2016. The main observations in the database are dummy-type indices. iMaPP collects data from different sources, including IMF's Annual Macroprudential Policy Survey, the Bank for International Settlements (BIS), the Financial Stability Board (FSB), and the jurisdictions' authorities. We select the records of regulatory tightening under six categories: a countercyclical capital buffer ("CCB_T"), a capital conservation buffer ("Conservation_T"), capital requirements targeting corporate sector ("Capital_Corp_T"), broad-based capital requirements ("Capital_Gen_T"), capital requirements targeting FX-loans ("Capital_FX_T"), and non-risk-based leverage requirements ("LVR_T"). We then supplement iMaPP with the database created by Cerutti et al. (2016), which records the implementation quarters of Basel Accords.[29] After merging the two databases, we drop all events that are duplicated in each country in each quarter. In total, there are 74 tightening events in 11 countries during 2007-2019 (the description of the tightening events is included in Appendix C.1). The 11 countries are Australia, Canada, France, Germany, Japan, Netherlands, Singapore, Spain, Switzerland, the UK, and the USA. Section 4.5.1 specifies the testing methodology and presents the results.

4.5.1 The spillover effect in the cross-country sample

To test the cross-country spillover effect, we use a stacked DiD method. The DiD compares spreads of the cross-border loans originating from a country that tightens the capital regulation (tightening source country) with those from countries that do

[29]In Cerutti et al. (2016)'s data, the implementation Basel II does not count as regulatory tightening, while the implementation of Basel II.5 does.

not (non-tightening source country). We first construct a sample of all cross-border loans from the 11 countries, merging it with 74 tightening events. In the setting, each event serves as a cohort. In terms of the event window, a too-short window will lack observations, while an excessively long one may include other regulatory changes concurrent with the capital regulation tightening. Thus, we use an eight-quarter window [-8,8] and drop the quarter when the tightening event occurs.[30] For each event, we include all loans from both tightening and non-tightening source countries originating within an event window. We drop the destination countries that borrow only from the non-tightening source countries. For every event, the treatment group include the loans led by lead arrangers from the tightening source country. Correspondingly, the loans arranged by lenders from other source countries comprise the control group. Note that some countries in the control group may undergo regulatory tightening during the event window.[31] Thus, for each country in the original control group that tightens capital regulation, we remove all loans originating before (after) the tightening date if the tightening happens before (after) the event date. This ensures comparability and, at the same time, maintains the number of observations.[32]

$$
\begin{aligned}
\text{Loan Spreads}_l = \beta_1 \text{Treatment}_b^c + \beta_2 \text{Post}_t^c + \theta_1 \text{Treatment}_b^c \times \text{Post}_t^c \\
+ \sum_{i=3}^{8} \beta_i \text{Loan-level Controls}_l + \sum_{i=9}^{12} \beta_i \text{Lender-level Controls}_{bt-1} \\
+ \sum_{i=13}^{17} \beta_i \text{Borrower-specific Controls}_{ft-1} + d_l + d_b + d_f + u_{lbft} \qquad (3)
\end{aligned}
$$

The dependent variables and control variables are the same as in Equation 1. Treatment^c takes one if a loan is arranged by an event country's lead arranger. As in

[30] We find similar results when using a four-quarter window.

[31] For example, consider the event in Switzerland, the event date is the 28^{th} of February 2013, and the window is from the 28^{th} of February 2013 to the 28^{th} of February 2014. Imagine that there are two loans made by Swiss lead arrangers before and after the event date. Also, there are two loans made by U.S. lead arrangers that originated on 1^{st} of October 2012 and 1^{st} of February 2014, respectively. According to iMaPP, one capital regulation tightening happened in the U.S. on 2014-01-01. In this situation, when we calculate the DiD, we actually compare the loan spread from two tightening countries before and after the event.

[32] We report the distribution, by borrower countries, of the (treated) loan sample in Table C.1.4. Dropping all missing values, there are in total 53 borrower countries. The treated lead arrangers lend the most to the U.S., followed by Canada and the U.K.

Table 4.1, on average, there are about 31% loans lending from the tightening country. Post_t^c takes one if a loan originates after the event quarter, and the mean value is 0.44. We include loan type FEs, loan purpose FEs, lender country FEs, borrower country FEs, and borrower industry FEs. The coefficient of interest is θ_1, and we expect a negative sign.

Table 4.6 presents the results. The coefficient of $\text{Treatment}^c \times \text{Post}^c$ is significantly negative in each of the first three specifications. However, its magnitude is notably smaller than observed in the U.S. stress test implementation test. This discrepancy may be attributed to the spillover effect being less pronounced in non-U.S. countries, and this can be seen from a smaller magnitude when we exclude the tightening events in the U.S. later. Additionally, the smaller effect might stem from the perceived cleanliness and exogeneity of stress test implementation compared to the broader and potentially less targeted capital regulation tightening in this cross-country analysis. In the next and following columns, we use stricter FEs by incorporating cohorts into all FEs to source the variations within events. For example, we replace borrower country FEs with borrower country-cohort FEs. The result in the fourth column shows that, following capital regulation tightening, spreads of loans from the tightening country reduce by 13.16 bp, compared to the loans from non-tightening countries. Next, we exclude from the sample the six events initiated in the U.S. in Column 5. Although the coefficient is smaller, it is still significant. This suggests that the spillover effect also exists in non-U.S. countries. Then, in Column 6, we include quarter-cohort FEs, and the coefficient remains significantly negative.

The cross-country analysis is subject to confounding effects from policy changes at the country level. In the next column, we test whether tightening of other macro-prudential policies influences the strength of the spillover effect of capital regulation tightening. Specifically, we interact $\text{Treatment}^c \times \text{Post}^c$ with a dummy variable indicating whether the treated country (i.e., tightening source country) tightens other macro-prudential policies in the same quarter as the capital regulation tightening. It shows that the coefficient of triple interaction term is statistically insignificant, suggesting that the spillover effect is independent of other regulatory changes. In the last column, we test the dynamics of capital regulation tightening. We split the sample into four-quarter periods and omit the second year before the event (i.e., [-8,-6]) in the regression. Post^{c-1}, Post^{c1}, Post^{c2} respectively represent the first year before, the first year after, and the second after the event. According to the result, the difference in spreads between the treatment group and the control group remains unchanged before the event, and then it consistently decreases in the subsequent two years with a larger magnitude in the second year. Lastly, Notably, the definition of tightening events excludes capital regulation targeting the household sector, given that syndicated loans are corporate loans. However, in Table C1, we conduct a placebo test that instead uses only the tightening events specific to the household sector. As expected, the coefficient of interest is insignificant.

Insert Table 4.6 here.

4.5.2 Further tests on the Regulatory Arbitrage Channel by exploring lending through subsidiaries

As previously mentioned, a bank can avoid stricter capital requirements implemented in its home country by lending through its foreign subsidiary, especially a local subsidiary in the borrower's country. However, if capital regulation is also tightened in the borrower country, regulatory arbitrage becomes unattainable. To test this, we create a dummy, Borrower Country Tightening, taking one if capital regulation tightening occurs in the borrower country during the event window. We then interact the dummy with$\text{Treatment}^c \times \text{Post}^c$ in Equation 3. The result is reported in the first column of Table 4.7. The coefficient of the triple interaction term is positive. This result is consistent with the expectation: if stricter capital regulation is also introduced in the borrower's country, a reduction in cross-border loan spreads will not be observed because the lead arranger is unable to pursue regulatory arbitrage.

To further test the spillover effect under the *Regulatory Arbitrage Channel*, we explore the cross-country variation in implementing Basel II. According to the 2011 Bank Regulation and Supervision Survey published by the World Bank, at the time when the sample countries adopted Basel II but not Basel III, capital requirements were applied on an unconsolidated basis in four countries (Germany, Switzerland, the U.K., and the U.S.) while on a consolidated basis in the rest.[33] If capital regulation is implemented on an unconsolidated basis, a bank can achieve regulatory arbitrage because its foreign subsidiary is exempt from the regulation. Conversely, if stricter capital requirements are introduced on a consolidated basis, a lead arranger has no incentive to invest in cross-border loans, leading to no shift in the lead arranger curve and no reduction in loan spreads after the tightening. Thus, we test this argument. Specifically, we select all loans from the 11 countries originating before and after eight quarters of each country's implementation of Basel II (the description of the Basel II implementation is included in Appendix A). As there is only a single event, a standard DiD regression is employed for analysis, as follows.

$$\begin{aligned}\text{Loan Spreads}_l = {} & \beta_1 \text{Unconsolidated Basis Dummy}_b + \beta_2 \text{Post}_t^c + \theta_1 \text{Treatment}_b^c \times \text{Post}_t^c \\ & + \sum_{i=3}^{8} \beta_i \text{Loan-level Controls}_l + \sum_{i=9}^{12} \beta_i \text{Lender-level Controls}_{bt} \\ & + \sum_{i=13}^{17} \beta_i \text{Borrower-specific Controls}_{ft} + d_l + d_b + d_f + u_{lbft} \end{aligned} \tag{4}$$

In Equation 4, Unconsolidated Basis Dummy takes one if a loan is lent from one of Germany, Switzerland, the U.K., and the U.S., with a mean of 0.5. The other variables and FEs are the same as in Equation 3. The coefficient of the interaction estimates the impact of the adoption of the unconsolidated basis as opposed to the consolidated basis on the changes in loan spreads before and after the implementation of Basel II. We expect θ_1 to be negative as supporting evidence of the *Regulatory Arbitrage Channel*.

In Column 2 of Table 4.7, the interaction term has an insignificant coefficient.[34] However, when we further interact the Foreign Subsidiary Dummy, while the coefficient of

[33] The 2011 Survey does not contain Japan and Singapore, so we find the relevant information from the banking regulators' websites in the two countries

[34] One reason for the insignificance could be that Basel II is not treated as capital regulation tightening, as in Cerutti et al. (2016).

Unconsolidated Basis Dummy × Post^c is still insignificant, the coefficient of Unconsolidated Basis Dummy × Post^c × Foreign Subsidiary Dummy is significantly negative. This suggests that the choice between unconsolidated and consolidated basis is relevant only when the cross-border loan is lent through foreign subsidiaries. Under this situation, the application of the unconsolidated basis channels the spillover effect.

Insert Table 4.7 here.

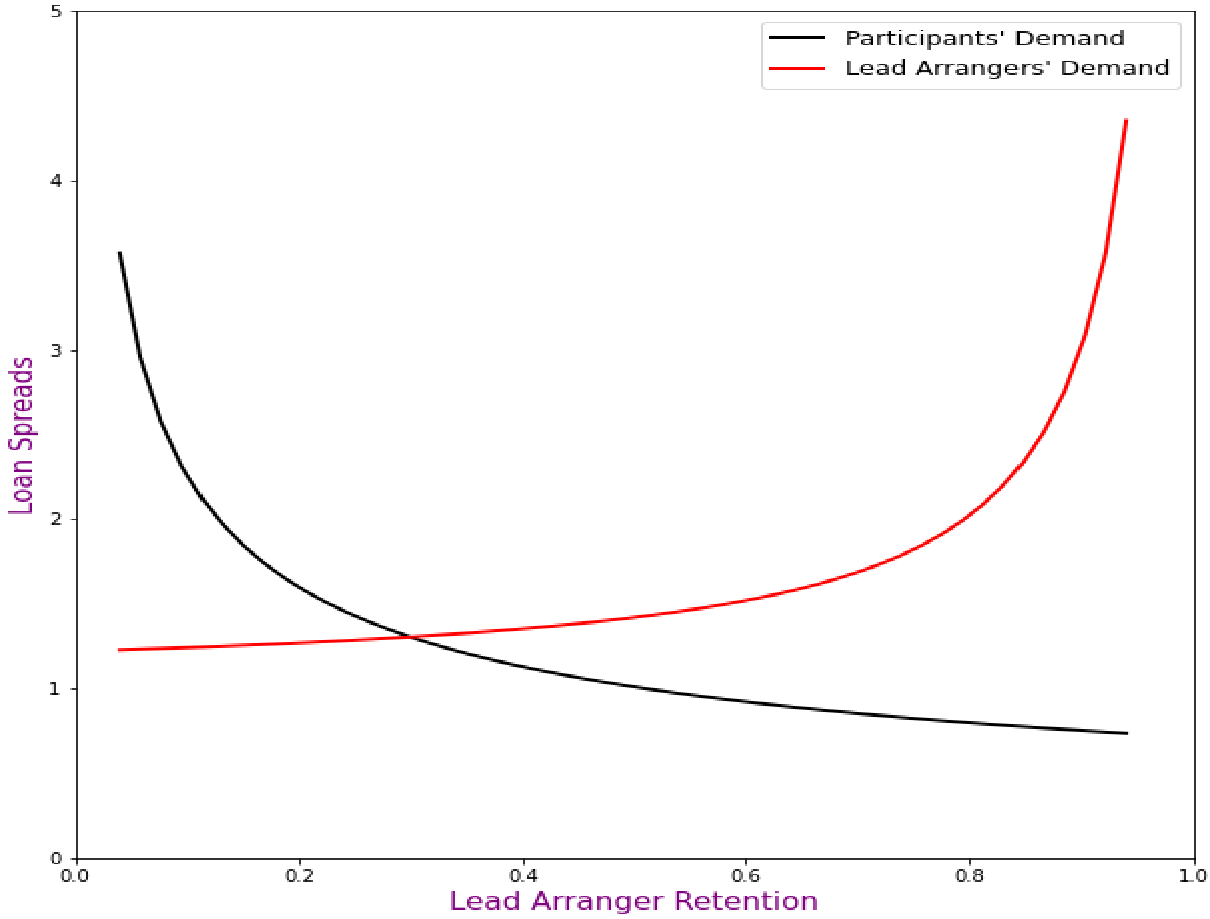
4.6 Conclusion

In this paper, we examine the spillover effect of capital regulation tightening in a lead arranger's country. Conducting a DiD analysis, we find that, after the U.S. stress test implementation, loan spreads decrease more in the cross-border loans led by stress-test banks compared to those led by other banks. The spillover effect is further confirmed outside the U.S. This suggests that borrowers in foreign countries, especially those closely related to the lead arranger, benefit from capital regulation tightening and would prefer cheaper cross-border loans. Then, how do local banks in foreign countries respond to the changes? Do the cheaper loans from the tightening country crowd out the local banks' lending supply? These questions deserve further investigation. Moreover, the prior literature finds an increase in domestic loan spreads after capital regulation tightening. Thus, to the same bank, the reduction in interest incomes from cross-border lending could be offset by the higher domestic loan spreads. Future work can examine changes in banks' interest margins afterwards and test whether banks make up overall profits by charging higher non-interest incomes such as fees.

While this paper observes an increase in lead arrangers' investment in cross-border loans following the U.S. stress test implementation, other research, such as by Aiyar et al. (2014); Forbes et al. (2017), reports a decrease in cross-border lending of U.K. banks. Forbes et al. (2017) attribute the observed decrease to an interaction between stricter capital requirements and an unconventional monetary policy adopted in 2012 in the U.K. The U.S. has also introduced unconventional monetary policy tools after the GFC, such as large-scale asset purchases. Thus, future research can test whether the spillover effect documented in this paper is weaker under the period of unconventional monetary policy in the U.S.

We posit that the spillover effect is rooted in a moral hazard issue among lenders in syndicated loans. Supporting evidence suggests that this effect is more pronounced when the moral hazard issue is more of a concern. Future studies could investigate this further by examining changes in the spreads of non-syndicated loans. Then, we propose the *Regulatory Arbitrage Channel* to explain the spillover effect: the incentive of regulatory arbitrage increases the lead arranger's share into a cross-border loan following capital regulation tightening. We demonstrate that the lead arranger shares indeed decline. In an unreported test, we also find that, although the lead arranger invests more, it is less likely for the bank to invite local participants. While examining the change in the syndicate structure is not the focus of this paper, this finding is worthy of further exploration. To provide additional support for the *Regulatory Arbitrage Channel*, we examine the heterogeneity in capital regulation tightening. It shows that, when lending through subsidiaries, whether implementing Basel II on an unconsolidated basis or not is crucial to trigger the spillover. This is because regulatory arbitrage is more feasible if stricter capital requirements do not encompass a bank's foreign subsidiaries. Given that the sample comprises only 11 countries, all of which are advanced economies, the scope for differences in implementing new capital regulations is limited. Therefore, we suggest future studies to collect more granular data and explore additional cross-country heterogeneity, such as the exemption of foreign lending in capital regulations. Lastly, in the appendix, we conduct a placebo test showing that capital regulation tightening that targets the household sector does not affect the spreads of syndicated loans. Future studies can test whether those regulations affect mortgages or commercial real estate lending.

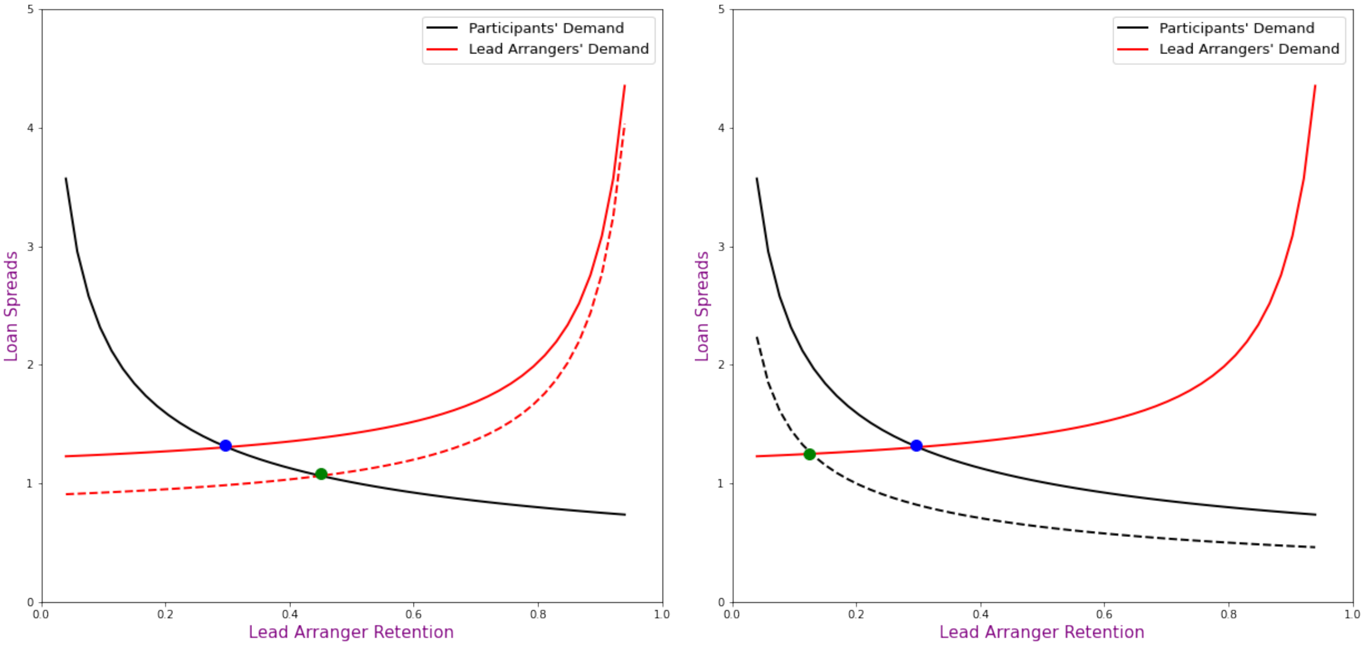
Figure 4.1: The Equilibrium Loan Spreads Determined by the Participants and Lead Arranger in A Loan



This figure depicts the equilibrium for a cross-border loan, with the horizontal axis representing the loan share retained by the lead arranger and the vertical axis indicating the loan spread. The equilibrium point is the intersection between the participants' demand curve (black) and the lead arranger's demand curve (red).

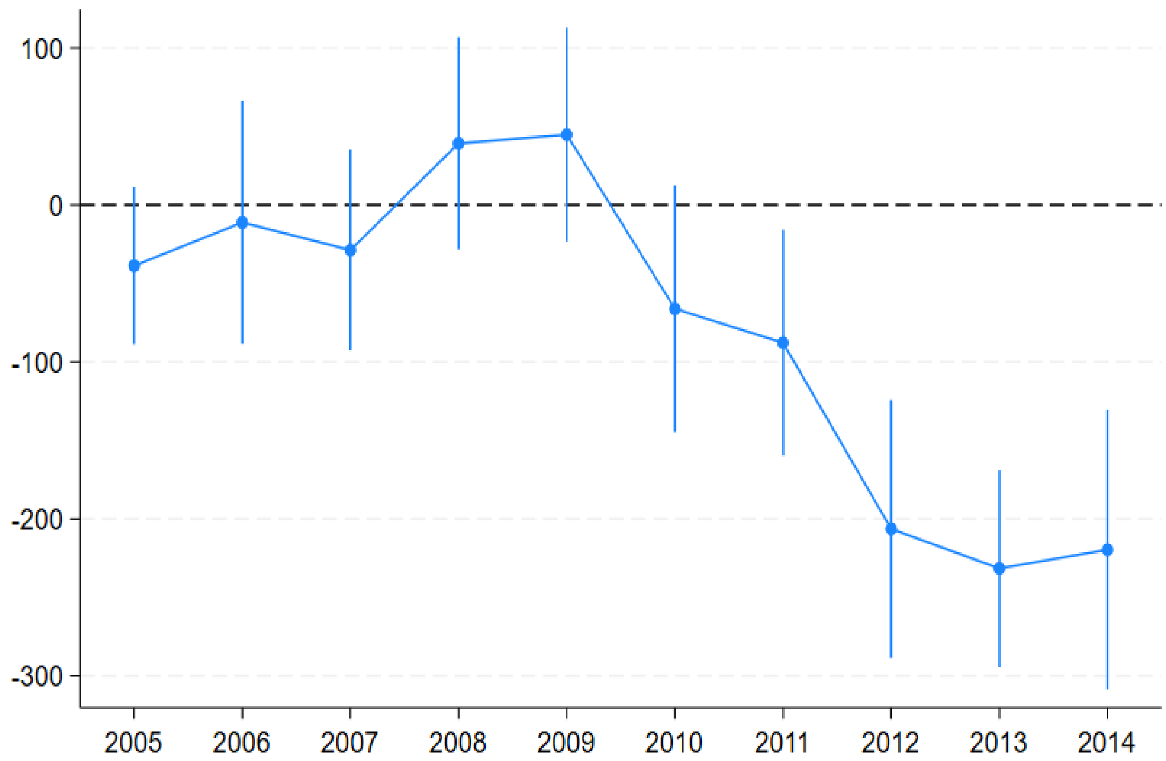
Figure 4.2: Movements of the Equilibrium following Capital Regulation Tightening

(a) The shift in the lead arranger's curve (b) The shift in the participants' curve



This figure depicts the changes in equilibrium for a cross-border loan. The horizontal axis represents the loan share retained by the lead arranger, and the vertical axis represents the loan spread. The equilibrium point is the intersection between the participants' demand curve (black) and the lead arranger's demand curve (red). It assumes that there is capital regulation tightening exogenously occurring in the lead arranger's country of the cross-border loan. It shifts the lead arranger's curve outwards, as shown in the left panel, and shifts the participants' curve downwards, as shown in the right panel

Figure 4.3: Changes in Spread Difference between the Treatment Group and the Control Group



This figure plots the coefficients from the regression of loan spreads on interaction terms between Treatment and each of the year dummies between 2004-2014. The first year and its interaction term are omitted. Treatment is a dummy taking one if the lead arranger of the loan participates in the 2009 SCAP. The regression includes loan type FEs, loan purpose FEs, bank FEs, borrower country FEs, borrowing industry FEs, and year FEs.

Table 4.1

Summary Statistics of Key Variables

The U.S. Sample	Observations	Mean	SD	25^{th}	Median	75^{th}
	Dependent Variables					
Loan Spreads (basis points)	3779	182.64	145.34	60	150	255
Lead Arranger Share (%)	986	11.76	10.92	5.4688	8.33	13
No. Participants	3779	12.18	10.61	5	10	17
	Main Independent Variables					
Treatment	3779	0.96	0.19	1	1	1
Post	3779	0.48	0.5	0	0	1
Later-included BHC	3779	0.01	0.07	0	0	0
Advanced-approach BHC	3779	0.95	0.23	1	1	1
Borrower Country Experiences	3779	0.17	0.12	0.08	0.14	0.24
Secured Loan	3779	0.32	0.47	0	0	1
Unrated Borrower	3779	0.52	0.50	0	1	1
Foreign Subsidiary Dummy	3779	0.07	0.25	0	0	0
Pre-Stress Test Quarter Dummy	3779	0.10	0.30	0	0	0
No. Local Participants	3779	2.24	2.97	0	1	4
Lead-participants Cooperation	3779	0.14	0.05	0.13	0.15	0.17
	Control Variables					
Loan maturity (months)	3779	54.24	23.97	36	60	60
Loan Amount (billion GBP)	3779	1.27	2.37	0.16	0.51	1.40
No. Lenders	3779	13.18	10.61	6	11	18
No. Covenants	3779	0.35	0.86	0	0	0
Relationship Intensity	3779	0.40	0.43	0	0.19	1
Log(Bank Assets)	3779	21.09	0.87	21.06	21.36	21.52
Bank Capital Ratio	3779	0.09	0.02	0.07	0.08	0.10
Bank ROA	3776	0.005	0.004	0.002	0.004	0.01
Bank Liquidity	3779	0.20	0.07	0.17	0.19	0.23
Log(Firm Assets)	3366	8.42	1.78	7.29	8.52	9.65
Firm Asset Tangibility	3334	0.32	0.22	0.14	0.29	0.48
Firm Cash and Equivalents	3351	0.10	0.09	0.04	0.07	0.12
Firm ROA	2463	36.41	213.53	0.02	0.04	0.47
Firm Interest Coverage	2873	15.37	34.02	3.37	6.88	13.53
Borrower Country GDP Growth	2755	0.01	0.04	-0.01	0.01	0.04
Borrower Country Inflation	3249	0.63	0.79	0.19	0.50	0.95
The Cross-country Sample	Observations	Mean	SD	25^{th}	Median	75^{th}
Loan Spreads (basis points)	52825	259.64	157.56	150	225	345
Treatment^{c}	52825	0.31	0.46	0	0	1
Post^{c}	52825	0.44	0.50	0	0	1
Borrower Country Tightening	52825	0.76	0.43	1	1	1
Unconsolidated Basis Dummy	756	0.49	0.50	0	0	1
Foreign Subsidiary Dummy	756	0.16	0.36	0	0	0

Notes: This table presents summary statistics for the variables used in the paper. The U.S. sample includes cross-border loans made by U.S. BHCs and originating from 2004 to 2014. The cross-country sample includes cross-border loans made by the 11 countries studied

Table 4.2

The Impact of Stress Test Implementation on Cross-border Loan Spreads

	1	2	3	4	5	6	7	8
	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads
Treatment×Post	-136.1***	-105.5***	-71.10***	-57.18*	-71.10**	-71.10*	-67.49**	-94.48***
	(-5.40)	(-4.07)	(-2.72)	(-1.91)	(-2.57)	(-1.87)	(-1.99)	(-3.51)
Log(Loan Maturity)	5.729	5.721	1.299	-1.483	1.299	1.299	6.511	5.131
	(1.12)	(1.13)	(0.22)	(-0.23)	(0.13)	(0.13)	(1.10)	(0.75)
Log(Loan Amount)	-13.64***	-13.27***	-9.990***	-10.89***	-9.990***	-9.990***	-6.314***	-13.01***
	(-9.30)	(-9.08)	(-5.42)	(-5.51)	(-4.40)	(-4.15)	(-3.43)	(-5.05)
Secured Indicator	64.18***	64.18***	55.04***	60.65***	55.04***	55.04***	58.69***	58.55***
	(12.35)	(12.36)	(8.49)	(8.68)	(10.87)	(7.42)	(8.32)	(7.46)
Log(No. Lenders)	-29.72***	-28.78***	-25.28***	-24.61***	-25.28***	-25.28**	-32.98***	-29.68***
	(-8.92)	(-8.70)	(-5.77)	(-5.33)	(-2.99)	(-3.05)	(-6.22)	(-5.22)
Log(No. Covenants)	-4.672	-5.855	-4.571	-4.568	-4.571	-4.571	0.874	10.50
	(-1.08)	(-1.35)	(-0.86)	(-0.80)	(-1.21)	(-0.74)	(0.14)	(1.30)
Relationship Intensity	-5.088	-4.513	-12.50**	-10.91**	-12.50**	-12.50**	-20.16***	-17.08***
	(-1.20)	(-1.07)	(-2.47)	(-2.03)	(-2.64)	(-2.33)	(-3.35)	(-2.80)
Log(Bank Assets)		-40.45**	-31.74	-44.13*	-31.74	-31.74	-34.31	4.214
		(-2.32)	(-1.35)	(-1.65)	(-0.98)	(-1.05)	(-1.36)	(0.17)
Bank Capital Ratio		643.8**	825.3**	1025.9**	825.3*	825.3	651.0*	1291.8***
		(2.45)	(2.46)	(2.55)	(2.01)	(1.66)	(1.65)	(3.43)
Bank ROA		144.0	1063.1*	1896.4***	1063.1*	1063.1	407.9	1456.9*
		(0.30)	(1.71)	(2.72)	(2.04)	(1.39)	(0.56)	(1.80)
Bank Liquidity		-80.01	33.52	29.29	33.52	33.52	-77.69	83.33
		(-0.87)	(0.32)	(0.26)	(0.25)	(0.23)	(-0.59)	(0.66)
Log(Firm Assets)			-5.136**	-4.255*	-5.136***	-5.136***	-5.189**	-1.620
			(-2.18)	(-1.67)	(-3.02)	(-3.82)	(-1.96)	(-0.58)
Firm Asset Tangibility			-8.704	-4.109	-8.704	-8.704	1.526	-41.12**
			(-0.59)	(-0.25)	(-0.45)	(-0.33)	(0.09)	(-2.23)
Firm Cash and Equivalents			78.56**	84.73**	78.56**	78.56*	79.31*	9.364
			(2.18)	(2.23)	(2.36)	(2.08)	(1.95)	(0.21)
Firm ROA			-0.0128	-0.0128	-0.0128	-0.0128	-0.0255**	-0.0113
			(-1.59)	(-1.59)	(-1.32)	(-1.31)	(-2.52)	(-1.36)
Firm Interest Coverage			-0.114	-0.149*	-0.114	-0.114	-0.0956	0.0256
			(-1.49)	(-1.87)	(-0.73)	(-0.72)	(-1.17)	(0.29)
Borrower Country GDP Growth								19.71
								(0.30)
Borrower Country Inflation								-3.103
								(-0.75)
Sample Period	2004-2014	2004-2014	2004-2014	Excluding the GFC	2004-2014	2004-2014	2004-2014	2004-2014
Bank and Borrower Country FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
SIC2 and Year FEs	Yes	Yes	Yes	Yes	Yes	Yes		Yes
SIC2-year FEs							Yes	
Industry-year FEs								
Cluster	No	No	No	No	BHC and Borrower	BHC and year	No	No
Observations	3759	3755	2212	2012	2212	2212	2131	1596
Adjusted R^2	0.593	0.596	0.601	0.603	0.601	0.601	0.646	0.637

Notes: This table reports results from a standard difference-in-difference regression model. The loan-level sample includes cross-border loans made by U.S. BHCs

Table 4.3

The Impact of Stress Test Implementation on Lead Arrangers' Willingness to Retain Loan Shares

	1	2	3	4	5	6
	Lead Arranger Share	Lead Arranger Share	Lead Arranger Share	No. Participants	No. Participants	No. Participants
Treatment×Post	6.825**	5.542*	8.029**	0.0384	-0.291	-0.0416
	(2.32)	(1.74)	(2.18)	(0.18)	(-1.29)	(-0.15)
Log(Loan Maturity)	-0.991	-1.023	-0.141	0.102**	0.0982**	0.169***
	(-1.41)	(-1.45)	(-0.15)	(2.46)	(2.34)	(2.87)
Log(Loan Amount)	0.0120	-0.00862	0.0491	0.279***	0.274***	0.224***
	(0.06)	(-0.04)	(0.17)	(20.00)	(19.37)	(11.14)
Secured Indicator	2.083**	2.145***	0.218	-0.318***	-0.308***	-0.286***
	(2.56)	(2.68)	(0.21)	(-7.13)	(-6.90)	(-4.78)
Log(No. Lenders)	-10.95***	-11.03***	-11.06***			
	(-17.40)	(-17.55)	(-10.59)			
Log(No. Covenants)	1.401	1.376	1.211	0.367***	0.375***	0.450***
	(1.49)	(1.45)	(1.01)	(8.66)	(8.76)	(7.34)
Relationship Intensity	-0.253	-0.167	-0.725	0.00291	0.00794	-0.131**
	(-0.41)	(-0.27)	(-0.75)	(0.08)	(0.21)	(-2.52)
Log(Bank Assets)		2.878	0.476		0.652***	0.554***
		(1.21)	(0.13)		(4.78)	(2.84)
Bank Capital Ratio		67.70*	14.96		-0.402	-0.736
		(1.94)	(0.25)		(-0.17)	(-0.22)
Bank ROA		57.79	158.6		14.82***	9.919
		(0.88)	(1.44)		(3.11)	(1.49)
Bank Liquidity		24.85*	14.35		2.620***	1.490
		(1.70)	(0.72)		(3.41)	(1.52)
Log(Firm Assets)			-0.176			0.190***
			(-0.46)			(9.38)
Firm Asset Tangibility			-2.121			-0.137
			(-0.74)			(-0.99)
Firm Cash and Equivalents			-3.817			0.333
			(-0.71)			(1.10)
Firm ROA			0.00151			-0.0000292
			(1.07)			(-0.25)
Firm Interest Coverage			-0.000863			-0.000681
			(-0.05)			(-0.88)
Sample Period	2004-2014	2004-2014	2004-2014	2004-2014	2004-2014	2004-2014
Bank, Borrower Country, SIC2, and Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1195	1195	694	5121	5117	3113
Adjusted/Pseudo R^2	0.552	0.552	0.519	0.087	0.089	0.103

Notes: This table reports results from a standard difference-in-difference regression model. The loan-level sample includes cross-border loans made by U.S. BHCs and originating from 2004 to 2014. In Column 1-3, the results are estimated using an ordinary least squares regression, and the dependent variable is the lead arranger share. In Column 4-6, the results are estimated using an ordered probit regression, and the dependent variable is the number of participants. Treatment is a dummy taking one if the lead arranger of the loan participates in the 2009 SCAP. Post is a dummy taking one if the loan originates after 2009. All other variables are defined in Table A1. All regressions include loan type FEs, loan purpose FEs, bank FEs, borrower country FEs, borrower SIC2 FEs, and year FEs. T-statistics, adjusted for heteroskedasticity, are reported in brackets. +, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Table 4.4

The Impact of Stress Test Implementation on Cross-border Loan Spreads in the Extended Sample

	1	2	3	4	5	6
	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads
Treatment×Post	-130.6***	-102.4***	-61.70**	-82.63***		
	(-5.18)	(-3.91)	(-2.25)	(-3.25)		
Later-included BHC×Post-2014	-78.95+	-62.26	-59.78			
	(-1.61)	(-1.34)	(-1.44)			
Treatment×Period 2010-2014					-82.44***	-71.90*
					(-3.24)	(-1.87)
Treatment×Advanced-approach BHC×Period 2010-2014						-10.41
						(-0.34)
Treatment×Post-2014					-118.9***	23.01
					(-3.56)	(0.35)
Treatment×Advanced-approach BHC×Post-2014						-142.4**
						(-2.47)
Sample	2004-2019	2004-2019	2004-2019	Excluding later-included BHCs	Excluding later-included BHCs	Excluding later-included BHCs
Bank, Borrower Country, SIC2, and Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Loan-level Controls	Yes	Yes	Yes	Yes	Yes	Yes
Lender-level Controls		Yes	Yes	Yes	Yes	Yes
Borrower-level Controls			Yes	Yes	Yes	Yes
Observations	4834	4830	2943	2934	2928	2934
Adjusted R2	0.572	0.575	0.573	0.573	0.573	0.573

Notes: This table reports results from a standard difference-in-difference regression model. The loan-level sample includes cross-border loans made by U.S. BHCs and originating from 2004 to 2014. In the last three columns, the sample excludes the BHCs that are included in CCAR after 2013. The dependent variable is loan spreads, which are the all-in-drawn spreads. Treatment is a dummy taking one if the lead arranger of the loan participates in the 2009 SCAP. Post is a dummy taking one if the loan originates after 2009. Later-included BHC is a dummy taking one if the lead arranger is new to the 2014 CCAR. Post-2014 is a dummy taking one if the loan originates after 2014. Advanced-approach BHC is a dummy taking one if the lead arranger is identified as an advanced-approach BHC in the 2014 CCAR. Period 2010-2014 is a dummy taking one if the loan originates during 2010-2014. The control variables include the loan maturity, loan amount, an indicator for secured loans, number of lenders, number of covenants, relationship intensity, lender size, lender's capital adequacy, lender ROA, lender liquidity ratio, borrower size, borrower asset tangibility, borrower cash and equivalents, borrower ROA, and borrower interest coverage ratio, and they are defined in Table A1. All regressions include loan type FEs, loan purpose FEs, bank FEs, borrower country FEs, borrower SIC2 FEs, and year FEs. T-statistics, adjusted for heteroskedasticity, are reported in brackets. +, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Table 4.5

Heterogeneity of the Treatment Effect

	1	2	3	4	5	6
	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads
Treatment×Post	121.9***	-95.79***	47.62	-0.614	-74.61***	-192.7***
	(2.60)	(-3.02)	(1.51)	(-0.02)	(-2.81)	(-5.65)
Treatment×Post×Borrower Country Experiences	-1294.5***					
	(-4.86)					
Treatment×Post×Secured Loan		101.0**				
		(2.37)				
Treatment×Post×Unrated Borrower			-121.8***			
			(-3.54)			
Treatment×Post×Foreign Subsidiary Dummy				-92.30*		
				(-1.94)		
Treatment×Post×Pre-Stress Test Quarter Dummy					109.7***	
					(4.00)	
Treatment×Post×Minimum Capital Distance						115.9***
						(3.53)
F-statistic	33.45	0.02		8.58	1.89	0.65
Sample Period	2004-2014	2004-2014	2004-2014	2004-2014	2004-2014	2004-2014
Bank, Borrower Country, SIC1, and Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
The Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2218	2218	2218	2218	2218	1772
Adjusted R^2	0.604	0.607	0.601	0.601	0.602	0.620

Notes: This table reports results from a standard difference-in-difference regression model. The loan-level sample includes cross-border loans made by U.S. BHCs and originating from 2004 to 2014. In the last three columns, the sample excludes the BHCs that are included in CCAR after 2013. The dependent variable is loan spreads, which are the all-in-drawn spreads. Treatment is a dummy taking one if the lead arranger of the loan participates in the 2009 SCAP. Post is a dummy taking one if the loan originates after 2009. Borrower Country Experiences measures the lead arranger's past lending experiences to the borrower's country. Secured Loan is a dummy taking one if a loan is secured by collaterals. Unrated Borrower is a dummy taking one if the borrower does not have an S&P credit rating. Foreign Subsidiary Dummy takes one if the loan is made by one of the lead arranger's foreign subsidiaries. Pre-Stress Test Quarter Dummy takes one if the loan is made within the last quarter before the submission of the year's stress test. Minimum Capital Distance is the minimum of the differences between stressed tier-one capital, total capital, leverage ratio and the minimum capital requirements. The variables in interaction terms that are not reported are included in regressions. The control variables include the loan maturity, loan amount, an indicator for secured loans, number of lenders, number of covenants, relationship intensity, lender size, lender's capital adequacy, lender ROA, lender liquidity ratio, borrower size, borrower asset tangibility, borrower cash and equivalents, borrower ROA, and borrower interest coverage ratio, and they are defined in Table A1. All regressions include loan type FEs, loan purpose FEs, bank FEs, borrower country FEs, borrower SIC2 FEs, and year FEs. T-statistics, adjusted for heteroskedasticity, are reported in brackets. +, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Table 4.6

The Spillover from Capital Regulation Tightening – Cross-country Results

	1	2	3	4	5	6	7	8
	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads
Treatment^c	8.027***	2.393	3.290+					
	(5.41)	(1.37)	(1.64)					
Post^c	6.348***	3.824**	6.550***	7.582***	8.789***			
	(5.11)	(2.56)	(3.79)	(3.25)	(3.70)			
Treatment$^c \times$Post^c	-6.611***	-6.283**	-11.39***	-13.16***	-10.74***	-12.64**	-22.11**	
	(-2.99)	(-2.44)	(-3.87)	(-3.47)	(-2.65)	(-2.22)	(-2.54)	
Treatment$^c \times$Post$^c \times$Macroprudential Tightening Dummy							15.35	
							(1.35)	
Treatment$^c \times$Post^{c-1}								-6.600
								(-0.73)
Treatment$^c \times$Post^{c1}								-16.94*
								(-1.85)
Treatment$^c \times$Post^{c2}								-21.84*
								(-1.90)
Sample	2007Q1-019Q3	2007Q1-019Q3	2007Q1-019Q3	2007Q1-019Q3	Drop the U.S.	2007Q1-019Q3	2007Q1-019Q3	2007Q1-019Q3
Lender Country, Borrower Country, SIC2 FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Stricter FEs				Yes	Yes	Yes	Yes	Yes
Quarter-cohort FEs					Yes	Yes	Yes	Yes
Loan-level Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Lender-level Controls		Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower-level Controls			Yes	Yes	Yes	Yes	Yes	Yes
Observations	52817	31862	20987	20377	18411	20354	20354	20354
Adjusted R^2	0.466	0.473	0.494	0.572	0.577	0.605	0.605	0.605

Notes: This table reports results from a stacked difference-in-difference regression model. Each event of capital regulation tightening is a cohort. There are 74 events in 11 countries. The event window is eight quarters before and after the event quarter, excluding the event quarter. For each event, the cross-border loans from the tightening country and non-tightening countries eight quarters before and after the event date are included in the sample. Then, the sample excludes the destination countries that borrow only from the non-tightening source countries. In the fifth column, the sample drops all tightening events in the U.S. The treatment group include the loans arranged by lead arrangers from the tightening source country. Correspondingly, the loans arranged by lenders from other source countries consist of the control group. For each country in the original control group that tightens capital regulation, we remove all loans originating before (after) the tightening date if the tightening happens before (after) the event quarter. The dependent variable is loan spreads, which are the all-in-drawn spreads. Treatment^c is a dummy taking one if a loan is from tightening countries. Post^c is a dummy taking one if a loan originates after the event date. Macroprudential Tightening Dummy takes one if the event/treated country experiences any tightening event of Macroprudential policies other than capital regulation in the event quarter. Post^{c-1} is a dummy taking one if a loan originates in the first year before the event quarter. Post^{c1} is a dummy taking one if a loan originates in the first year after the event quarter. Post^{c2} is a dummy taking one if a loan originates in the second year after the event quarter. The control variables include the loan maturity, loan amount, an indicator for secured loans, number of lenders, number of covenants, relationship intensity, lender size, lender's capital adequacy, lender ROA, lender liquidity ratio, borrower size, borrower asset tangibility, borrower cash and equivalents, borrower ROA, and borrower interest coverage ratio, and they are defined in Table A1. The first three regressions include loan type FEs, loan purpose FEs, lender country FEs, borrower country FEs, and borrower SIC2 FEs. The rest of the regressions include stricter FES: loan type-cohort FEs, loan purpose-cohort FEs, lender country-cohort FEs, borrower country-cohort FEs, and borrower SIC2-cohort FEs. The 6-8 regressions also includes quarter-cohort FEs. T-statistics, adjusted for heteroskedasticity, are reported in brackets. +, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Table 4.7

Regulatory Arbitrage by Lending from Foreign Subsidiaries in Non-tightening Countries

	1	2	3
	Loan Spreads	Loan Spreads	Loan Spreads
$\text{Treatment}^c \times \text{Post}^c$	-36.22***		
	(-3.86)		
$\text{Treatment}^c \times \text{Post}^c \times$Borrower Country Tightening	31.65***		
	(3.16)		
Unconsolidated Basis×Post		8.071	26.12
		(0.37)	(1.16)
Unconsolidated Basis×Post×Foreign Subsidiary Dummy			-138.8***
			(-2.73)
Sample	Capital Regulation Tightening Events	Basel II Implementation	Basel II Implementation
Stricter FEs	Yes	Yes	Yes
Observations	20354	739	739
Adjusted R^2	0.605	0.545	0.553

Notes: This table reports results from two difference-in-difference regression models. In the first column, the results are estimated from a stacked difference-in-difference regression. Each event of capital regulation tightening is a cohort. There are 74 events in 11 countries. The event window is eight quarters before and after the event quarter, excluding the event quarter. For each event, the cross-border loans from the tightening country and non-tightening countries eight quarters before and after the event date are included in the sample. Then, the sample excludes the destination countries that borrow only from the non-tightening source countries. The treatment group include the loans arranged by lead arrangers from the tightening source country. Correspondingly, the loans arranged by lenders from other source countries consist of the control group. For each country in the original control group that tightens capital regulation, we remove all loans originating before (after) the tightening date if the tightening happens before (after) the event quarter. The dependent variable is loan spreads, which are the all-in-drawn spreads. Treatment^c is a dummy taking one if a loan is from tightening countries. Post^c is a dummy taking one if a loan originates after the event date. Post^{c-1} is a dummy taking one if a loan originates in the first year before the event quarter. Borrower Country Tightening is a dummy taking one if the borrower's country of a loan has ever implemented stricter capital regulations during the event window. All regressions include stricter FES: loan type-cohort FEs, loan purpose-cohort FEs, lender country-cohort and borrower country-cohort FEs, borrower SIC2-cohort FEs, and quarter-cohort FEs.

In the last two columns, the results are estimated from a standard difference-in-difference regression. The sample includes all loans from the 11 countries originating before and after eight quarters of each country's implementation of Basel II. Unconsolidated Basis Dummy takes one if Basel II was implemented unconsolidated basis in the lead arranger's country of a loan. Foreign Subsidiary Dummy takes one if the loan is made by one of the lead arranger's foreign subsidiaries. All regressions include loan type FEs, loan purpose FEs, lender country FEs, borrower country FEs, and borrower SIC2 FEs.

In all regressions, the control variables include the loan maturity, loan amount, an indicator for secured loans, number of lenders, number of covenants, relationship intensity, lender size, lender's capital adequacy, lender ROA, lender liquidity ratio, borrower size, borrower asset tangibility, borrower cash and equivalents, borrower ROA, and borrower interest coverage ratio, and they are defined in Table A1. T-statistics, adjusted for heteroskedasticity, are reported in brackets. +, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.

Chapter 5

Contributions and Future Avenues for Research

This thesis explores two emerging trends in the syndicated loan market and tests the spillover effect of capital regulation changes.

Contributions

Chapter 2 extend the scope of Schwert (2018) by exploring the time-variation of the matching between bank-dependent firms and well-capitalised banks. Notably, it highlights a discernible trend of disappearing matching after 2010. This chapter contributes to the broader literature on two-sided bank-firm matching, examined by studies such as Berger et al. (2005); Levine et al. (2020), showing that capital-based matching is more vulnerable to macroeconomic and regulatory changes. While a wide range of research testing the impact of the prolonged low-interest rate environment and capital regulations on bank lending (Borio and Gambacorta, 2017; Acharya et al., 2018; Claessens et al., 2018; Heider et al., 2019; Cortés et al., 2020; Molyneux et al., 2020; Lopez et al., 2020, see), this chapter sheds light on that the changes also disturb firms' borrowing choices of banks. Ma et al. (2019) examine the effect of bank financing on bond prices. Inversely, this chapter shows that access to the bond market can also affect the loan market.

Chapter 3 demonstrates that co-lead arrangements facilitate ex-ante screening and ex-post monitoring, which complements existing literature by introducing a novel approach to improve information production in syndicated loans. The phenomenon of information production through collaboration and co-leadership is observed in various forms of syndication, as evidenced by studies on Venture Capital syndication (Lerner, 1994; De Clercq and Dimov, 2004; Casamatta and Haritchabalet, 2007), IPO underwriting syndication (Corwin and Schultz, 2005), and bond underwriting syndication (Song, 2004; Shivdasani and Song, 2011; Carbó-Valverde et al., 2021); this chapter contributes to the literature by providing evidence in the syndicated loan market.

Chapter 4 documents that capital regulation tightening in the country of a lead arranger reduces spreads of the lender's cross-border loans. Houston et al. (2012); Aiyar et al. (2014); Bremus and Fratzscher (2015); Berrospide et al. (2016); Buch and Goldberg (2016); Bussière et al. (2016); Danisewicz et al. (2017); Forbes et al. (2017); Forbes (2021); Reinhardt et al. (2023) examine the spillover effect of stricter capital regulation on bank lending. While those papers focus on the quantity of lending, this chapter reveals that capital regulations can also influence the pricing of cross-border loans. This chapter also contributes to the literature studying the bank lending channel of other international spillovers (see (Temesvary et al., 2018; Avdjiev and Takáts, 2019; Morais et al., 2019; Takáts and Temesvary, 2020) for monetary policy changes and Cetorelli and Goldberg (2011); Giannetti and Laeven (2012); Hale et al. (2020) for financial shocks). Addressing the documented moral hazard problem in the U.S. syndicated loan market (Sufi, 2007; Ivashina, 2009; Dass et al., 2020), this study complements existing research by exploring the agency issue within the broader global syndicated market.

Future Avenues for Research

In this thesis, I document two noteworthy trends in the U.S. syndicated loan market: the diminishing matching phenomenon and the rising prevalence of co-lead loans.

Recent papers such as Aldasoro et al. (2022, 2023) highlight an increase in the involvement of shadow banks (e.g., mutual funds and investment banks) in the syndicated loan market. While Chapter 2 centres on commercial banks and Chapter 3 does not explicitly probe the driving forces behind co-lead arrangements, I propose future research to investigate the impact of growing non-bank participation on the observed trends. Furthermore, as the merged sample concludes in 2019, subsequent studies could explore changes in the syndicated loan market after the COVID-19 pandemic. For instance, Chapter 2 attributes the disappearing matching to the post-GFC low-interest rate environment; hence, re-evaluating the strength of capital-based matching after the 2022 interest rate rise is an intriguing avenue for exploration. Although Chapter 2 and Chapter 3 predominantly focus on the U.S. sample, unconventional monetary policies, extremely low policy rates, and stricter capital regulations are implemented globally after the GFC and the European debt crisis. Thus, I prompt future research to investigate whether similar changes in diminishing matching and rising co-lead loans are observable in other economies.

In Chapter 3 and Chapter 4, I investigate information production and regulatory spillovers, highlighting the uniqueness of syndicated loans. One potential for future research is to examine similar research questions using the data of non-syndicated corporate lending.

Furthermore, this thesis uses micro-level data to test individual bank-firm relationships and loan terms. I suggest an extension of these implications to the aggregate level. For instance, in Chapter 4, I find that capital regulation tightening leads to lower spreads in cross-border loans. This prompts a question about whether these cheaper cross-border capital inflows from the tightening country crowd out the credit supply of local banks.

Last but not least, while this thesis concentrates on the lending market, a suggestion for future research involves probing related questions in other capital markets. The existing literature has documented some relevant findings. In line with findings in Chapter 3, Shivdasani and Song (2011) documents the rapid emergence of co-lead arrangements in the syndicated bond underwriting market since 2000. Additionally, Papoutsi and Darmouni (2022) notes a considerable increase in banks' share of unrated bond issuance following the increasing participation of small and unrated firms in the European bond market. This indicates a possible extension of lending relationships between firms and banks to the bond market. Therefore, future studies could explore whether matching between bank-dependent firms and well-capitalized banks exists in other financing markets, such as bonds or commercial paper.

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Appendix A

Chapter 1 Appendix

A.1 Construction of the Dataset: Additional Details

A.1.1 Variable Definition

Table A.1 contains the definitions, references, and sources of key variables used in this study.

Insert Table A.1 here.

A.1.2 Details on Manual Matching Dealscan with Compustat from 2017 to 2019

We use firms' balance sheet information to measure borrower-specific variables. Dealscan only provides detailed loan-level data such as loan spreads, loan maturities, and so on. The source of balance sheet information comes from Compustat. Since the databases have no common digital identifier, We need to merge the firms in Dealscan with those in Compustat manually. Among the prior sources for the merge, Chava and Roberts (2008)'s matching table is the most popular approach. Since their 2008 paper, Chava and Roberts have kept publishing and updating their merging results. Their latest matching table contains loans originating from 1987 to 2017 and is updated in

2017. In Dealscan, however, some loans that originated in 2017 could be added to the database after 2017. Therefore, We use Chava and Roberts's matching results up to 2016 and manually merge firms' balance sheet information of all loans made from 2017 to 2020.

In total, Chava and Roberts (2008) have merged 177,012 loans, and there are 331,593 loans originated before 2018. There are several reasons for a firm being unmerged. One of the possible reasons is that Dealscan records loans from privately held firms, while Compustat only contains publicly-held companies. Another reason could be that the two databases are sourced differently. For a North American company to be added to Compustat, it must fill distinct 10K's or 10Q's with the SEC, while Dealscan data is compiled from a broader source, including the SEC fillings other than 10K or 10Q. The next paragraphs briefly specify the way we process the merge.

Because there is no common digital identifier between the databases, a successful merger depends on the similarity of the other identifying information (which is most likely to be string-type). The identifying information used by Chava and Roberts is company names. For example, a Dealscan firm named "oaktree specialty lending corp" is matched with a Compustat firm named "oaktree specialty lending cp". We use a Python package called "FuzzyWuzzy" to calculate the similarity between two strings. This package provides several techniques to perform the calculation. Following Chava and Roberts (2008), We use the one called "partial_ratio". "partial_ratio" compares the shorter string with the substring of the same length in the longer string and gives a score from 1 to 100 (a higher score means higher similarity). In the previous example, "partial_ratio" will give multiple matches for 'oaktree specialty lending corp' with different scores. Among all the matches, the greatest score is 96 when the Dealscan firm is matched with "oaktree specialty lending cp", which We consider as a successful match. However, the highest score may not always produce a successful match - there could be false matches. For example, a Dealscan company named "nextcare inc" and a different Compustat company named "nextcure inc" have a score of 92 because of the high similarity of the letters involved in the strings.

To avoid false matches, We manually check each match and drop the false ones.[1]

A firm in Dealscan may change its name to Compustat. Following (Chava and Roberts, 2008), We conduct the matches loan by loan because a loan-level matching can locate the date of the borrower appearing in Dealscan. In Dealscan, there are 64,411 loans during 2017-2020, and 26937 of those are borrowed by North American companies (identified by the Dealscan variable "country"). To date, We have manually checked all matching pairs with a score greater than or equal to 90 (In (Chava and Roberts, 2008), only 23.78% of matches have a score lower than 90). As a result, We have matched 8559 loans, among which 6698 are borrowed by North American companies.

Besides company names, we also use the tickers and the names of parent companies to match. Here is an example of using the parent name to match. Searching all names in Compustat, there is not a close match for a Dealscan firm named "sc realty private reit inc". Then, using the name of that company's parent - "sumitomo corp", We find a perfect match in Compustat. In addition, some firms borrowing before 2017 borrow again. These firms already have matching results in (Chava and Roberts, 2008). These firms may be taken over or go private during 2017-2020, but if they are still contained in Compustat with the same name, We include them in the linking table.

Taking those further steps, we extend the matches to 12947 loans, among which 9737 are borrowed by North American companies. Given the total number of loans, the matching rate (matching rate for North American loans) is 20.10% (36.15%), compared to 53.38% (56.88%) in (Chava and Roberts, 2008). As a result, the final sample increases by 1927 loans. From 2018Q1 to 2019Q4, the loan observations are 234, 340, 191, 280, 205, 243, 221, and 213. Noticed that the average loan observation during 2010Q1-2017Q4 is 232.75, and this indicates that our matching results are representative.

[1] Another way to address the false matches is to use more identifications, such as states and industries, to make a match of names more precise (see Cohen et al. (2021)).

A.2 Bond Market Expansion and Narrowing Capital Differences

A.2.1 Time-series of U.S. Public Bond Issues and Issuance

Figure A.1 plots annual issues and issuance in the U.S. public bond market.

Insert Figure A.1 here.

A.2.2 Moody's Seasoned Corporate Bond Yields

Figure A.2 plots two time series of Moody's seasoned corporate bond yields.

Insert Figure A.2 here.

A.2.3 Time-series of Bond Yields Relative to Loan Yields

Figure A.3 plots two time series of bond yields relative to loan yields.

Insert Figure A.3 here.

A.2.4 Time-series of Capital ratios of Well-capitalised Banks and Poorly-capitalised Banks

Figure A.4 plots two time series of capital ratios of well-capitalised banks and poorly-capitalised banks.

Insert Figure A.4 here.

A.3 Additional Results of the Semiparametric Matching Model

A.3.1 Estimates of the Semiparametric Model in the Pre-COVID and the Pre-GFC Periods

Table A.2 estimates the Semiparametric Matching Model from 2015Q1 to 2019Q4 and from 2002Q3 to 2007Q2.

Insert Table A.2 here.

A.3.2 Lending Growth under Counterfactuals - Loan Amount is Equally split to Each Syndicate Participate

Table A.3 estimates the credit access of unrated firms under counterfactuals. We equally split the loan amount to each syndicate participant.

Insert Table A.3 here.

A.4 Details on the Procedure of Counterfactual Construction

A.4.1 The Procedure of Counterfactual Construction

In this subsection, we list the steps of comparing the loan access during the COVID-19 shock under the benchmark and counterfactual scenarios in Section 2.5. The testing procedure is summarised in five steps.

Step 1: To construct the benchmark, we identify an unrated firm's pre-crisis relationship bank as the bank from which the unrated firm actually borrows in the most recent loan before the COVID-19 shock.

Step 2: We estimate Equation 3 from 2015Q1 to 2019Q4.

Step 3: To generate the counterfactual scenario, we either use the estimates from a pre-GFC sample or take the absolute value of $\hat{\beta}_1$. Then, in each quarter, we sort all bank-firm matchings in a descending order based on the estimated value of V_{bf} in Equation 2. We assign a firm to a bank from the first to the last bank-firm matching in the ladder until all firms have a matched bank and all banks use up their quotes. The quote of a bank is the number of firms it lends to in the quarter. For all unrated firms, we identify their pre-crisis relationship bank as the matched bank in the most recent quarter.

Step 4: We calculate a bank's total annualised syndicated loan amount both in the pre-COVID period (2017Q1-2019Q4) and in the first year of the COVID period (2020Q2-2020Q4); then, we measure the change in lending provisions as the percentage change of the bank's pre- and within-COVID loan amount.[2] We then average changes in lending supply across all unrated firms' pre-crisis relationship banks as a measure for loan access under each of the benchmark and counterfactual scenarios.

Step 5: Following the same procedure of constructing confidence intervals for the parameter estimate, we first extract 50 subsamples of original inequalities to get 50 sets of parameter estimates. Then, we redo Step 3 to obtain counterfactual matches and conduct a bootstrap by obtaining 20 random draws from the counterfactual matches for each set of parameters. Lastly, we redo Step 4 to obtain sampling distribution for the loan access. The 95% confidence interval is between the 5^{th} and 95^{th} percentiles of the sampling distribution.

[2]Because a syndicated loan involves multiple participants, we also split the loan amount equally to each bank and construct an alternative measure for lending provisions. The results are reported in Appendix A.3.2.

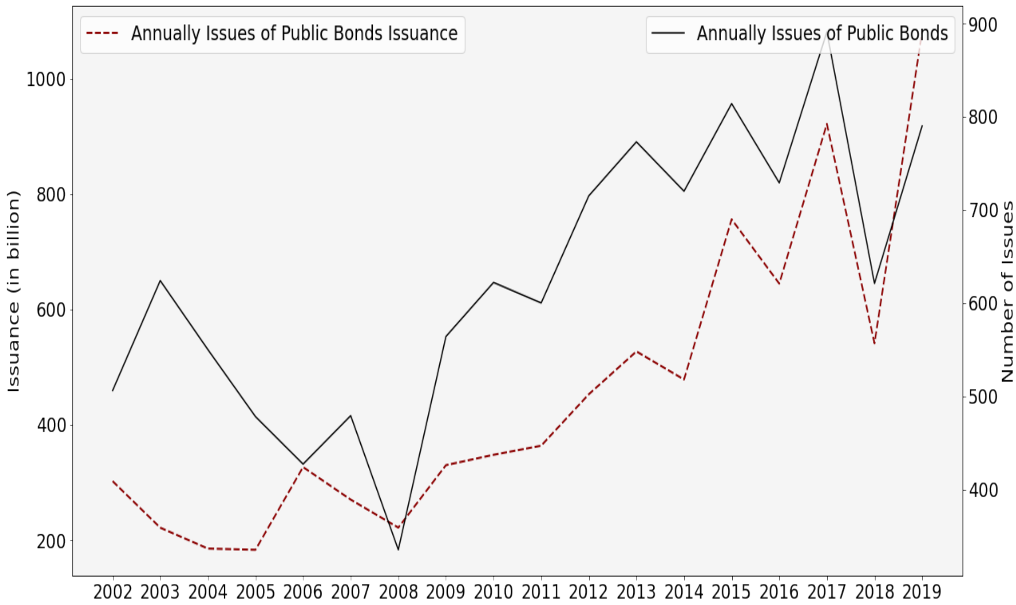
Table A.1
Variable Definitions

Name	Measure	Source	Name	Measure	Source
		Dependent Variable			
Observed Match	A dummy takes one if the bank-firm pair is observed in Dealscan	Dealscan	Obtain Rating Dummy	A dummy taking one if the borrower in a loan obtains a rating after the loan origination	Capital IQ
		Main Independent Variables			
Bank Capitalisation	Market capitalization (product of share price and common share outstanding) divided by quasi-market assets (sum of market capitalization and book liabilities)	Compustat Bank	Unrated Dummy	A dummy taking the value of one if a firm does not have an S&P long-term issuer rating in the loan origination month	Compustat North America
Firm Size	Natural logarithm of a firm's total book assets	Compustat North America	Small (Large) Unrated Dummy	A dummy taking the value of one if a firm is unrated and is in the bottom (top) tercile (terciles) based on the firm size in a given quarter	Compustat North America
Estimated Costs of Switching from Loans to Bonds	The average bond yields of the closest bond issuing firms minus the average loan rates paid by the estimated firm in a given quarter. The closest bond issuing firms are those being in the same two-digit SIC as the estimated firm and in the same tercile as the estimated firm based on firm size, operating income over total book assets, and Tobin's Q.	DealScan; Compustat North America; FISD; authors own calculation.	Costly-switch (Costless-switch) Unrated Dummy	A dummy taking the value of one if a firm is unrated and has positive (nonpositive) estimated switching costs	DealScan; Compustat North America; FISD; authors own calculation.
Loan Outstanding over Debts at Previous Year-end	The proportion of a firm's loan outstanding in total debts in a given year	Capital IQ Capital Structure	Loan-heavy (Loan-light) Unrated Dummy	A dummy taking the value of one if a firm is unrated and has 100% (less than 100%) loan outstanding over debts	Capital IQ Capital Structure

Variable Definitions – Table A.1 Continued

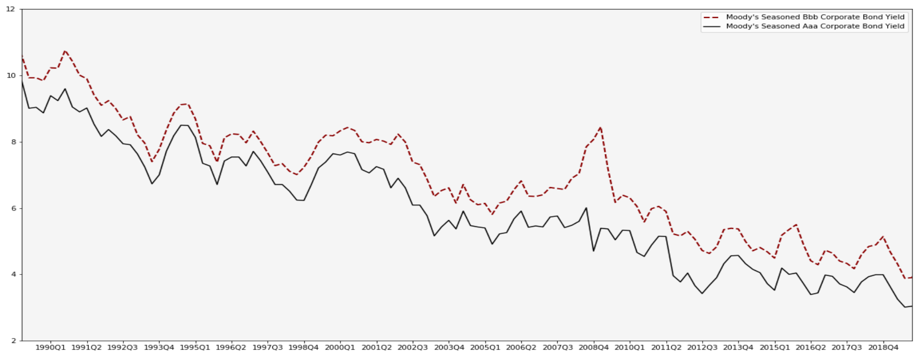
Name	Measure	Source	Name	Measure	Source
Non-performing Asset Ratio	Non-performing assets divided by total book assets	Compustat Bank	Loan Loss Provision over Loans	Loan loss provision divided by total gross loans	Compustat Bank
Liquidity Ratio Asset	Liquidity assets divided by total book assets	Compustat Bank	Interest-bearing Deposit Ratio	Interest-bearing deposits divided by total book assets	Compustat Bank
		Control Variables			
Lending Relationship Dummy	A dummy takes one if a firm and a bank have a loan within twenty quarters before the loan origination quarter	DealScan	Bank-firm Distance	The geographic distance between a firm's headquarter and a bank's headquarter in kilometres.	Compustat North America and CompustatCompustat Bank
Top Industry Dummy	A dummy takes one if a firm's industry falls into its bank's top three industries according to the number of the bank's borrowers in a given quarter	Compustat North America	Altman's Z-score	The sum of 1.2*working capital, 1.4*retained earnings, 3.3*pretax income, and 0.999*total sales over total book assets	Compustat North America
Profitability	Operating income over the average total assets between this quarter's and last quarter's numbers	Compustat North America	Asset Tangibility	Fixed assets (sum of property, plant and equipment) over total book assets	Compustat North America
Cash Holdings	Cash and short-term investment over total book assets	Compustat North America	Financial Leverage	Short-term liabilities plus long-term liabilities divided by total book assets	Compustat North America
Tobin's Q	Quasi-market assets (sum of market capitalization and book liabilities) over total book assets	Compustat North America	Year since IPO	Number of year to date of a company's initial public stock offering	Compustat North America
loan Type	A discrete variable representing the type of the loan with 65 categories, such as term loan and bridge loan	DealScan	Loan Purpose	A discrete variable representing the primary purpose of the loan with 42 categories, such as LBO and working capital	DealScan

Figure A.1: Annual Issues and Issuance of the U.S. Public Bonds



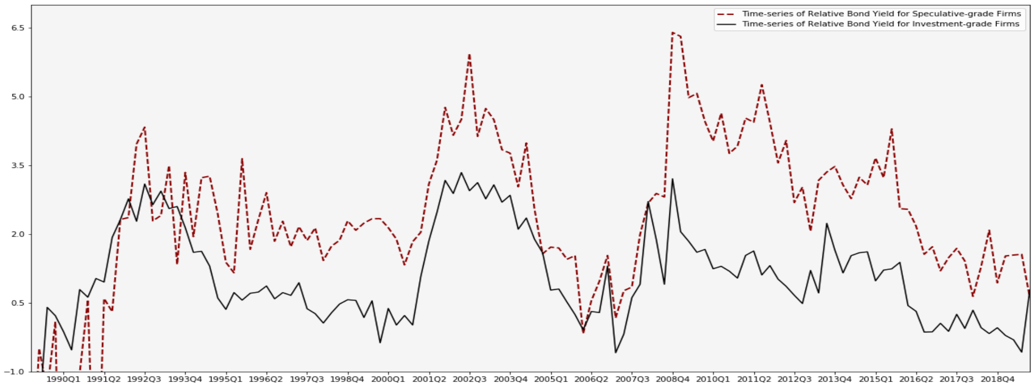
This Figure plots two annual time series in the U.S. public bond market. All bonds issued by financial firms are excluded. The data is sourced from FISD. The solid black line represents the total number of bond issues in each year. The red dotted line plots the total amount of bond issuance in each year.

Figure A.2: Moody's Seasoned Corporate Bond Yields



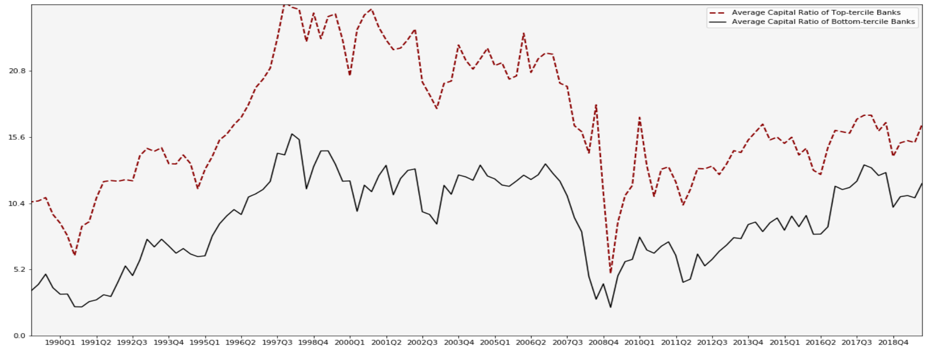
This Figure plots two quarterly time-series of Moody's Seasoned Corporate Bond Yields. The data is sourced from the Federal Reserve Economic Data of St. Louis Fed. The dashed red line is the Bbb bond yields, and the solid black line is the Aaa bond yields.

Figure A.3: Time-series of Bond Yields Relative to Loan Yields



This Figure plots two quarterly time series of bond yields relative to loan yields. In each quarter, the bond yield is an average across all public bonds. The bond information comes from FISD, excluding nonconvertible bonds and all bonds to financial companies. The loan yield is an average of the sum of the loan spread and the 12-month LIBOR rate of the quarter. The loan information comes from Dealscan, excluding all loans to financial companies. Each point in the figure is the difference between the average bond yield and the average loan yield. The dashed red line is the yield difference calculated from all bonds and loans to speculative-grade firms (i.e., a firm with an S&P rating lower than BBB-). The solid black line is the yield difference calculated from all bonds and loans to investment-grade firms (i.e., a firm with an S&P rating equal to or higher than BBB-).

Figure A.4: Time-series of Capital Ratios of Well-capitalised Banks and Poorly-capitalised Banks



This Figure plots two quarterly time-series of average capital ratios. We divide the sample banks into terciles in each quarter based on bank capitalisation. Bank capitalisation is a bank's market equity value divided by the sum of market equity and book liabilities. The dashed red line is the capital ratio of the banks in the top tercile, and the solid black line is the ratio of those in the bottom tercile.

Table A.2

Estimates of the Semiparametric Model in the Pre-COVID and the Pre-GFC Periods

	1	2
Bank Capitalisation Unrated Dummy	-34.96	54.25**
	[-48.53, 17.97]	[30.69, 68.12]
Bank Size Firm Size	49.19**	11.92**
	[22.25, 68.26]	[5.67, 34.58]
Lending Relationship Dummy	1000	1000
	[-]	[-]
Top Industry Dummy	400.56**	224.68**
	[285.48, 452.44]	[180.83, 393.32]
Bank-firm Distance	-0.0183	-0.0474**
	[-0.0362, 0.0228]	[-0.0901, -0.0226]
Sample	2015Q1-2019Q4	2002Q3-2007Q2
Number of Inequalities	128269	488759
Fraction of Inequalities Satisfied	0.95	0.930

Notes: This table reports estimates of the semiparametric model. There are two sample periods – 2015Q1-2019Q4 and 2002Q3-2007Q2. The sample includes all possible matches between banks and firms recorded in Dealscan in a given quarter. In the semiparametric model, we estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. All borrower and bank-specific characteristics are demeaned each quarter. Number of Inequalities is the number of total inequalities. Fraction of Inequalities Satisfied is the fraction of inequalities satisfied the pairwise stability condition under the model prediction. 95% confidence intervals are constructed by subsampling and are reported in square brackets. ** indicates that the confidence interval does not contain zero. Bank capitalisation is a bank's market equity value divided by the sum of market equity and book liabilities. Unrated Dummy takes one if a firm does not have an S&P long-term credit rating. Bank Size is a bank's total assets measured in logarithms. Firm Size is a firm's total assets measured in logarithms. For all interactions, we do not report the coefficients of the two interacting variables. There are three other variables constructed at the bank-firm level. Lending Relationship Dummy is a binary variable taking one if a firm and a bank have a past contract within 20 quarters prior to the loan origination quarter. Bank-firm Distance is the physical distance between a firm's headquarter and a bank's headquarter in thousand kilometres. Top Industry Dummy is a dummy taking one if a firm's industry falls in the top three industries of a bank's borrowers in a given quarter. Quarter-, bank-, and firm-fixed effects are included in OLS regressions. The other (unreported) controls include z-score, asset tangibility, profitability, cash, financial leverage, Tobin's Q, and years since IPO. See Table A.1 for further information on variable definitions. All OLS coefficients are multiplied by 100. T-statistics based on standard errors clustered by banks are reported in brackets. a, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively. In the semiparametric model, we estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. All borrower and bank-specific characteristics are demeaned each quarter. Number of Inequalities is the number of total inequalities. Fraction of Inequalities Satisfied is the fraction of inequalities satisfied by the pairwise stability condition under the model prediction. 95% confidence intervals are constructed by subsampling and are reported in square brackets. ** indicates that the confidence interval does not contain zero.

Table A.3

Loan Access under Counterfactuals - Loan Amount is Equally Split to Each Syndicate Lender

	Panel A Loan Access in COVID		***Panel B Loan Access in GFC***	
	Benchmark (%)	Benchmark − Counterfactual (%)	Benchmark (%)	Benchmark − Counterfactual (%)
Observed Matches	-32.77		-57.66	
	[-]		[-]	
Estimates from the Pre-GFC Model		-3.11		
		[-7.46, 0.87]		
Absolute $\hat{\beta}_1 = \lvert\hat{\beta}_1\rvert$		-4.41		
		[-7.28, 1.74]		
Estimates from the Pre-COVID Model				4.60**
				[1.02, 5.97]
Shut-off $\hat{\beta}_1 = 0$				3.08**
				[1.24, 4.67]

Notes: This table presents unrated firms' loan access during the COVID and the GFC. For the credit access, we proxy it using the average loan growth of all unrated firms' (most recent) pre-crisis relationship banks under observed matches (the benchmark) and counterfactuals. The counterfactual relationship banks are assigned by using the model estimates or altering the estimates. We estimate the parameters by maximising the number of inequalities that satisfy the pairwise stability condition. The inequalities are constructed from all combinations of two actual matchings in a given quarter, and we include all sample quarters. The estimated sample is 20 quarters prior to the COVID-19 shock (GFC) for Panel A (Panel B). Each bank's lending growth is the growth rate of their annualised investment amount of syndicated loans from the pre-crisis period to the crisis period. The investment amount of a loan is the total amount equally split across all lenders. For the COVID (GFC), the pre-crisis and crisis period are 2017Q1-2019Q4 (2004Q4-2007Q2) and 2020Q2-2020Q4 (2008Q4-2009Q2). In the benchmark scenario, an unrated firm's relationship bank is the one in its actual loan contract in the most recent loan facility. In the counterfactuals, each unrated firm's relationship bank is identified in three steps: adjusting the estimated parameters, using them to calculate the matching value, and then matching each firm with a bank that maximises the matching value. In Panel A, we make unrated firms' relationship banks to be well-capitalised. To do so, we let $\hat{\beta}_1$, the coefficient of the interaction between bank capitalisation and borrowers' unrated status, to be positive in two ways. First, we use the estimates from the pre-GFC sample. Second, we take an absolute of $\hat{\beta}_1$, while keeping all other parameter estimates unchanged. Similarly, in Panel B, we use the pre-COVID estimates and take $\hat{\beta}_1$ to be zero. 95% confidence intervals (reported in square brackets) are obtained by estimating 50 sets of parameter estimates based on subsampling, then drawing 20 times from the counterfactual matches for each set of estimates. ** indicates that the confidence interval does not contain zero.

Appendix B

Chapter 2 Appendix

B.1 Additional Information

B.1.1 Variable Definition

Table B.1 contains the definitions, references, and sources of key variables used in this study.

Insert Table B.1 here.

B.1.2 The Plots of the Proportion of Loans with Various Lender Roles

Figure B.1 plots the time variation of the proportion of loans with four lender roles. Figure B.2 plots the time variation of the proportion of loans with "Admin agent" or "Arranger".

Insert Figure B.1 here.

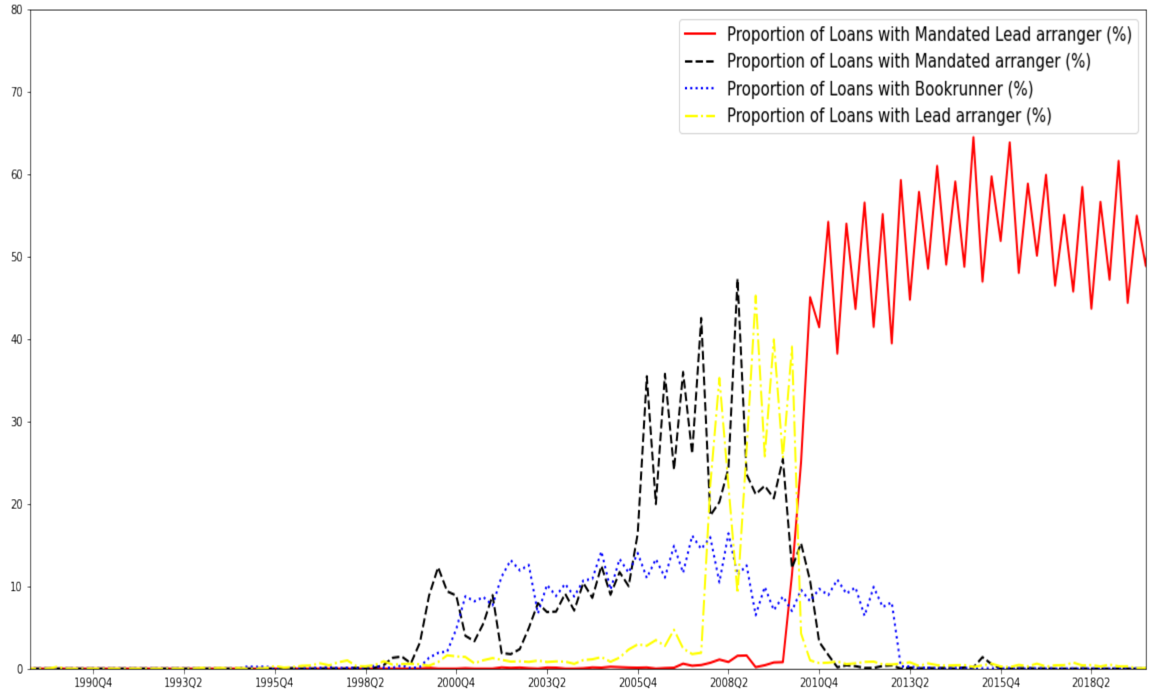
Insert Figure B.2 here.

Table B.1
Variable Definitions

Name	Measure	Source	Name	Measure	Source
	Dependent Variable				
Loan Spreads	All-in-drawn spreads, averaging across all facilities	Dealscan			
Covenant Violations	The number of violation times of all covenants in a loan package; a violation happens when the underlying variable of a covenant breaches the threshold in a given quarter-end.	Dealscan	Violation Severity	The relative difference between the threshold and the covenant variable in a given quarter-end conditional on a covenant violation, averaging across all covenants and violations.	Dealscan
	Main Independent Variables				
Co-lead Arrangement	A dummy taking one if the loan package has more than one lead arranger	Dealscan	Lowest Future Rating	A borrower's lowest S&P credit rating within five years after the loan origination date	Dealscan; Capital IQ
Log(No. Past Relationship Lenders)	The number of lead arrangers (in logarithms) that have ever lent to the borrower within five years before the loan origination	Dealscan	Log(Lead Arranger Size)	The average total assets (in logarithms) across all lead arrangers	Compustat
Lead Arranger Market Share	The number of loans taken by the lender divided by the number of all loans in a given quarter, averaging across all lead arrangers	DealScan	Log(No. of Lead Arrangers)	The number of lead arrangers (in logarithms) in a loan package	DealScan
	Control Variables				
Log(Loan Amount)	Total amount (in logarithms) of a loan package	DealScan	Log(Loan Maturity)	The longest facility maturity (in logarithms) in a loan package	DealScan
Revolving Facility Dummy	A dummy taking one if a loan package includes a revolving facility	DealScan	Secured Dummy	A dummy taking one if a loan package includes a secured facility	DealScan

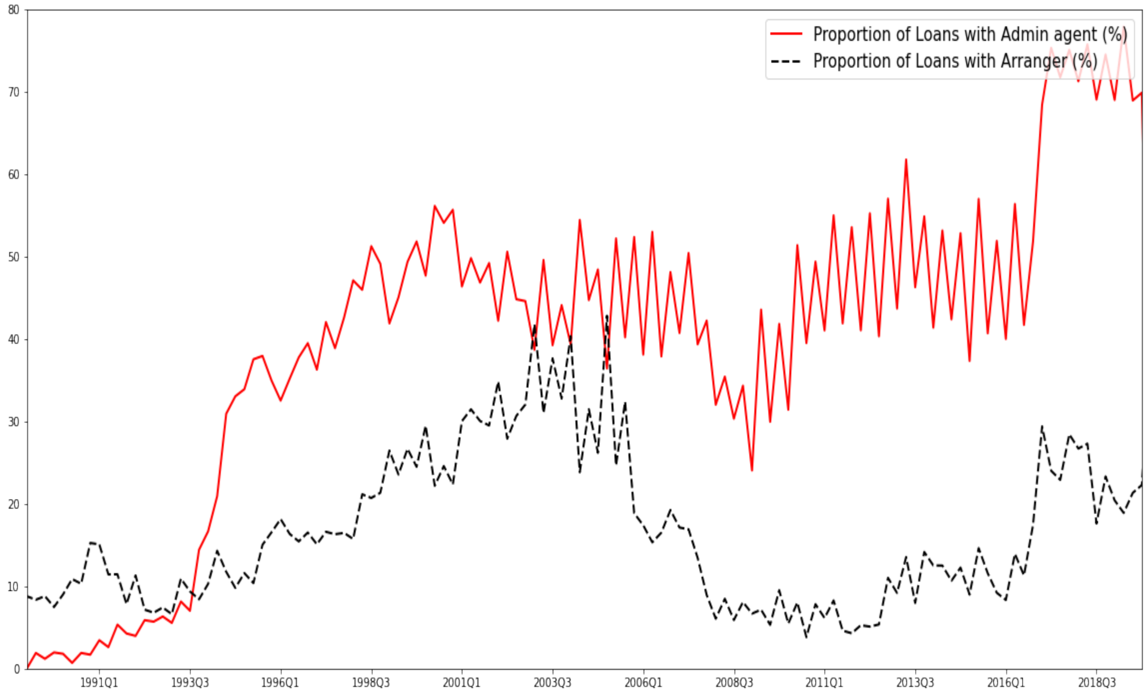
Name	Measure	Source	Name	Measure	Source
Log(No. of Covenant)	The number of covenants (in logarithms) in a loan package	DealScan	Log(No. of Co-agents)	The number of co-agents (in logarithms) in a loan package	DealScan
Log(No. of Lenders)	The number of lenders (in logarithms) in a loan package	DealScan	Lender-borrower Distance	The geographic distance between a firm's headquarter and a bank's headquarter in kilometres.	Compustat
Lending Relationship Intensity	The number of lead arrangers (in logarithms) that have ever lent to the borrower within five years before the loan origination	DealScan	Current Credit Rating	The borrower's S&P credit rating in the month of loan origination	Capital IQ
Altman's Z-score	The sum of 1.2*working capital, 1.4*retained earnings, 3.3*pre-tax income, and 0.999*total sales over total book assets	Compustat	Log(Analyst Coverage)	The number of analyst estimates	IBES
Profitability	Operating income over the average total assets between this quarter's and last quarter's numbers	Compustat	Asset Tangibility	Fixed assets (sum of property, plant and equipment) over total book assets	Compustat
Firm Size	Total assets in logarithms	Compustat	Financial Leverage	Short-term liabilities plus long-term liabilities divided by total book assets	Compustat
Tobin's Q	Quasi-market assets (sum of market capitalization and book liabilities) over total book assets	Compustat	Lead Arranger Capital Ratio	Equity over total book assets, averaging across all lead arrangers	Compustat
Lead Arranger Exposure	The total loan amount over a lender's assets, averaging across all lead arrangers	Compustat	Loan Purpose	A discrete variable representing the primary purpose of the loan with 42 categories, such as LBO and working capital	DealScan
Unrated Dummy	A dummy taking one if the borrower does not have an S&P credit rating in the month of loan origination	Capital IQ	Covenant Strictness	Ex-ante strictness of covenants of a loan package	Demerjian and Owens (2016)

Figure B.1: The Trend of the Loans Containing Several Lender Roles



This figure plots the change in the proportion of loans with several lender roles as the lead arranger in each quarter.

Figure B.2: The Trend of the Loans Containing “Admin agent” and ”Arranger”



This figure plots the change in the proportion of loans with several lender roles as the lead arranger in each quarter.

Appendix C

Chapter 3 Appendix

C.1 Construction of the dataset: Additional details

C.1.1 Variable definition

Table A1 contains the definitions, references, and sources of key variables used in this study.

Insert Table A1 here.

C.1.2 The tightening events of capital regulation in the 11 countries

Table A2 presents the 74 tightening events of capital regulation in the 11 countries. The information is sourced from the IMF dataset iMaPP.

Insert Table A2 here.

C.1.3 The implementation of Basel II in the 11 countries

Table A3 presents the information on Basel II implementation in the 11 countries. The information is sourced from the IMF dataset iMaPP and the 2011 World Bank Bank Regulation and Supervision Survey.

Insert Table A3 here.

C.1.4 The distribution of the sample

Table A4 presents the distribution, by borrower countries, of the (treated) loan sample used for the analysis of the U.S. stress test implementation, dropping missing observations for all variables in Equation 1. It contains the number of treated loans, treated lead arrangers, and borrowers to which the treated lead arrangers lend. Table A5 presents the distribution, by borrower countries, of the (treated) loan sample used for the cross-country analysis of capital regulation tightening, dropping missing observations for all variables in Equation 3. It contains the number of treated loans, treated lead arrangers, borrowers to which the treated lead arrangers lend, and the tightening source countries.

Insert Table A4 here.

Insert Table A5 here.

C.2 A Simple Model for the Spillover Effect of Capital Regulation Tightening

C.2.1 The model setting

The borrower and the project: the borrower has a project with investment initialised at 1. The investment is fully financed by a syndicated loan. There are two outcomes. The project generates a return of X with a probability of θ and zero with $1-\theta$.

The syndicated cross-border loan: the syndicated loan has a lead arranger investing α and participants investing $1-\alpha$. The lead arranger monitors the borrower while the participants only provide funds. The lead arranger can choose the monitoring effort θ which leads to a one-to-one change in the probability of success but induces a cost of $\frac{1}{2}\beta\theta^2$, where β can be thought as the difficulty of monitoring (e.g., the borrower's opaqueness). If the project succeeds, the borrower repays R_L; if it fails, the borrower repays zero. Assume a zero risk-free rate; thus, R_L represents the loan spread. The loan contract also includes a fee, F, which is given to the lead arranger and deducted at the beginning.

The lead arranger: the lead arranger maximises profits by choosing the lead arranger share and monitoring efforts for a given loan spread. k of total assets is funded by the lead arranger's own capital, and the rest is funded by deposits, with the required repayment as R_D. benefit of investing. As mentioned in Section 4.2.2, engaging in cross-border lending offers the advantage of regulatory arbitrage. As a bank increases its investment in a cross-border loan, it gains additional exposures that can be leveraged for regulatory arbitrage purposes. Thus, the benefit of regulatory arbitrage is denoted as A, and it is proportional to α. The bank also faces a loss of diversification, denoted C, that is proportional to α.

The participants and depositors of the lead arranger: they require break-even.

Capital regulation tightening: consider stricter capital regulation introduced exogenously to the lead arranger's country. It affects the equilibrium in two aspects. First, it requires a higher minimum capital ratio, and thus, increases k. Note that even if the original capital ratio of the lead arranger is not binding, the inclination to maintain a capital buffer above the minimum requirement can drive the bank to lift k (see Fonseca and González, 2010; Shim, 2013, among others, for discussion on capital buffers). Second, stricter capital regulation amplifies the benefit of regulatory arbitrage and thus leads to a larger A.

C.2.2 The optimisation problem

The participants demand loan spreads R_L, given the lead arranger's monitoring efforts θ:

$$(1-\alpha)R_L\theta = 1-\alpha \Rightarrow \theta = \frac{1}{R_L} \tag{B1}$$

The lead arranger exerts monitoring efforts to maximise its profits:

$$\max_{\theta} \theta\left(\alpha R_L - (1-k)\alpha R_D\right) - \frac{1}{2}\beta\theta^2$$

Solving the first-order condition, we have:

$$\alpha R_L - (1-k)\alpha R_D = \beta\hat{\theta}$$

The first-order condition requires the marginal benefit of monitoring – the left side to be equal to the marginal cost – the right side. The depositors require break-even:

$$(1-k)\alpha R_D \theta = (1-k)\alpha \Rightarrow \theta = \frac{1}{R_D} \tag{B2}$$

Using B1 and B2, we have:

$$R_L = \sqrt{\frac{\beta}{\alpha k}} \tag{B3}$$

$$\hat{\theta} = \sqrt{\frac{\alpha k}{\beta}} \tag{B4}$$

B3 indicates a negative relationship between loan spreads and lead arranger share. This plots the downward slopping curve in Figure 4.1. When capital regulation tightening increases k, the curve shifts downwards, as in the right panel in Figure 4.2.

Now, we solve the optimal choice of the lead arranger's investment α by maximising the bank's expected payoff:

$$\max_{\alpha} F + \theta\left(\alpha R_L - (1-k)\alpha R_D\right) - \frac{1}{2}\beta\theta^2 - \alpha C + \alpha A(k) - \alpha$$
$$\text{s.t. } \theta\left(X - R_L\right) - F \geq 0$$

The maximisation problem is subject to a borrower's participation constraint that balances the loan spread and the fee, and it is binding in the optimal condition. The maximisation indicates a positive relationship between the loan spread and the lead arranger share. With a higher loan spread, the marginal benefit increases. Thus, the lead arranger increases α.

Solving the first-order condition:

$$\frac{d}{d\hat{\alpha}}\left(\theta\left(X-R_L\right)+\theta\left(\hat{\alpha}R_L-(1-k)\hat{\alpha}R_D\right)-\frac{1}{2}\beta\theta^2-\hat{\alpha}C+\hat{\alpha}A-\hat{\alpha}\right)=0$$

Using B1 and B2, we have:

$$\frac{d\theta}{\hat{\alpha}}-\frac{d\theta}{\hat{\alpha}}\beta\theta+(A+k)-C-1=0 \tag{B5}$$

According to B4:

$$\frac{d\hat{\theta}}{\alpha}=\frac{1}{2}\sqrt{\frac{k}{\alpha\beta}}$$

Replacing it in B5:

$$\hat{\alpha}=\frac{kX^2}{\beta\left(2C+2-2A-k\right)^2}$$

Therefore, as A increases after capital regulation tightening, α becomes larger, shifting the lead arranger's curve outwards, as shown in the left panel of Figure 2.

C.3 Robustness Checks and Additional Tests

Table C1 reports the results of robustness checks and additional tests.

Insert Table C1 here.

Table A1
Variable Definitions

Name	Measure	Source	Name	Measure	Source
		Dependent Variable			
Loan Spreads	All-in-drawn spreads of a loan facility	Dealscan	Lead Arranger Share	The lead arranger's allocation to a loan	Dealscan
No. Participants	The count of total participants in a loan	Dealscan			
		Main Independent Variables			
Treatment	A dummy taking one if the lead arranger of the loan participates in the 2009 SCAP	The Federal Reserve released information on CCAR	Post	A dummy taking one if the loan originates after 2009	Author own calculation
Later-included BHC	A dummy taking one if the lead arranger is new to the 2014 CCAR	The Federal Reserve released information on CCAR	Post-2014	A dummy taking one if the loan originates after 2014	Author own calculation
Advanced-approach BHC	A dummy taking one if the lead arranger is identified as an advanced-approach BHC in the 2014 CCAR	The Federal Reserve released information on CCAR	Period 2010-2014	A dummy taking one if the loan originates during 2010-2014	Author own calculation
Borrower Country Experiences	The number of loans that the lead arranger of a loan has ever lent to the country in the past five years divided by the total loans in the country	Author own calculation	Secured Loan	A dummy taking one if a loan is secured by collaterals	Dealscan
Unrated Borrower	A dummy taking one if the borrower does not have an S&P credit rating	Capital IQ	Foreign Subsidiary Dummy	A dummy taking one if the loan is made by one of the lead arranger's foreign subsidiaries	Dealscan
Pre-Stress Test Quarter Dummy	A dummy taking one if the loan is made within the last quarter before the submission of the year's stress test	The Federal Reserve released information on CCAR	Treatment^{c}	A dummy taking one if a loan is from tightening countries	Author own calculation
Post^{c}	A dummy taking one if a loan originates after the event date	Author own calculation	Post^{c-1}	a dummy taking one if a loan originates in the first year before the event quarter	Author own calculation
Post^{c1}	A dummy taking one if a loan originates in the first year after the event quarter	Author own calculation	Post^{c2}	A dummy taking one if a loan originates in the second year after the event quarter	Author own calculation
Borrower Country Tightening	A dummy taking one if the borrower's country of a loan has ever implemented stricter capital regulations during the event window	Author own calculation	Unconsolidated Basis Dummy	A dummy taking one if Basel II was implemented unconsolidated basis in the lead arranger's country of a loan	2011 World Bank Bank Regulation and Supervision Survey
Minimum Capital Distance	the minimum of the differences between stressed tier-one capital, total capital, leverage ratio and the minimum capital requirements	The Federal Reserve released information on CCAR			
		Control Variables			
Log(Maturity)	The logarithm of loan maturity	DealScan	Log(Amount)	The logarithm of loan amount, converted to the same currency	DealScan
Secured Indicator	A dummy takes one if the loan is secured by collaterals	Dealscan	Log(No. Lenders)	The logarithm of the count of total lenders in a loan	Dealscan
Log(No. Covenants)	The logarithm of the count of total covenants in a loan package	Dealscan	Relationship Intensity	The number of loans that the firm and the lead arranger(s) of a loan have ever contracted within five years before the loan origination, divided by the number of total loans the firm has borrowed in the past five years	Author own calculation
Log(Bank Assets)	The logarithm of the lead arranger's total assets, converted to the same currency	FR Y-9C; Compustat	Bank Capital Ratio	The lead arranger's book equity divided by total assets	FR Y-9C; Compustat

Table A2

The Tightening Events of Capital Regulation in the 11 Countries

Event Quarter	Country	CCB_T	Conservation_T	Capital_Gen_T	Capital_Corp_T	Capital_FX_T	LVR_T	Basel_T
2007Q1	Japan	0	0	0	0	0	0	0
2008Q1	Switzerland	0	0	0	0	0	0	0
2008Q2	Spain	0	0	0	0	0	0	0
2011Q1	Switzerland	0	0	0	0	0	0	1
2011Q3	Switzerland	0	0	0	0	0	0	0
2011Q4	France	0	0	0	0	0	0	1
2011Q4	Spain	0	0	0	0	0	0	1
2012Q1	Australia	0	0	0	0	0	0	1
2012Q1	Canada	0	0	0	0	0	0	1
2012Q1	Germany	0	0	0	0	0	0	1
2012Q1	Netherlands	0	0	0	0	0	0	1
2012Q1	Singapore	0	0	0	0	0	0	1
2012Q1	United Kingdom	0	0	0	0	0	0	1
2012Q2	Japan	0	0	0	0	0	0	1
2012Q3	Switzerland	0	0	0	0	0	0	0
2013Q1	Australia	0	0	0	0	0	0	1
2013Q1	Canada	0	0	0	0	0	0	1
2013Q1	Singapore	0	0	0	0	0	0	1
2013Q1	Spain	0	0	0	0	0	0	1
2013Q1	Switzerland	0	0	0	0	0	0	1
2013Q1	United States	0	0	0	0	0	0	1
2013Q2	Japan	0	0	0	0	0	0	1
2013Q3	Switzerland	0	0	0	0	0	0	0
2014Q1	France	0	0	0	0	0	0	1
2014Q1	Germany	0	0	0	0	0	0	1
2014Q1	United Kingdom	0	0	0	0	0	0	1
2014Q1	United States	0	0	0	0	0	0	1
2014Q2	Switzerland	0	0	0	0	0	0	0
2014Q3	Netherlands	0	0	0	0	0	0	1
2014Q4	Canada	0	0	0	0	0	0	0
2015Q1	France	0	0	0	0	0	0	0
2015Q1	United States	0	0	0	0	0	0	0
2016Q1	Australia	0	0	0	0	0	0	0
2016Q1	Canada	0	0	0	0	0	0	0
2016Q1	France	0	0	0	0	0	0	0
2016Q1	Germany	0	0	0	0	0	0	0
2016Q1	Japan	0	0	0	0	0	0	0
2016Q1	Netherlands	0	0	0	0	0	0	0
2016Q1	Singapore	0	0	0	0	0	0	0
2016Q1	Spain	0	0	0	0	0	0	0
2016Q1	United Kingdom	0	0	0	0	0	0	0
2016Q3	Switzerland	0	0	0	0	0	0	0
2016Q4	Switzerland	0	0	0	0	0	0	0
2017Q1	Canada	0	0	0	0	0	0	0
2017Q1	France	1	1	1	1	1	1	0
2017Q1	Germany	0	0	0	0	0	0	0
2017Q1	Japan	0	0	0	0	0	0	0
2017Q1	Netherlands	0	0	0	0	0	0	0
2017Q1	Singapore	0	0	0	0	0	0	0
2017Q1	Spain	0	0	0	0	0	0	0
2017Q1	United Kingdom	0	0	0	0	0	0	0
2017Q3	France	1	1	1	1	1	1	0
2018Q1	Canada	1	1	1	1	1	1	0
2018Q1	France	0	0	0	0	0	0	0
2018Q1	Germany	0	0	0	0	0	0	0
2018Q1	Japan	0	0	0	0	0	0	0
2018Q1	Netherlands	0	0	0	0	0	0	0
2018Q1	Singapore	0	0	0	0	0	0	0
2018Q1	Spain	0	0	0	0	0	0	0
2018Q1	Switzerland	0	0	0	0	0	0	0

Table A3

The Implementation of Basel II in the 11 Countries

Implementation Quarter	Country	Unconsolidated Basis Dummy
2007Q1	Switzerland	1
2007Q1	Germany	1
2007Q1	France	0
2007Q1	Netherland	0
2007Q2	Japan	0
2007Q4	Canada	0
2008Q1	Australia	0
2008Q1	Spain	0
2008Q1	United Kingdom	1
2008Q1	Singapore	0
2008Q2	United States	1

Notes: This table reports the information on Basel II implementation in 11 countries. Unconsolidated Basis Dummy takes one if Basel II was implemented on the unconsolidated basis in the lead arranger's country of a loan.

Table A4

The Distribution of the (Treated) Loan Sample Used for the Analysis of the U.S. Stress Test Implementation

Borrower Country	Number of Treated Loans	Number of Treated Lenders	Number of Borrowers	Borrower Country	Number of Treated Loans	Number of Treated Lenders	Number of Borrowers
ARE	7	2	2	JEY	19	3	4
ARG	1	1	1	JPN	6	1	1
AUS	18	5	6	KAZ	2	1	1
AUT	1	1	1	KOR	2	1	1
BEL	5	3	3	LBR	6	2	2
BHS	3	2	2	LUX	26	4	8
BMU	52	5	15	MEX	5	3	3
BRA	18	3	6	MHL	7	3	6
CAN	154	8	41	NLD	46	5	19
CHE	21	3	6	NOR	2	2	2
CHL	3	3	3	OMN	1	1	1
CYM	13	4	8	PHL	1	1	1
DEU	114	6	27	POL	3	0	2
DNK	6	2	3	PRT	3	2	1
ESP	77	4	19	QAT	3	2	1
FIN	1	1	1	RUS	47	5	11
FRA	83	8	25	SAU	9	2	1
GBR	127	7	45	SGP	6	2	2
GRC	2	1	1	SWE	21	5	8
HKG	1	1	1	THA	1	1	1
HUN	2	1	1	TUR	14	3	4
IND	5	1	1	TWN	4	2	3
IRL	52	3	17	VGB	6	2	4
ISR	7	3	5	ZAF	5	2	2
ITA	11	2	5				

Notes: this table presents the distribution, by borrower countries, of the (treated) loan sample used for the analysis of the U.S. stress test implementation, dropping missing observations for all variables. It contains the number of treated loans, treated lead arrangers, and borrowers to which the treated lead arrangers lend.

Table A5

The Distribution of the (Treated) Loan Sample Used for the Cross-country Analysis of Capital Regulation Tightening

Borrower Country	Number of Treated Loans	Number of Treated Lenders	Number of Borrowers	Number of Tightening Countries	Borrower Country	Number of Treated Loans	Number of Treated Lenders	Number of Borrowers	Number of Tightening Countries
ARE	6	3	2	3	JEY	19	4	4	4
AUS	20	5	10	4	JPN	6	1	1	1
AUT	1	1	1	1	KAZ	1	1	1	1
BEL	7	3	3	3	KOR	2	1	1	1
BMU	79	10	21	6	LBR	12	6	2	5
BRA	23	7	4	5	LUX	10	2	3	2
CAN	134	9	40	5	MEX	4	3	4	2
CHE	11	3	4	2	MHL	9	3	4	3
CHL	4	3	2	2	MYS	11	2	1	1
CHN	1	1	1	1	NGA	2	2	2	2
COL	1	1	1	1	NLD	51	9	18	6
CUW	1	1	1	1	NOR	6	2	2	2
CYM	27	6	12	4	OMN	1	1	1	1
DEU	46	6	10	4	POL	6	2	3	1
DNK	7	2	3	2	PRT	3	2	2	2
ESP	53	6	12	5	QAT	2	1	1	1
FIN	1	1	1	1	RUS	16	4	5	3
FRA	35	7	19	5	SAU	1	1	1	1
GBR	84	9	32	5	SGP	2	1	1	1
GRC	1	1	1	1	SWE	22	4	5	4
HKG	4	3	4	3	THA	2	1	1	1
HUN	1	1	1	1	TUR	4	1	2	1
IDN	6	4	4	4	TWN	39	4	15	4
IND	1	1	1	1	USA	705	14	258	5
IRL	82	9	16	4	VGB	7	1	1	1
ISR	8	2	5	1	ZAF	4	2	2	2
ITA	14	5	6	4					

Notes: this table presents the distribution, by borrower countries, of the (treated) loan sample used for the cross-country analysis of capital regulation tightening, dropping missing observations for all variables. It contains the number of treated loans, treated lead arrangers, borrowers to which the treated lead arrangers lend, and the tightening source countries.

Table C1
Robustness Checks and Additional Tests

	1	2	3	4	5
	Drop Loan Contractual Terms	Using Dealscan Country Information	Alternative definition for country experiences	Quarter FEs	Real estate Placebo
	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads	Loan Spreads
Treatment×Post	-56.46*	-114.5***	-69.66**	86.97**	
	(-1.70)	(-7.47)	(-2.47)	(1.99)	
Treatment×Post×Borrower Country Experiences^{alter}				-228.8***	
				(-3.83)	
Post					7.847**
					(2.06)
Treatment×Post					3.233
					(0.37)
Sample Period	2004-2014	2004-2014	2004-2014	2004-2014	2007Q1-019Q3
FEs	Yes	Yes	Yes	Yes	Yes
Loan-level Controls		Yes	Yes	Yes	Yes
Lender-level Controls	Yes	Yes	Yes	Yes	Yes
Borrower-level Controls	Yes		Yes	Yes	Yes
Observations	2226	6544	2212	2212	5598
Adjusted R^2	0.629	0.58	0.593	0.536	0.5

Notes: This table reports results from two difference-in-difference regression models. In the first four columns, the results are estimated from a standard difference-in-difference regression model. The loan-level sample includes cross-border loans made by U.S. BHCs and originating from 2004 to 2014. The dependent variable is loan spreads, which are the all-in-drawn spreads. Treatment is a dummy taking one if the lead arranger of the loan participates in the 2009 SCAP. Post is a dummy taking one if the loan originates after 2009. Borrower Country Experiences measures the lead arranger's past lending experiences to the borrower's country. Both regressions include loan type FEs, loan purpose FEs, bank FEs, borrower country FEs, borrower SIC2 FEs, and year FEs.

In the last column, the result is estimated from a stacked difference-in-difference regression. The sample is constructed based on capital tightening events, and the tightening events are specific to the household section in the last column. Each event of capital regulation tightening is a cohort. The event window is four and eight quarters before and after the event quarter in the fifth and sixth columns, respectively. For each event, the cross-border loans from the tightening country and non-tightening countries eight quarters before and after the event date are included in the sample. Then, the sample excludes the destination countries that borrow only from the non-tightening source countries. The treatment group include the loans arranged by lead arrangers from the tightening source country. Correspondingly, the loans arranged by lenders from other source countries consist of the control group. For each country in the original control group that tightens capital regulation, we remove all loans originating before (after) the tightening date if the tightening happens before (after) the event quarter. The dependent variable is loan spreads, which are the all-in-drawn spreads. Treatment^c is a dummy taking one if a loan is from tightening countries. Post^c is a dummy taking one if a loan originates after the event date. Post^{c-1} is a dummy taking one if a loan originates in the first year before the event quarter. All regressions include stricter FES: loan type-cohort FEs, loan purpose-cohort FEs, lender country-cohort and borrower country-cohort FE.

In all regressions, the control variables include the loan maturity, loan amount, an indicator for secured loans, number of lenders, number of covenants, relationship intensity, lender size, lender's capital adequacy, lender ROA, lender liquidity ratio, borrower size, borrower asset tangibility, borrower cash and equivalents, borrower ROA, and borrower interest coverage ratio, and they are defined in Table A1. T-statistics, adjusted for heteroskedasticity, are reported in brackets. +, *, **, and *** indicate that the corresponding p-values are less than 0.11, 0.1, 0.05, and 0.01, respectively.